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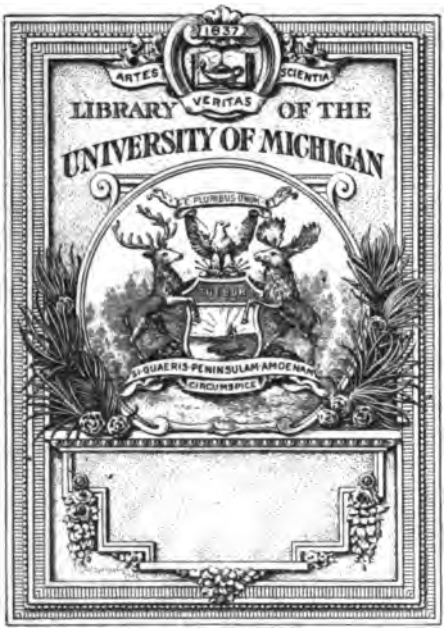
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OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

TWENTY-NINTH MEETING,

HELD AT

Boston, Mass.,

AUGUST, 1880.

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C. V. RILEY, of Washington, D. C.

SECRETARY OF SECTION A.
JOHN TROWBRIDGE, of Cambridge, Mass.

SECRETARY OF SECTION B.
WILLIAM SAUNDERS, of London, Ontario.

SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.
H. W. WILEY, of Lafayette, Ind.

SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.
S. P. SHARPLES, of Boston, Mass.

SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.
J. G. HENDERSON, of Winchester, Ill.

SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.
B. PICKMAN MANN, of Cambridge, Mass.

TREASURER.
WILLIAM S. VAUX, of Philadelphia, Pa.

STANDING COMMITTEE.

Past Presidents. — JAMES D. DANA, of New Haven; JAMES HALL, of Albany; STEPHEN ALEXANDER, of Princeton; ISAAC LEA, of Philadelphia; F. A. P. BARNARD, of New York; J. S. NEWBERRY, of New York; B. A. GOULD, of Boston; T. STERRY HUNT, of Montreal; ASA GRAY, of Cambridge; J. LAWRENCE SMITH, of Louisville; JOSEPH LOVERING, of Cambridge; JOHN L. LECONTE, of Philadelphia; J. E. HILGARD, of Washington; WILLIAM B. ROGERS, of Boston; SIMON NEWCOMB, of Washington; O. C. MARSH, of New Haven; GEORGE F. BARKER, of Philadelphia.

The President, Vice Presidents, Permanent and General Secretaries, the Secretaries of Sections A and B, and Treasurer of the Meeting.

Officers of the Preceding Meeting. — LEWIS H. MORGAN, of Rochester; ASAPH HALL, of Washington; ALEXANDER AGASSIZ, of Cambridge; JOHN K. REES, of St. Louis; HENRY B. NASON, of Troy.

From the Association at large. — [Six Fellows to be elected on the first day of the meeting]

LOCAL COMMITTEE

FOR THE

CINCINNATI MEETING.

EXECUTIVE COMMITTEE.

A. T. GOSHORN, *Chairman;* F. W. CLARKE, ORMOND STONE, *Secretaries;* JULIUS DEXTER, *Treasurer;* J. D. COX, WILLIAM MCALPIN, HERBERT JENNEY, GEORGE W. JONES, ARCHER BROWN, C. W. WENDTE, ROBERT BROWN, JR.

SPECIAL COMMITTEES.

Committee on Reception. — J. D. Cox, *Chairman;* Miss E. O. Abbot, Larz Anderson, John Bachman, G. K. Bartholomew, J. D. Brannan, Robert Brown, Jr., Mrs. Robert Brown, Jr., Gustav Bruehl, Dr. J. D. Buck, Pitts H. Burt, Dr. Julia Carpenter, V. T. Chambers, J. B. Chickering, H. F. Clark, Robert Clarke, Dr. Wm. Clendenin, Dr. C. G. Comegys, Dr. P. S. Conner, E. W. Coy, J. M. Crawford, C. H. Daniels, Dr. John Davis, William Henry Davis, Dr. W. W. Dawson, Mrs. William Dodd, S. W. Duncan, Fred. Eckstein, Jr., H. T. Eddy, James Eels, Sumner Ellis, Jacob Elsas, T. J. Emery, Mrs. T. J. Emery, L. J. Evans, Seth Evans, M. Fechheimer, Francis Ferry, Miss Fannie Field, Dr. C. D. Fishburn, Frank S. Fitch, Mrs. Richard Folsom, M. F. Force, Mrs. M. F. Force, Dr. F. Forchheimer, Julius Freiberg, W. H. French, Alex. Fries, Mrs. G. R. Fries, H. B. Furness, Florien Gianque, John Goddard, Miss Emma Goepper, George S Gray, Telford Groesbeck, W. S. Groesbeck, J. W. Hall, G. W. Harper, L. B. Harrison, J. M. Hart, Dr. A. E. Heighway, A. E. Heighway, Jr., J. W. Herron, J. S. Highlands, A. H. Hinkle, Russell Hinman, J. M. Hodge, Dr. A. J. Howe, Z. M. Humphrey, Mrs. Z. M. Humphrey, Dr. Geo. Hunert, Dr. J. H. Hunt, E. W. Hyde, Dr. F. Kebler, John Kebler, Mrs. John Kebler, A. T. Keckeler, Dr. A. C. Kemper, Rufus King, Thomas Kite, Miss Annie Laws, E. D. Ledyard, T. D. Lincoln, Mrs. T. D. Lincoln, H. P. Lloyd, Joseph Longworth, N. Longworth, G. W. Lasher, A. H. McGuffey, William McMaster, P. Mallon, William Means, C. W. Merrill, W. E. Merrill, John Mickleborough, H. Thane Miller, S. A. Miller, Ottmar v. Mohl, Paul Mohr, Dr. J. A. Murphy, Dr. W. H. Mussey, N. Newburgh, J. M. Newton, Mrs. M. L. Nichols, Dr. O. D. Norton, R. H. Parkinson, E. H. Pendleton, Aaron F.

Perry, Mrs. A. F. Perry, John T. Perry, H. W. Pohlmeyer, Henry Probasco, H. A. Ratterman, Edward Rawson, D. W. Rhodes, H. B. Ridgaway, C. D. Robertson, Dr. F. Roelker, Mrs. R. S. Rust, Dr. Robert Sattler, W. W. Scarborough, J. G. Schaal, Lewis Seasongood, Dr. W. W. Seely, Murray Shipley, David Sinton, T. H. Skinner, Fayette Smith, R. R. Springer, J. B. Stallo, C. A. Stuntz, Alphonso Taft, Dr. Jonathan Taft, Charles Turcke, W. H. Venable, William Vieth, J. M. Walden, E. S. Wayne, J. W. Wayne, C. W. Wendte, C. W. West, Mrs. H. C. Whitman, Dr. J. T. Whittaker, Mrs. A. E. Wilde, I. W. Wiley, Dr. E. Williams, Davies Wilson, Mrs. Davies Wilson, M. F. Wilson, I. M. Wise, J. S. Zerbe.

Committee on Finance. — Julius Dexter, *Chairman;* Matthew Addy, B. Bettmann, B. F. Brannan, Richard Dymond, A. L. Frazer, G. P. Griffith, C. H. Kellogg, Joseph Kinsey, Max Lilienthal, Alfred Springer, Theodore Stanwood.

Committee on Rooms for Meeting. —William McAlpin, *Chairman;* Louis Ballauf, W. H. Bellows, E. R. Donohue, Charles Dury, R. H. Galbreath, Thomas Gilpin, F. W. Handy, L. M. Hosea, George Ward Nichols, Arthur Stem, A. G. Wetherby, W. C. Cochran.

Committee on Hotels and Lodgings. — Herbert Jenney, *Chairman;* Dr. S. C. Ayres, J. S. Conner, Alex. Hill, Thornton M. Hinkle, P. P. Lane, Nelson W. Perry, W. L. Robinson, W. H. Stewart.

Committee on Entertainments and Excursions. — George W. Jones, *Chairman;* A. L. Anderson, G. Bouscaren, R. M. Byrnes, A. B. Champion, Theodore Cook, Mrs. John Davis, Mrs. W. B. Davis, Mrs. William Henry Davis, Hermann Goepper, L. A. Harris, Mrs. A. J. Howe, M. E. Ingalls, G. B. Kerper, Mrs. John Kilgour, C. F. Low, P. F. Mohr, J. B. Peaslee, John Simpkinson, Mrs. Alphonso Taft, S. Lester Taylor, A. W. Whelpley, Mrs. Elkanah Williams, D. Gano Ray, Joseph Cox, S. F. Hunt.

Committee on Transportation. — A. T. Goshorn, temporary *Chairman;* A. D. Bullock, E. V. Cherry, J. M. Doherty, J. J. Henderson, A. Hickenlooper, J. M. Kinney, J. O. Shiras.

Committee on Press and Printing. — Archer Brown, *Chairman;* O. P. Austin, W. L. Dudley, John Hagemann, Joseph James, Dr. F. W. Langdon, Leopold Markbreit, M. L. Hawkins, V. R. Shepard, Percy Procter, C. P. Taft, R. B. Warder, Henry Haacke.

SPECIAL COMMITTEES.

1. *Permanent Committee on Weights, Measures and Coinage.*

F. A. P. BARNARD, of New York,
B. A. GOULD, of Boston,
J. E. HILGARD, of Washington,
H. A. NEWTON, of New Haven,
WILLIAM B. ROGERS, of Boston,
J. LAWRENCE SMITH, of Louisville,
E. B. ELLIOTT, of Washington,
JOHN TROWBRIDGE, of Cambridge,
A. M. MAYER, of Hoboken.

2. *Committee to Memorialize the Legislature of New York for a New Survey of Niagara Falls.*

F. A. P. BARNARD, of New York,
JAMES HALL, of Albany,
G. W. HOLLEY, of Niagara Falls,
J. T. GARDNER, of Albany.

3. *Committee on the Relation of Science to the Industrial Arts.*

R. H. THURSTON, of Hoboken,
J. M. SAFFORD, of Nashville,
J. T. WILDER, of Chattanooga,
O. CHANUTE, of New York,
J. W. OSBORNE, of Washington.

4. *Committee to obtain Permanent Funds for the Association, and to advise with Treasurer and Permanent Secretary on financial matters.*

WILLIAM B. ROGERS, of Boston,
J. LAWRENCE SMITH, of Louisville,
LEWIS H. MORGAN, of Rochester,
ALEXANDER AGASSIZ, of Cambridge,
ROBERT H. THURSTON, of Hoboken.

5. *Committee on Membership.*

J. S. NEWBERRY, of New York,
SIMON NEWCOMB, of Washington,
WILLIAM B. ROGERS, of Boston,
J. D. DANA, of New Haven,
J. LAWRENCE SMITH, of Louisville,
The GENERAL SECRETARY,
The PERMANENT SECRETARY.

6. *Committee on the best methods of Science Teaching in the Public Schools.*

E. L. YOUMANS, of New York,
A. R. GROTE, of New York,
J. S. NEWBERRY, of New York,
J. W. POWELL, of Washington,
N. S. SHALER, of Cambridge,
N. T. LUPTON, of Nashville.

7. *Committee on the Registration of Deaths, Births, and Marriages.*

E. B. ELLIOTT, of Washington,
J. B. KILLEBREW, of Nashville,
E. T. COX, of Oakland,
F. B. HOUGH, of Lowville,
J. S. COPES, of New Orleans,
JOHN COLLETT, of Indianapolis.

8. *Committee on Standard Time.*

ORMOND STONE, of Mt. Lookout,
S. P. LANGLEY, of Allegheny,
E. C. PICKERING, of Cambridge,
J. R. EASTMAN, of Washington,
LEWIS BOSS, of Albany,
LEONARD WALDO, of New Haven,
J. K. REES, of St. Louis,
G. W. HOUGH, of Lowville,
H. S. PRITCHETT, of Glasgow.

9. *Committee on Stellar Magnitudes.*

E. C. PICKERING, of Cambridge,
LEWIS BOSS, of Albany,
S. W. BURNHAM, of Washington,
ASAPH HALL, of Washington,
WILLIAM HARKNESS, of Washington,
E. S. HOLDEN, of Ann Arbor,
SIMON NEWCOMB, of Washington,
C. H. F. PETERS, of Clinton,
ORMOND STONE, of Mt. Lookout,
C. A. YOUNG, of Princeton.

10. *Committee to Audit the Accounts of the Permanent Secretary and Treasurer.*

HENRY WHEATLAND, of Salem,
THOMAS MEEHAN, of Philadelphia.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr., C. B. Trego,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,	J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848.	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851.	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851.	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853.	Cleveland, Ohio,	Benjamin Pierce,*		S. St. John, 7	S. F. Baird,	A. L. Elwyn.
8th	April 26 1854.	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte. 8
9th	Aug. 15. 1855.	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856.	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857.	Montreal, Canada.	Alexis Caswell,* 9	Alexis Caswell,*	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858.	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859.	Springfield, Mass.,	Stephen Alexander,	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860.	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866.	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867.	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868.	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869.	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870.	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871.	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.
21st	Aug. 15, 1872.	Dubuque, Iowa,	J. Lawrence Smith,	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.
22d	Aug. 20, 1873	Portland, Me.,	Joseph Lovering,	A. H. Worthen.†	C. A. White,	F. W. Putnam,	W. S. Vaux.
23d	Aug. 12, 1874.	Hartford, Conn.,	J. L. LeConte,	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam,	W. S. Vaux.†

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION C, CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION D, ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION E, MICROSCOPY.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,	—— ——
25th	Aug. 23, 1876,	Buffalo, N. Y.,	W. B. Rogers,	C. A. Young,	Edward S. Morse,	G. F. Barker,	L. H. Morgan,	R. H. Ward,
26th	Aug. 29, 1877,	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,*	R. H. Ward,
27th	Aug. 21, 1878,	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	—— ——[2]	R. H. Ward,[3]
28th	Aug. 27, 1879.	Saratoga, N. Y.	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,
29th	Aug. 25, 1880,	Boston, Mass.,	L. H. Morgan,	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION C, CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION D, ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION E, MICROSCOPY.	TREASURER.
F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	—— ——	W. S. Vaux.
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	W. S. Vaux.
F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	—— ——[2]	T. O. Summers, Jr.,	W. S. Vaux.*
F. W. Putnam,	H. C. Bolton,[5]	F. E. Nipher,	George Little,	A. P. S. Stuart,	—— ——[2]	G. J. Engelmann,	W. S. Vaux.*
F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,	J. G. Henderson,	A. B Hervey,	W. S. Vaux.*
F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	W. S. Vaux.

[1] In the absence of E. C. Pickering.
[2] The Subsection united with Sec. B.
[3] In the absence of G. S. Blackie.
[4] In the absence of Ira Remsen.
[5] In the absence of George Little.
[6] In the absence of A. C. Wetherby.
* Not present.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

ORDER OF PROCEEDINGS

IN

ORGANIZING A MEETING.

---

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers on the register and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Elections to complete the Standing Committee.
7. Election of members.
8. Election of fellows.
9. Unenumerated business.
10. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in general session.

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in general session. But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, to the number of ten for each section, may be elected; the nominations to be made by the Standing Committee and approved by ballot in the respective sections before election by ballot in general session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization or business of both sections at the same meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in general session from the fellows and shall consist of a President, two Vice Presidents, a General Secretary, a Permanent Secretary, a Treasurer, a Secretary of Section A, and a Secretary of Section B; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The Permanent Secretary shall be elected at each fifth meeting.

ART. 10. The President, or, in his absence, one of the Vice Presidents, shall preside at all general sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a general session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the presiding officers of Sections A and B, and of the Sectional Committees, and it shall be part of their duty to give an address, each before his respective section, at such time as the section shall determine. The Vice Presidents may request their respective sections to appoint temporary chairmen to preside over the sessions of the sections, but shall not delegate their other duties.

ART. 12. The General Secretary shall be the Secretary of all general sessions of the Association, and of all sessions of the Standing Committee, and shall keep a record of the business of these sessions. He

shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee. He shall give to the Secretary of each Section the list of papers assigned to it by the Standing Committee.

ART. 13. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually at the first meeting of the Standing Committee, a report which shall be laid before the Association. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least four months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and, at the close of each year, shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ a clerk at such compensation as may be agreed upon by the Standing Committee.

ART. 14. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 15. The Secretaries of Sections A and B shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the secretaries of the sectional committees.

ART. 16. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, General Secretary, Permanent Secretary and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in general session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the section.

ART. 17. The Standing Committee shall consist of the past Presidents, the President, the Vice Presidents, the four Secretaries, the Treasurer, with the above named officers of the preceding meeting, and six fellows elected by ballot after open nomination at the first general session. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The special business of the Committee shall be: to receive and assign papers to the respective sections; to examine and, if necessary, to exclude papers; to decide which papers, discussions and other proceedings shall be published, and to have the general direction of

the publications of the Association; to manage the financial affairs of the Association; to arrange the business and programmes for general sessions; to appoint general sessions for the evening; to suggest subjects for discussion, investigation or reports; to nominate members and fellows; to receive and act upon all invitations extended to the Association and report the same at a general session of the Association.

ART. 18. The Nominating Committee shall consist of the Standing Committee, and four members or fellows elected by each of the sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice Presidents and Secretaries of the Sections shall be recommended to the Nominating Committee by sub-committees consisting of the Vice Presidents and Secretaries, and the four persons elected by each section under the first clause of this article.

MEETINGS.

ART. 19. The Association shall hold public meetings annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 20. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 21. The Association shall be divided into two Sections, namely: **A** (*Mathematics*, *Astronomy*, *Physics*, *Chemistry* and *Mineralogy*) and **B** (*Geology*, *Zoology*, *Botany* and *Anthropology*). Either Section may, at its pleasure, form temporary or permanent subsections for the reading of papers.

ART. 22. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, and the Chairman and Secretary of the subsections, shall form its Sectional Committee. The Sectional Com-

mittees shall have power to fill vacancies in their own numbers. There shall be no sectional meeting during a general session.

ART. 23. When any Subsection organizes, it shall elect a Chairman and Secretary and report the result to the Secretary of its Section. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section. Any Permanent Subsection may elect its Chairman for the ensuing meeting.

ART. 24. No paper shall be read in any Section or Subsection until it has been placed on the programme of the day by the Sectional Committees.

SECTIONAL COMMITTEES.

ART. 25. The Sectional Committees shall arrange and direct the business of their respective sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself must be returned to the Standing Committee with the reasons why it was refused.

ART. 26. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 27. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 28. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

Reports of Meetings.

Art. 29. Whenever practicable, the proceedings and discussions at general sessions, sections and subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the secretaries.

Printed Proceedings.

Art. 30. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles will appear in the printed volumes. The Standing Committee shall have power to print an abstract only of any paper. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

Local Committee.

Art. 31. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

Library of the Association.

Art. 32. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full

shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting, or the value of any not so returned paid to the Permanent Secretary. Not more than ten books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

Admission Fee and Assessments.

ART. 33. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

ART. 34. The annual assessment for members and fellows shall be three dollars.

ART. 35. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Standing Committee.

ART. 36. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

ART. 37. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by two auditors appointed by the Standing Committee.

Alterations of the Constitution.

ART. 38. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in general session, after notice given at a general session of a preceding meeting of the Association.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRON.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22). (Entitled to annual volume.)

MEMBERS.[3]

Abbot, Miss Elizabeth O., 16 Clark St., Cincinnati, Ohio (20).
Abbot, S. L., M. D., 90 Mt. Vernon St., Boston, Mass. (29).
Abbott, Dr. Chas. C., Trenton, N. J. (29).
Abbott, E. K., M. D., Salinas, Monterey Co., Cal. (29).
Acker, E. O'C., U. S. N., U. S. S. Alaska, Care U. S. Navy, Pay Agent, San Francisco, Cal. (28).
Adams, Frank, 41 McGill College Avenue, Montreal, Can. (29).
Adams, John S., M. D., Oakland, Cal. (29).
Adams, Julius W., 155 Congress St., Brooklyn, N. Y. (29).
Agard, Dr. A. H., Oakland, Alameda Co., Cal. (28).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Allen, Edmund T., 419 Olive St., St. Louis, Mo. (27).
Allen, Dr. Harrison, 117 South 20th St., Philadelphia, Pa. (29).
Allen, Horatio. South Orange, N. J. (29).
Allen, J. M., Hartford, Conn. (22).
Allen, Hon. Thomas, St. Louis, Mo. (27).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Alling, Miss Mary R., State Normal School, Providence, R. I. (29).
Allis, Solon M., Tucson, Arizona (29).
Ammen, Rear Admiral Daniel, U. S. Navy, Beltsville, Md. (26).
Anderson, Rev. Joseph, Waterbury, Conn. (29).
Andrews, James M., jr., Saratoga Springs, N. Y. (28).
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Armsby, Henry P., Box 495, New Haven, Conn. (23).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Wm., 1014 Pine St., San Francisco, Cal. (29).
Ashmead, Wm. H., Jacksonville, Fla. (29).
Atchison, Prof. Thos. A., University of Nashville, Nashville, Tenn. (26).
Atkinson, Edward, 131 Devonshire St., Boston, Mass. (29).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwater, Richard W., Millville, N. J. (29).
Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29).
Atwell, W. H., Care Atwell and Sneed, Nashville, Tenn. (26).
Atwood, E. S., East Orange, N. J. (29).
Atwood, John W., Jersey City, N. J. (29).
Auchincloss, Wm. S , 209 Church St., Philadelphia, Pa. (29).
Aughey, Prof. Samuel, Univ. of Nebraska, Lincoln, Neb. (29).

Babbit, John, Fredericton, N. B. (29).
Bailey, E. H. S., Bethlehem, Pa. (25).
Baird, Alexander J., D.D., Nashville, Tenn. (26).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Baker, Wm. E., 158 Tremont St., Boston, Mass. (29).
Ballou, Geo. F., Waltham, Mass. (29).
Ballou, Nahum E., M. D., Sandwich, Ill. (29).
Ballou, Wm. H., Evanston, Ill. (29).
Baptie, George, Normal School, Ottawa, Ontario, Can. (29).
Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., 7 Rowes Wharf, Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barnett, Prof. Samuel, jr., Washington, Wilkes Co., Ga. (26).
Barrows, Nathan, M. D., Hartford, Conn. (29).
Barstow, Charles W., St. Louis, Mo. (27).
Bartholomew, Geo. W., 39 Pearl St., Hartford, Conn. (28).
Bartlett, Arthur K., Battle Creek, Mich. (29).
Bartlett, Prof. Edwin J., Dartmouth Coll., Hanover, N. H. (28).
Bartlett, Dr. Homer L., Flatbush, L. I. (28).
Bassett, Daniel A., Crawfordsville, Ind. (29).
Bassett, George W., M.D., Vandalia, Ill. (20).
Bassnett, Thomas, P. O. Box 936, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, P. O. Box 936, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).

Bastin, Prof. E. S., Univ. of Chicago, Chicago, Ill. (29).
Batchelor, Charles, Electrician, Menlo Park, N. J. (28).
Bate, Gen. Wm. Brimage, Nashville, Tenn. (26).
Batterson, J. G., Hartford, Conn.(23).
Bausch, Edward, Rochester, N. Y. (26).
Bayne, Prof. Herbert A., Royal Military College, Kingston, Ont. (29).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beal, F. E. L., Agricultural College, Ames, Iowa (29).
Bean, Thos. E., Box 441, Galena, Ill. (28).
Beckwith, Henry C., Passed Ass't Eng'r U. S. N., Chesterfield, Conn. (29).
Beebe, William, B. A., New Haven, Conn. (28).
Belfrage, G. W., Clifton, Basque Co., Texas (29).
Belknap, Chas., Lieut. U. S. N., Annapolis, Md. (29).
Belknap, George E., Capt. U. S. N., Malden, Mass. (29).
Belknap, Morris B., Louisville, Ky. (29).
Belknap, Wm. B., Louisville, Ky. (29).
Bell, John J., Exeter, N. H. (22).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y. (25).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19).
Benjamin, Marcus, 6 Barclay St., New York, N. Y. (27).
Berliner, Emile, 95 Milk St., Boston, Mass. (29).
Bessels, Dr. Emil, Smithsonian Institution, Washington, D. C. (26).
Best, W. F., St. John, N. B. (29).
Bickwell, Hon. Thos. W., Boston, Mass. (29).
Bierstadt, Albert, Brevoort House, New York, N. Y. (28).
Bigelow, Wm. Sturgis, M. D., 61 Beacon St., Boston, Mass. (29).
Bill, Charles, Springfield, Mass. (17).
Birge, Charles P., Keokuk, Iowa (29).
Bissell, Wm. S., Pres. Penn. State Agric. Soc'y, Pittsburgh, Pa. (28).
Blackford, Eugene G., 809 Bedford Avenue, Brooklyn, N. Y. (29).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Blair, Henry W., Coast and Geodetic Survey Office, Washington, D. C. (26).
Blaisdell, F. E., Poway, San Diego Co., Cal. (29).
Blake, Mrs. Clarence J., 19 St. James Avenue, Boston, Mass. (29).
Blake, Francis C., Penna. Lead Works, Mansfield Valley, Alleghany Co., Pa. (29).
Blake. Prof. John R., Davidson College, N. C. (29).
Blanchard, Frederick, P. O. Box cc. Lowell, Mass. (29).
Blatchford. Eliphalet W., 375 North La Salle St., Chicago, Ill. (17).
Blatchford, John S., 13 Exchange St., Boston, Mass. (29).
Blodgett, A. N., M.D., Boylston St., Boston, Mass. (29).
Boardman, Miss E. D., 120 Beacon St., Boston, Mass. (29).
Boardman, Mrs. Wm. D., 38 Kenilworth St., Roxbury, Mass. (28).

Boerner, Chas. G., Vevay, Ind. (29).
Bolles, Dr. Wm. P., 571 Dudley St., Dorchester, Mass. (29).
Bond, Geo. W., Boston, Mass. (29).
Borden, Spencer, Fall River, Mass. (29).
Bourland, Dr. Addison M., Van Buren, Ark. (29).
Bowditch, Frederic C., P. O. Box 615, Brookline, Mass. (29).
Bowen, Edmund S., General Supt. Erie Railway, New York, N. Y. (25).
Bowers, Miss Virginia K., Seventy-six P. O., Perry Co., Mo. (27).
Bowles, Miss Margaretta, The Institute, Columbia, Tenn. (26).
Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28).
Boyd, William, Cambridge, Mass. (29).
Boynton, Miss S. P., Box 150, Lynn, Mass. (19).
Brackett, John W., 581 Washington St., Boston, Mass. (29).
Brackett, Solomon H., St. Johnsbury, Vt. (29).
Braden, John, D.D.,Pres. Central College, Nashville, Tenn. (26).
Bradford, Edward H., M. D., 150 Boylston St., Boston, Mass. (29).
Bradley, C. W., 104 North 4th St., St. Louis, Mo. (27).
Bragg, Everett B., No. 3 Park Place, New York, N. Y. (29).
Brandegee, T. S., Cañon City, Col. (29).
Branlette, Mrs. T. E., Louisville, Ky. (29).
Bray, Prof. C. D., College Hill, Mass. (29).
Bray, Mrs. Maria H., West Gloucester, Mass. (29).
Breckinridge, S. M., P. O. Box 2794, St. Louis, Mo. (27).
Brewster, Rev. Joseph, New Haven, Conn. (28).
Brewster, William, 61 Sparks St., Cambridge, Mass. (29).
Briggs, Albert D., 7 Fort Block, Springfield, Mass. (18).
Briggs, Charles S., Vanderbilt University, Nashville, Tenn. (26.)
Briggs, Robert, C. E., 220 So. Fourth St., Philadelphia, Pa. (29).
Britton, N. L., New Dorp, Staten Island, N. Y. (29).
Brooks, Miss Margarette W., Salem, Mass. (29).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brown, Horace, Counsellor at Law, Salem, Mass. (27).
Brown, Hon. John C., Vice President, Texas and Pacific R. R. Co., Pulaski, Tenn. (26).
Brown, Prof. John J., Syracuse Univ., Syracuse, N. Y. (29).
Brown, Jonathan, 390 Broadway, Somerville, Mass. (29).
Brühl, Gustav, N. W. cor. John and Hopkins Sts., Cincinnati, Ohio (28).
Bryan, Malachi Thomas, Nashville, Tenn. (26).
Buck, C. Elton, Wilmington, Del. (29).
Buck, Henry C., West Somerville, Mass. (29).
Buckhout, W. A., State College, Centre Co., Pa. (20).
Buckingham, Chas. L., U. S. Patent Office, Washington, D. C. (28).
Buffum, Miss Fannie A., Linden, Mass. (29).
Buist, John Robinson, M.D., Pres. Nashville Board of Health, Nashville, Tenn. (26).
Burke, Jos. C., Middletown, Conn. (29).

Burke, William, U. S. Patent Office, Washington, D. C. (28).
Burrill, Prof. T. J., Champaign, Ill. (29).
Bush, Rev. Stephen, D. D., Waterford, N. Y. (19).
Butcher, Mrs. S. D., Washington, D. C. (29).
Butler, Cyrus, 24 Cliff St., New York, N. Y. (29).
Butters, Alexander, Carlinville P. O., Ill. (27).
Bynner, Edwin L., 13 Pemberton Square, Boston, Mass. (29).

Cabot, Sam'l, jr., 53 Kilby St., Boston, Mass. (29).
Cady, J. C., 111 Broadway, New York, N. Y. (28).
Calder, Edwin E., Box 17, Providence, R. I. (29).
Caldwell, John W., M.D., Stewart College, Clarksville, Tenn. (26).
Calkins, Dr. Marshall, Springfield, Mass. (29).
Call, Prof. R. Ellsworth, 1722½ Woodland Ave., Des Moines, Iowa (29).
Calthrop, Rev. S. R., Syracuse, N. Y. (25).
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22).
Campbell, Dr. M., Nashville, Tenn. (27).
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25).
Capen, Miss Bessie T., Northampton, Mass. (23).
Carhart, Prof. Henry S., Evanston, Ill. (29).
Carman, Lewis, Bangall, N. Y. (29).
Carpenter, Geo. O., jr., care St. Louis Lead & Oil Co., St. Louis, Mo. (29).
Carrington, Col. Henry B., U. S. A., care Adj. Gen. U. S. A., Washington, D.C. (20).
Case, Col. Theo. S., Ed. Western Review of Science, Kansas City, Mo. (27).
Cassino, S. E., Naturalists' Agency, 32 Hawley St., Boston, Mass. (25).
Castle, Frederick A., M. D., 102 East 57th St., New York, N. Y. (29).
Caswell, John H., 11 West 48th Street, New York, N. Y. (26).
Cattell, William C., President Lafayette College, Easton, Pa. (15).
Chace, Arnold B., Valley Falls, R. I. (29).
Chadbourn, Erlon R., Lewiston, Me. (29).
Chadwick, James R., M. D., Clarendon St., Boston, Mass. (29).
Chance, H. Martyn, 2433 Fairmount Avenue, Philadelphia, Pa. (29).
Chandler, Prof. Chas. Henry, Yellow Springs, Green Co., Ohio (28).
Chandler, Seth C., jr., 153 Tremont St., Boston, Mass. (29).
Channing, Dr. Wm. F., Providence, R. I. (29).
Chapman, F. M., Danforth, Iroquois Co., Ill. (17).
Chapman, Henry C., M. D., 1226 Walnut St., Philadelphia, Pa. (29).
Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26).
Chase, Prof. F. A., Fisk University, Nashville, Tenn. (26).
Chase, Prof. Pliny E., Haverford College, Haverford, Pa. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).
Chatfield, A. F., Albany, N. Y. (29).
Chauvenet, Regis, 701 Pine St., St. Louis, Mo. (27).
Cheney, Miss Margaret S., Jamaica Plain, Mass. (29).
Chester, Prof. Albert H., Hamilton College, Clinton, N. Y. (29).

Chester, Lt. Com'dr Colby M., U. S. N.,Coast Survey Office, Washington, D. C. (28).
Chittenden, Russell H., Ph. D., 95 Humphrey St., New Haven, Conn. (29).
Cisco, J. G., Jackson, Tenn. (29).
Clark, F. Y., M.D., Saratoga Springs, N. Y. (28).
Clark, Simeon T., M.D., Lockport, Niagara Co., N. Y. (25).
Clarke, Miss Cora H., Jamaica Plain, Mass. (29).
Clarke, Hon. Enos, St. Louis, Mo. (27).
Clarke, Maurice D., 68 Ohio St., East Cambridge, Mass. (29).
Clevenger, Dr. S. V., 189 37th St., Chicago, Ill. (29).
Cloud, Jno. W., Altoona, Pa. (28).
Coakley, George W., LL.D., Hempstead, L. I. (29).
Cobb, Samuel C., 235 Boylston St., Boston, Mass. (29).
Cobb, Rev. Sanford H., Saugerties, N. Y. (29).
Coe, Miss Emily M., 33 West 45th St., New York, N. Y. (25).
Coffinberry, W. L., Grand Rapids, Mich. (20).
Colby, C. E., M. E., School of Mines, Columbia College, New York, N. Y. (28).
Collier, M. Dwight, St. Louis, Mo. (27).
Collier, Peter, Dept. of Agric., Washington, D. C. (29).
Collin, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21).
Collins, Lieut. Fred'k, U. S. N., Care Navy Department, Washington, D. C. (28).
Colman, Henry, M.D., Swampscott, Mass. (25).
Colton, G. Woolworth, 172 William St., New York, N. Y. (22).
Colyar, John B., Nashville, Tenn. (26).
Comstock, J. Henry, Dep't of Agriculture, Washington, D. C. (28).
Comstock, Dr. Thos. G., 14th & Washington Avenue, St. Louis, Mo. (29).
Conant, Alban J., St. Louis, Mo. (27).
Cone, James B., 357 Lexington Ave., New York, N. Y. (28).
Conklin, W. A., Director Central Park Menagerie, New York, N. Y. (29).
Constable, Casimir, Chief Engr. Colorado Coal & Iron Co. South Pueblo, Col. (29).
Cook, Dr. Charles D., 133 Pacific St., Brooklyn, N. Y. (25).
Coolidge, Herbert, 48 Kilby St., Boston, Mass. (29).
Coon, Henry C., M. D., Alfred Centre, N. Y. (29).
Copes, Dr. Joseph S., New Orleans, La. (11).
Corson, Mrs. Henrietta H., 137 Washington St., Hartford, Conn. (28).
Couzins, Miss Phœbe W., LL.B., 2647 Washington Ave., St. Louis, Mo. (27).
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Sullivan, J. A., U. S. Coast Survey Office, Washington, D. C. (27).
Summers, Thos. O, M. D., Vanderbilt Univ., Nashville, Tenn. (22).
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20).
Swasey, Oscar F., M.D., Beverly, Mass. (17).
Swift, Lewis, Ph. D., Rochester, N. Y. (29).

Tainter, Sumner, Watertown, Mass. (29).
Talbot, Henry, Waterloo, Monroe Co., Ill. (27).
Tatum, Jos. T., 221 Chestnut St., St. Louis, Mo. (27).

Taylor, Arthur F., care School of Applied Science, Cleveland, Ohio (29).
Taylor, Thos. Dept. of Agric., Washington, D. C. (29).
Taylor, Wm. B., Smithsonian Institution, Washington, D. C. (29).
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Tepper, Fred., P. O. Box 3331, New York, N. Y. (29).
Terry, James, American Museum Natural History, Central Park (77th St. and 8th Ave.), New York, N. Y. (28).
Theobald, Albert G. R., Forest Dept., Anamalai, Coimbatore District, India (27).
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Thompson, Prof. Chas. O., Worcester, Mass. (29).
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Thomson, Prof. A., Ames, Iowa (21).
Thorburn, John, LL. D., Ottawa, Ont., Can. (29).
Thruston, Gates P., Cor. Sec. Tenn. Hist. Society, Nashville, Tenn. (26).
Thurber, Miss Elizabeth, Plymouth, Mass. (22).
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Timby, Theo. R., Nyack, N. Y. (29).
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Todd, Albert, St. Louis, Mo. (27).
Todd, Andrew J., 37 Park Row, New York, N. Y. (29).
Todd, D. P., Nautical Almanac Office, Washington, D. C. (27).
Todd, Prof. James E., Tabor, Fremont Co., Iowa (22).
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Toney, Rev. Thomas (26).
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Toombs, Robert, M. D., Washington, Ga. (26).
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Townshend, Prof. N. S., Columbus, Ohio (17).
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27).
Treadwell, Geo. A., Phœnix, Arizona (29).
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Treat, Hon. Sam'l, St. Louis, Mo. (27).
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Trowbridge, Luther H., Detroit, Mich. (29).
Trowbridge, S. H., Glasgow, Mo. (27).
True, Fred W., Ass't U. S. Fish Commission, Washington, D. C. (28).
Trumbull, Dr. J. H., Hartford, Conn. (29).
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Tyler, John M., Amherst, Mass. (29).

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Van Brunt, C., Poughkeepsie, N. Y. (28).
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28).
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Vredenburgh, Edw. H., Rochester, N. Y. (29).

Wainright, Ellis, St. Louis, Mo. (29).
Wait, Prof. Chas. E., Rolla, Mo. (29).
Waite, Chas. N., Manchester, N. H. (29).
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Walcott, Charles D., U. S. Geol. Survey, Washington, D. C. (25).
Walcott, Dr. H. P., Cambridge, Mass. (29).
Waldo, Leonard, S. D., New Haven, Conn. (28).
Walker, Dr. G. S., St. Louis, Mo. (27).
Walker, George C., 124 La Salle St., Chicago, Ill. (17).
Walker, N. B., 19 and 21 Cliff St., New York, N. Y. (20).
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Wallace, Wm., Ansonia, Conn. (28).
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28).
Walton, Miss Evelyn M., Sangus Centre, Mass. (29).
Walton, Jos. J., Philadelphia, Pa. (29).
Walworth, Rev. Clarence A., Albany, N. Y. (28).
Walworth, Mrs. Ellen Hardin, Saratoga Springs, N. Y. (28).
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Ward, W. E., D.D., President Ward's Seminary for Young Ladies, Nashville, Tenn. (26).
Warder, Prof. Robert B., North Bend, Hamilton Co., Ohio (19).
Wardwell, George J., Rutland, Vt. (20).
Waring, Col. George E., Jr., Newport, R. I. (29).
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Warren, Mrs. S. D., 67 Mt. Vernon St., Boston, Mass. (29).
Warring, Charles B., Poughkeepsie, N. Y. (25).

Waterhouse, Al, M. D., Jamestown, N. Y. (29).
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Watkins, Samuel, President Board of Education, Nashville, Tenn. (26).
Weakley, Richard W., Supt. Public Schools, Nashville, Tenn. (26).
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23).
Weist, J. R., M. D., Richmond, Ind. (29).
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Whitaker, Prof. Channing, Mass. Inst. Technology, Boston, Mass. (29).
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White, Prof. Charles Joyce, Cambridge, Mass. (29).
White, Miss Florence, Pittsfield, Mass. (29).
White, Prof. Frances E., Woman's Med. College, Philadelphia, Pa. (29).
White, Prof. H. C., Univ. of Georgia, Athens, Ga. (29).
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White, LeRoy S., Box 157, Waterbury, Conn. (23).
White, Thomas W., Hernando, De Soto Co., Miss. (28).
White, Z. L., Washington, D. C. (26).
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Whitworth, John L., Nashville, Tenn. (26).
Wickstead, R. J., LL. D., Ottawa, Province of Ont., Can. (29).
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Wilder, Gen. John T., Chattanooga, Tenn. (26).
Wilkin, Daniel F., Nashville, Tenn. (26).
Willets, Joseph C., Skaneateles, N Y. (29).
Williams, Charles H., Buffalo, N. Y. (25).
Williams, David, 83 Reade St., New York, N. Y. (29).
Williams, E. B., Strawberry Point, Iowa (24).

Williams, Edward H., jr., Danville, Pa. (25).
Williams, F. H., M. D., 100 Boylston St., Boston, Mass. (29).
Williams, Frank, Buffalo, N. Y. (25).
Williams, H. S., Cornell University, Ithaca, N. Y. (18).
Williams, P. O., M.D., Watertown, Jefferson Co., N. Y. (24).
Williams, Rev. R. R., Canajoharie, Montgomery Co., N. Y. (28).
Williams, Willoughby, Nashville, Tenn. (26).
Williamson, Lieut. Col. R. S., U. S. Engineers, Light House Engineer, San Francisco, Cal. (12).
Willoughby, Hugh L., Saratoga Springs, N. Y. (28).
Wills, A. W., corner Union and Cherry streets, Nashville, Tenn. (26).
Wilmot, Thos. J., Direct U. S. Cable Co., Rye, N. H. (27).
Wilson, Prof. Hiram A., Saratoga Springs, N. Y. (28).
Wilson, M. C., Florence, Ala. (26).
Winslow, Sam'l W., Room 30, Rialto Building, 131 Devonshire cor. Milk St., Boston, Mass. (29).
Winsor, Justin, Lib'n Harv. Univ., Cambridge, Mass. (29).
Witherbee, Frank S., care Witherbees, Sherman & Co., Port Henry, N. Y. (29).
Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29).
Wolcott, Mrs Henrietta L. T., Hotel Vendôme, Boston, Mass. (29).
Wood, Prof. De Volson, Hoboken, N. J. (29).
Wood, Dr. Rob't W., Jamaica Plain, Mass. (29).
Wood, Dr. Wm., Portland, Me. (29).
Woodbury, C. J. H., 131 Devonshire St., Boston, Mass. (29).
Woodward, J. J., M. D., Army Medical Museum, Washington, D. C. (28).
Woodward, Richard W., Ouray, Col. (29).
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Worthen, T. W. D., Hanover, N. H. (28).
Wright, Rev. Geo. F., Andover, Mass. (29).
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Yeatman, Prof. Joseph W., Nashville, Tenn. (26).
Youmans, Wm. Jay, M. D., Popular Science Monthly, 549 Broadway, New York, N. Y. (28).

Zentmayer, Joseph, 147 So. Fourth St., Philadelpeia, Pa. (29).

[1149 MEMBERS.]

NOTE.—The omission of an address in the foregoing list, indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

FELLOWS.[1]

Abbe, Prof. Cleveland, Washington, D. C. (16). 1874.
Agassiz, Alex., Curator Mus. Comp. Zool., Cambridge, Mass. (18). 1875.
Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20). 1874.
Allen, Joel A., Mus. Comp. Zoology, Cambridge, Mass. (18). 1875.
Allen, Zachariah, Providence, R. I. (1). 1874.
Alvord, Benj., Brig. Gen., U. S. Army, 1207 Q St., N. W., Washington, D. C. (17). 1874.
Anthony, Prof. Wm. A., Cornell Univ., Ithaca, N. Y. (28). 1880.
Appleton, Prof. John H., Brown Univ., Providence, R. I. (22). 1874.
Austen, Peter T., Ph. D., Rutgers College, Lock Box No. 2, New Brunswick, N. J. (26). 1879.
Austin, E. P., 46 E. Newton St., Boston, Mass. (18). 1874.

Bailey, Prof. Loring W., University of Fredericton, N. B. (18). 1875.
Bailey, W. W., Olney Prof., Brown University, Providence, R. I. (18). 1874.
Baird, Prof. S. F., Sec'y Smith. Inst., Washington, D. C. (1). 1875.
Bandelier, Ad. F., Highland, Ill. (25). 1879.
Barker, Prof. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1875.
Barnard, F. A. P., President Columbia College, New York, N. Y. (7). 1874.
Barnard, Gen. J. G., U. S. Army, Army Building, New York, N. Y. (14). 1875.
Barnard, Wm. S., Ithaca, N. Y. (24). 1880.
Bassett, Homer F., Waterbury, Conn. (23). 1874.
Batchelder, Dr. J. H., Salem, Mass. (18). 1874.
Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875.
Beal, Prof. Wm. James, Agricultural College, Lansing, Mich. (24). 1880.
Bell, Prof. Alex. Graham, Boston University, Boston, Mass. (26). 1879.
Bell, Samuel N., Manchester, N. H. (7). 1874.
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21). 1880.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Bickmore, Prof. Albert S., Am. Mus. of Nat. Hist., Central Park, New York. N. Y. (17). 1880.
Blackie, Geo. S., M. D., Nashville, Tenn. (26). 1877.
Blake, Clarence J., M. D., Hotel Berkeley, Boston, Mass. (24). 1877.
Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, jr., U. S. Coast Survey, Newton Lower Falls, Mass. (23). 1874.
Bolton, Dr. H. Carrington, Trinity Coll., Hartford, Conn. (17). 1875.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bouvé, Thos. T., Pres't Boston Soc. Nat. Hist., Boston, Mass. (1). 1875.

[1] The date at the end of the line indicates the year when the member was made a Fellow.

Bowditch, Prof. H. P., Harvard Med. School, Boston, Mass. (28). 1880.
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875.
Breneman, A. A., Ithaca, N. Y. (20). 1880.
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875.
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879.
Brocklesby, Prof. John, Trinity College, Hartford, Conn. (23). 1875.
Brooks, Major Thomas B., Geol. State Surveys, Mich. and Wis., Newburgh, N. Y. (26). 1879.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., Office Gas Light Co., Cincinnati, Ohio (11). 1874.
Brown, Mrs. Robert, jr., Cincinnati, Ohio (17). 1874.
Brush, Prof. George J., Yale College, New Haven, Conn. (4). 1874.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass (22). 1877.
Burnham, S. W., 52 Vincennes Ave., Chicago, Ill. (25). 1877.

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875.
Canby, Wm. M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878.
Capen, Rev. Francis L., 5 Worcester Sq., Boston, Mass. (23). 1874.
Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21). 1875.
Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
Carr, Lucien, Peabody Mus. Arch. and Ethn., Cambridge, Mass. (25). 1877.
Chadbourne, Prof. P. A., Pres't Williams Coll., Williamstown, Mass. (10). 1875.
Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., N. Y. (19). 1875.
Chandler, Prof. W. H., The Lehigh Univ., Bethlehem, Pa. (19). 1874.
Chanute, O., care Erie Railway, New York, N. Y. (17). 1877.
Chesbrough, E. S., Chicago, Ill. (2). 1877.
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877.
Clark, Alvan G., Cambridgeport, Mass. (28). 1880.
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17). 1875.
Clarke, Prof. F. W., Cincinnati University, Cincinnati, Ohio (18). 1874.
Coffin, Prof. John H. C., U. S. Navy, Washington, D. C. (1). 1874.
Coffin, Prof. Selden J., Ph.D., Lafayette College, Easton, Pa. (22). 1874.
Collett, Prof. John, Chief Ind. Bureau of Statistics of Geology, Indianapolis, Ind. (17). 1874.
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880.
Comstock, Prof. M. L., Ph. D., Galesburg, Knox Co., Ill. (21). 1874.
Comstock, Prof. Theo. B., Eureka, San Juan Co., Col. (24). 1877.
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24). 1880.
Cook, Prof. George H., New Brunswick, N. J. (4). 1875.
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880.

Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1875.
Cox, Prof. Edward T., Tucson, Arizona Terr. (19). 1874.
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Crocker, Charles F., Lawrence, Mass. (22). 1874.
Crocker, Susan E., M.D., Lawrence, Mass. (21). 1874.
Cross, Prof. Chas. R., Mass. Inst. Technology, Boston, Mass. (29). 1880.
Cummings, Rev. Joseph, D.D., 82 Prospect St., Cambridgeport, Mass. (13). 1874.
Curtis, Dr. Josiah, Washington, D. C. (18). 1874.
Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17). 1874.

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Dana, Edward Salisbury, New Haven, Conn. (23). 1875.
Dana, Prof. James D., New Haven, Conn. (1). 1875.
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Day, F. H., M. D., Wauwatosa, Wis. (20). 1874.
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Edison, Thos. A., Menlo Park, N. J. (27). 1878.
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Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22). 1874.
Emerton, James H., Salem, Mass. (18). 1875.
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Engelmann, Dr. George, St. Louis, Mo. (1). 1875.
Engelmann, Geo. J., M. D., 3003 Locust St., St. Louis, Mo. (25). 1878.
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Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Henry, Ph.D., St. Johnsbury, Vt. (14). 1874.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1875.
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Farquharson, Dr. Robert James, Davenport, Iowa (24). 1880.
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Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
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Fitch, O. H., Ashtabula, Ohio (7). 1874.
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Freeman, H. C., Alto Pass, Union Co., Ill. (17). 1876.
Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880.

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Gardner, James T., Director N. Y. State Survey, Albany, N. Y. (25). 1879.
Garland, Rev. Dr. L. C., Chancellor Vanderbilt Univ., Nashville, Tenn. (25). 1877.
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Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1875.
Gilbert, G. K., P. O. Box 240, Salt Lake City, Utah (18). 1874.
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Gooch, Frank A., U. S. Geological Survey, Newport, R. I. (25). 1880.
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875.
Goode, Prof. G. Brown, Washington, D. C. (22). 1874.
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Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874.
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Green, Traill, M. D. Easton, Pa. (1). 1874.
Grimes, J. Stanley, 165 Mason St., Milwaukee, Wis. (17). 1874.
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Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877.
Hall, Prof. James, Albany, N. Y. (1). 1875.
Hambly, John B., Portsmouth, R. I. (18). 1874.
Hamlin, Dr. A. C., Bangor, Me. (10). 1874.

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Harkness, Prof. Wm., U. S. N., U. S. Observatory, Washington, D. C. (24). 1874.
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Harrison, Dr. B. F., Washington, Conn. (11). 1874.
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Hazelton, Rev. David Greene, Cambridge, Mass. (23). 1874.
Hazlett, C. S., Johns Hopkins University, Baltimore, Md. (25). 1878.
Hawes, Dr. George W., National Museum, Washington, D. C. (23). 1875.
Hedrick, B. S., Examiner U. S. Pat. Office, Washington, D. C. (19). 1874.
Henderson, John G., Winchester, Ill. (25). 1879.
Henshaw, Henry W., Care Major J. W. Powell, 910 M St., Washington, D. C. (24). 1877.
Herrick, Mrs. Sophie B. (23). 1877.
Hervey, Rev. A. B., Taunton, Mass. (22). 1879.
Hilgard, Prof. E. W., Univ. of California, Berkeley, Cal. (11). 1874.
Hilgard, Prof. J. E., U. S. Coast Survey, Washington, D. C. (4). 1874.
Hill, George W., Nautical Almanac Office, Navy Department, Washington, D. C. (23). 1874.
Hill, Rev. Dr. Thomas, 68 Deering St., Portland, Me. (3). 1875.
Hinrichs, Prof. Gustavus, State University, Iowa City, Iowa (17). 1874.
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874.
Hosmer, Prof. E. S., Director Washburn Observatory, Univ. of Wisconsin, Madison, Wis. (23). 1875.
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876.
Hosford, Charles E., Terre Haute, Ind. (20). 1874.
Hough, Franklin B., Lowville, N. Y. (4). 1874.
Hough, G. W. (15). 1874.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Montreal, Canada (1). 1874.
Huntington, Prof. J. H., Box 1914, Boston, Mass. (19). 1874.
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875.
Hyatt, James, Stanfordville, Duchess Co., N. Y. (10). 1874.

Irving, Roland D., Wisconsin State Geol. Survey, Madison, Wis. (26). 1879.

James, Thomas Potts, Cambridge, Mass. (22). 1879.
Jenks, Elisha T., Middleborough, Mass. (22). 1874.
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874.
Johnson, Prof. S. W., Sheffield Sci. School, New Haven, Conn. (22). 1874.
Joy, Prof. Charles A., Stockbridge, Mass. (8). 1879.
Julien, A. A., School of Mines, Columbia Coll., New York, N. Y. (24). 1875.

Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
Langley, Prof. J. W., Univ. of Michigan, Ann Arbor, Mich. (23). 1875.
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18). 1874.
Lattimore, Prof. S. A., Univ. of Rochester, Rochester, N. Y. (15). 1874.
Lawrence, George N., 45 E 21st St , New York, N. Y. (7). 1877.
Laws. Samuel S.. Pres. Univ. of Missouri, Columbia, Mo. (23). 1874.
Lea, Dr. Isaac, 1622 Locust St., Philadelphia, Pa. (1). 1875.
LeConte, Dr. John L., 1625 Spruce St., Philadelphia, Pa. (1). 1874.
Leeds. Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874.
Leonard, N. R., State University. Iowa City, Iowa (21). 1875.
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Philadelphia, Pa. (2). 1874.
Levette, Dr. Gilbert M., Geologist, Indianapolis. Ind. (23). 1874.
Lewis, Prof. Henry C., E. Washington Lane, Germantown, Pa. (26). 1880.
Lindsley, Dr. J. B., Nashville, Tenn. (1). 1874.
Lintner, J. A., N. Y. State Mus. of Nat. Hist.. Albany, N. Y. (22). 1874.
Little, Prof. Geo., State Geologist of Georgia, Atlanta, Ga. (15). 1874.
Little, W. C., Albany. N. Y. (22). 1874.
Litton, Abram, Washington University, St. Louis, Mo. (28). 1879.
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 1875.
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.
Loughridge, Prof. R. H., Ass't State Geologist, Atlanta, Ga. (21). 1874.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1875.
Lull, Edward P., Commander U. S. N., Washington, D. C. (28). 1880.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyle, David Alexander, Lieut. U. S. A., Ordnance Department, National Armory, Springfield, Mass. (28). 1880.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1875.
Lyman, Col. Theodore, Brookline, Mass. (23). 1875.
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

Mallery, Brevet Lieut. Col. Garrick, U. S. Army, P. O. Box 585, Washington, D. C. (26). 1879.
Mann, B. Pickman, 19 Follen St., Cambridge, Mass. (22). 1874.
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874.
Mark, Edw. L., 48 Shepard St., Cambridge, Mass. (21). 1879.
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15). 1874.
Martin, Prof. H. Newell, Johns Hopkins Univ., Baltimore, Md. (27). 1880.
Mason. Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayer, Prof. A. M., So. Orange, N. J. (19). 1874.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McRae, Hamilton S., Supt. of Schools, Muncie, Ind. (20). 1874.
Means, A., D.D., LL.D., Oxford, Ga. (5). 1877.
Meehan, Thomas, Germantown, Pa. (17). 1875.
Mees, Carl Leo, Louisville, Ky. (24). 1876.

Mendenhall, Prof. T. C., care Martin Hensel, Columbus, Ohio (20). 1874.
Merriman C. C., Rochester, N. Y. (29). 1880.
Michelson, A. A., Master U. S. N., Nautical Almanac Office, Washington, D. C. (26). 1879.
Minot, Charles Sedgwick, Roslindale P. O., Boston, Mass. (28). 1880.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22). 1874.
Morgan, Hon. L. H., Rochester, N. Y. (10). 1875.
Morley, Prof. Edward W., Hudson, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875.
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22). 1874.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13). 1874.
Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24). 1877.
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia College, New York, N. Y. (5). 1875.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13). 1874.
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18). 1875.
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876.
Norton, Prof. W. A., New Haven, Conn. (6). 1875.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1875.
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 1874.
Ordway, Prof. John M., Mass. Inst. Technology, Boston, Mass. (9). 1875.
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875.
Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22). 1874.
Owen, Dr. R., New Harmony, Ind. (20). 1874.

Packard, Dr. A. S., jr., 115 Angell St., Providence, R. I. (16).
Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parkhurst, H. M., Law Stenographer, 31 Park Row, New York, N. Y. (23). 1874.
Parry, Dr. Charles C., Davenport, Iowa (6). 1875.
Peckham, S. F., Univ. of Minnesota, Minneapolis, Minn. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1875.
Peirce, B. O., Beverly, Mass. (18). 1874.
Perkins, Maurice, Schenectady, N. Y. (15). 1875.
Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.
Phillips, A. W., New Haven, Conn. (24). 1879.

Phippen, Geo. D., Salem, Mass. (18). 1874.
Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1875.
Potter, Prof. William B., Washington University, St. Louis, Mo. (25). 1879.
Powell, Major J. W., 910 M St., Washington, D. C. (23). 1875.
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875.
Prime, Frederick, jr., Ass't Geologist of Pa., 907 Walnut St., Philadelphia, Pa. (24). 1875.
Pulsifer, Wm. H., St. Louis, Mo. (26). 1879.
Pumpelly, Prof. Raphael, U. S. Geol. Survey, Newport, R. I. (17). 1875.
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.
Putnam, J. Duncan, Cor. Sec. Davenport Acad. of Nat. Sciences, Davenport, Iowa (27). 1880.
Putnam, James J., M. D., 63 Marlborough St., Boston, Mass. (28). 1880.
Pynchon, Rev. T. R., Pres't of Trinity Coll., Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.
Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.
Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875.
Redfield, J. H., care A. Whitney and Sons, Philadelphia, Pa. (1). 1874.
Rees, Prof. John K., Washington University, St. Louis, Mo. (26). 1878.
Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875.
Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.
Richards, Mrs. R. H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.
Ricketts, Pierre de Peyster, Ph.D., School of Mines, Columbia College, New York, N. Y. (26). 1880.
Riley, Prof. C. V., U. S. Entomologist, 1700 13th St. N.W., Washington, D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, Pa. (11). 1874.
Rogers, Prof. Robert E., Jefferson Medical College, Philadelphia, Pa. (18). 1874.
Rogers, W. A., Ass't Harvard College Observ., Cambridge, Mass. (15). 1875.
Rogers, Prof. Wm. B., 117 Marlborough St., Boston, Mass. (1). 1874.
Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879.
Rood, Prof. O. N., Cor. E. 49th St. & Madison Ave., New York, N. Y. (14). 1875.
Roosevelt, Clinton, 11 Wall St., New York, N. Y. (11). 1874.
Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1875.

Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (13). 1875.

Sadtler, Prof. S. P., University of Pa., Philadelphia, Pa. (22). 1875.
Safford, Dr. James M., Nashville, Tenn. (6). 1875.
Saunders, William, London, Ontario, Canada (17). 1874.
Schott, Charles A., Coast Survey Office, Washington, D. C. (8). 1874.
Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24). 1877.
Scudder, Samuel H., Cambridge, Mass. (13). 1874.
Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, D. C. (23). 1874.
Shaler, Prof. N. S., Harvard University, Cambridge, Mass. (19). 1875.
Sheafer, P. W., Pottsville, Pa. (4). 1879.
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.
Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1874.
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19). 1874.
Skinner, Joseph J., New Britain, Conn. (23). 1880.
Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20). 1877.
Smith, Mrs. Erminnie A., 203 Pacific Ave., Jersey City, N. J. (25). 1880.
Smith, Prof. F. H., University of Virginia, Charlottesville, Va. (26). 1880.
Smith, Dr. J. Lawrence, Louisville, Ky. (1). 1874.
Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1875.
Smock, Prof. John Conover, Rutgers College, New Brunswick, N. J. (23). 1879.
Spencer, John W., Geol , Paxton, Sullivan Co., Ind. (20). 1874.
Springer, Dr. Alfred, P. O. Box 442, Cincinnati, Ohio (24). 1880.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., P. O. Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
Stephens, W. Hudson, Lowville, N. Y. (18). 1874.
Sternberg, George M., Surgeon U.S.A., Care Surgeon General, U.S.A., Washington, D. C. (24). 1880.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1875.
Stone, Mrs. Leander, 3352 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Cincinnati Observatory, Univ. of Cincinnati, Mt. Lookout, Ohio (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874.
Stubbs, Prof. William C., Alabama Agricultural and Mechanical College, Auburn, Lee Co., Ala. (25). 1877.
Swallow, Prof. G. C., Columbia, Mo. (10). 1875.

Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thompson, A. Remsen, 50 West 51st Street, New York, N. Y. (1). 1874.
Thurston, Prof. R. H., Stevens Institute, Hoboken, N. J. (23). 1875.

Trembley, J. P., M.D., Oakland, Alameda Co., Cal. (17). 1880.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25). 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.
Upham, Warren, Nashua, N. H. (25). 1880.

Van der Weyde, P. H., M. D., P. O. Box 3619, New York, N. Y. (17). 1874.
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875.
VAUX, WILLIAM S., 1702 Arch St., Philadelphia, Pa. (1). 1875.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16). 1875.
Vose, Prof. Geo. L., Bowdoin College, Brunswick, Me. (15). 1875.

Wadsworth, M. Edward, Ph. D., Assistant in Lithology, Museum of Comparative Zoology, Cambridge, Mass. (23). 1874.
Walker, Prof. J. R., 154 Canal St., New Orleans, La. (19). 1874.
WALLER, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walling, H. F., U. S. Coast & Geodetic Survey Office, Washington, D. C. (16). 1874.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875.
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 1874.
Warren, Gen. G. K., U.S.A., Engineer's Office, Newport, R. I. (12). 1875.
Warren, Prof. S. Edward, Newton, Mass. (17). 1875.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1875.
Wead, Prof. Charles K., Univ. of Michigan, Ann Arbor, Mich. (23). 1880.
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (7). 1874.
Wells, Daniel H., Hartford, Conn. (18). 1875.
Westcott, O. S., Racine, Wis. (21). 1874.
Wetherby, Albert G., Univ. of Cincinnati, Cincinnati, Ohio (26). 1877.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheeler, Capt. Geo. M., U. S. E., Lock Box 93, Washington, D. C. (25). 1879.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875.
Whitfield, R. P., Am. Mus. Nat. Hist., 77th St. & 8th Avenue, N. Y. (18). 1874.
Whitman, Chas. O., care G. F. Leonard, Newton Highlands, Mass. (27). 1880.
Whitney, Solon F., Watertown, Mass. (20). 1874.
Whittlesey, Col. Charles, Historical Rooms, Cleveland, Ohio (1). 1875.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.

Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875.
Wiley, Prof. Harvey W., Purdue Univ., Lafayette, Ind. (21). 1874.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, 117 Bloor St., Toronto, Canada (25). 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1875.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Witter, F. M., Muscatine, Iowa (21). 1875.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875
Wyckoff, Wm. C., 44 Howard St., New York, N. Y. (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Yarrow, Dr. H. C., care Army Med. Mus., Washington, D. C. (23). 1874.
Youmans, Prof. Edward L., New York, N. Y. (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, New Jersey (18). 1874.

[406 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1555.

DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.

Abert, J. J., Washington, D. C. (1). Born in 1785. Died January 27, 1863.

Adams, C. B., Amherst, Mass. (1). Born January 11, 1814. Died Jan'y 19, 1853.

Adams, Edwin F., Charlestown, Mass. (18).

Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.

Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec. 14, 1873.

Ainsworth, J. G., Barry, Mass. (14).

Allston, R. F. W., Georgetown, S. C. (3). Born April 21, 1801. Died April 7, 1864.

Andrews, E. B., Lancaster, Ohio (7).

Anthony, Charles H., Albany, N. Y. (6). Died in 1874.

Ames, N. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.

Appleton, Nathan, Boston, Mass. (1). Born Oct. 6, 1779. Died July 14, 1861.

Armstrong, John W., Fredonia, N. Y. (24).

Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11, 1878.

Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.

Bache, Franklin, Philadelphia, Pa. (1). Born Oct. 25, 1792. Died March 19, 1864.

Bailey, Jacob W., West Point, N. Y. (1). Born April 29, 1811. Died Feb. 26, 1857.

Bardwell, F. W., Lawrence, Kan. (13.) Died in 1878.

Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.

Beck, C. F., Philadelphia, Pa. (1).

Beck, Lewis C., New Brunswick, N. J. (1). Born Oct. 4, 1798. Died April 21, 1853.

Beck, T. Romeyn, Albany, N. Y. (1). Born Aug. 11, 1791. Died Nov. 19, 1855.

Belt, Thomas, London, Eng. (27). Died Sept 8, 1878.

Benedict, G. W., Burlington, Vt. (16).

Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19, 1877.
Binney, Amos, Boston, Mass. (1). Born Oct. 18, 1803. Died Feb. 18, 1847.
Binney, John, Boston, Mass. (3).
Blake, Homer C., New York, N. Y. (28). Born Feb. 1, 1822.
Blanding, William, ———, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.
Bomford, George, Washington, D. C. (1). Born 1780. Died March 25, 1848.
Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Brigham, Charles H., Ann Arbor, Mich. (17). Born July 27, 1820. Died in Jan., 1879.
Brown, Andrew, Natchez, Miss. (1).
Burnap, G. W., Baltimore, Md. (12). Born Nov. 20, 1802. Died Sept. 8, 1859.
Burnett, Waldo I., Boston, Mass. (1).
Butler, Thomas B., Norwalk, Conn. (10).

Cairns, F. A., New York, N. Y. (27). Died in 1879.
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, Leonard, Cleveland, Ohio (15). Born 1820. Died 1880.
Case, William, Cleveland, Ohio (6).
Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died Jan. 8, 1877.
Chapman, N., Philadelphia, Pa. (1). Born May 28, 1780. Died July 1, 1853.
Chase, S., Dartmouth, N. H. (2).
Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec. 13, 1870.
Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.
Clark, H. J., Cambridge, Mass. (13).
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1). Born Sept. 6, 1806. Died Feb. 6, 1873.
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooke, Caleb, Salem, Mass. (18). Born Feb. 5, 1838. Died June 5, 1880.

Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6). Born Dec. 14, 1794. Died April 9, 1872.
Couper, J. Hamilton, Darien, Ga. (1).
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 18, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18).
Croswell, Edwin, Albany, N. Y. (6). Born May, 1797. Died June 18, 1871.
Curry, W. F., Geneva, N. Y. (11).

Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.
Dean, Amos, Albany, N. Y. (6). Born Jan. 16, 1803. Died Jan. 26, 1868.
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York, N. Y. (1). Born 1792. Died Nov. 21, 1851.
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1). Born Oct. 25, 1781. Died Dec. 15, 1867.
Dexter, G. M., Boston, Mass. (11).
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Dorr, E. P., Buffalo, N. Y. (25). Died March 28, 1881.
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Born July 4, 1794. Died June 26, 1869.
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10).
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14).
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emerson, Geo. B., Boston, Mass. (1).
Emmons, Ebenezer, Williamstown, Mass. (1). Born May 16, 1799. Died October 1, 1863.
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2). Born April 11, 1794. Died Jan. 15, 1865.
Ewing, Thomas, Lancaster, Ohio (5). Born Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York, N. Y. (6). Born Oct. 9, 1798. Died June 16, 1873.

Feuchtwanger, Lewis, New York, N. Y. (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7). Born Jan. 7, 1800. Died March 8, 1874.
Fisher, Mark, Trenton, N. J. (10).
Fitch, Alexander, Hartford, Conn. (1).
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born Nov. 26, 1790. Died Jan. 23, 1868.
Ford, A. C , Nashville, Tenn. (26).
Foster, J. W., Chicago, Ill. (1). Born in 1815. Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Born Oct. 17, 1795. Died Feb. 6, 1865.
Fox, Charles, Grosse Ile, Mich. (7).
Frazer, John F., Philadelphia, Pa. (1).
French, J. W., West Point, N. Y. (11).

Gavit, John E., New York, N. Y. (1).
Gay, Martin, Boston, Mass. (1).
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10.) Born in 1816. Died Jan'y 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Gould, Augustus A., Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2). Born June 15, 1787. Died Oct. 24, 1859.
Graham, James D., Washington, D. C. (1). Born in 1799. Died Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in 1808. Died March 10, 1860.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D , Boston, Mass. (1).
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19). Born in 1822. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York, N. Y. (4). Born March 9, 1809. Died January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, S. S., Chickies, Pa. (1). Died Sept. 10, 1880.
Hale, Enoch, Boston, Mass. (1). Born Jan. 19, 1790. Died Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (11). Born Jan. 17, 1781. Died May 15, 1858.

Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1).
Hartt, Charles F., Ithaca, N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Born Jan. 4, 1816. Died May 23, 1874.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1).
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878.
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.
Hilgard, Theo. C., St. Louis, Mo. (17). Born Feb, 28, 1828. Died Mch. 5, 1875.
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1). Born May 24, 1793. Died Feb. 27, 1864.
Hodgson, W. B., Savannah, Ga. (10). Born 1815.
Holbrook, John E., Charleston, S. C. (1). Born Dec. 31, 1796. Died Sept. 8, 1871.
Hopkins, Albert, Williamstown, Mass. (19).
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.
Hopkins, Wm., Lima, N. Y. (5.) Died in March, 1867.
Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Houghton, Douglas, Detroit, Mich. (1). Born Sept. 21, 1809. Died Oct. 13, 1845.
Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, Edward B., Washington, D. C. (2). Born in 1822. Died Oct. 2, 1863.
Hunt, Freeman, New York, N. Y. (11). Born March 21, 1804. Died March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles T., Boston, Mass. (1). Died in Aug. 1880.
Johnson, Walter R., Washington, D. C. (1). Born June 21, 1794. Died April 26, 1852.
Jones, Catesby A. R., Washington, D. C. (8).
Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).
Keely, G. W., Waterville, Me. (1).
Keep, N. C., Boston, Mass. (13). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12).
King, Mitchell, Charleston, S. C. (3). Born June 8, 1783. Died in 1862.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died Sept. 14, 1875.
LaRoche, R., Philadelphia, Pa. (12).
Lasel, Edward, Williamstown, Mass. (1).
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (3). Born Sept. 8, 1830. Died in military service June 27, 1862.
Lincklaen, Ledyard, Cazenovia, N. Y. (1).
Linsley, James H., Stafford, Conn. (1). Born May 5, 1787. Died Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.
Logan, William E., Montreal, Canada (1). Born April 23, 1798. Died in June, 1875.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June 24, 1872.

Maack, G. A., Cambridge, Mass. (18).
Mahan, Dennis H., West Point, N. Y. (9). Born April 2, 1802. Died Sept. 16, 1871.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10).
Mather, William W., Columbus, Ohio (1). Born May 24, 1804. Died Feb. 27, 1859.
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
McMahon, Mathew, Albany, N. Y. (11).
M'Conihe, Isaac, Troy, N. Y. (5).
Meade, George G., Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 31, 1829. Died Nov. 9, 1879.

Minifie, William, Baltimore, Md. (12). Born in 1805. Died Oct. 24, 1880.
Mitchel, O. M., Cincinnati, Ohio (3). Born Aug. 28, 1810. Died Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (3). Died in 1869.
Mitchell, Wm. H., Florence, Ala. (17).
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel G., Philadelphia, Pa. (1). Born Jan. 26, 1799. Died May 15, 1851.
Mudge, Benjamin F., Manhattan, Kansas (25). Born Aug. 11, 1817. Died Nov. 21, 1879, aged 62 years.
Munroe, Nathan, Bradford, Mass. (6). Born May 16, 1804. Died July 8, 1866.
Munroe, William, Concord, Mass. (18). Died April 27, 1877.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, C. A., Providence, R. I. (17). Died in 1877.
Nicholson, Thomas, New Orleans, La. (21).
Nicollett, Jean N., Washington, D. C. (1). Born July 24, 1786. Died Sept. 11, 1843.
Norton, John P., New Haven, Conn. (1). Born in 1822. Died Sept. 5, 1852.
Noyes, J. O., New Orleans, La. (21).
Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.
Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.
Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.
Olmsted, Alexander F., New Haven, Conn. (4).
Olmsted, Denison, New Haven, Conn. (1). Born June 18, 1791. Died May 13, 1859.
Olmsted, Denison, jr., New Haven, Conn. (1).
Otis, George Alexander, Washington, D. C. (10). Born Nov. 12, 1830. Died Feb. 23, 1881.

Painter, Jacob, Lima, Pa. (23). Died in 1876.
Painter, Minshall, Lima, Pa. (7).
Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.
Parkman, Samuel, Boston, Mass. (1).
Peirce, Benjamin, Cambridge, Mass. (1). Born April 4, 1809. Died Oct. 6, 1880.
Perkins, George R., Utica, N. Y. (1). Died Aug. 22, 1876. Born May 3, 1812.

Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.
Perry, John B., Cambridge, Mass. (16).
Perry, M. C., New York, N. Y. (10).
Piggot, A. Snowden, Baltimore, Md. (10).
Plumb, Ovid, Salisbury, Conn. (9).
Pope, Charles A., St. Louis, Mo. (12). Born May 15, 1818. Died July 6. 1870.
Porter, John A., New Haven, Conn. (14). Born March 15, 1822. Died Aug. 25, 1866.
Pourtalès, Louis François de, Cambridge, Mass. (1). Died July 18, 1880, in the 58th year of his age.
Pruyn, John V. L., Albany, N. Y. (1). Born in 1806. Died Nov. 22, 1877.
Pugh, Evan, Centre Co., Pa. (14).
Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.

Read, Ezra, Terra Haute, Ind. (20). Died in 1877.
Redfield, William C., New York, N. Y. (1). Born March 26, 1789. Died Feb. 12, 1857.
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10). Born August 27, 1803. Died February 10, 1861.
Rogers, Henry D., Glasgow, Scotland (1). Born 1809. Died May 29, 1866.
Rogers, James B., Philadelphia, Pa. (1). Born February 22, 1803. Died June 15, 1852.

Sager, Abram, Ann Arbor, Mich. (6). Born December 22, 1810. Died August 6, 1877.
Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Senter, Harvey S., Aledo, Ill. (20). Died in 1875.
Seward, William H., Auburn, N. Y. (1). Born May 16, 1801. Died Oct. 10, 1872.
Sherwin, Thomas, Dedham, Mass. (11). Born March 26, 1799. Died July 23, 1869.
Silliman, Benjamin, New Haven, Conn. (1). Born August 8, 1779. Died November 24, 1864.
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12).
Smith, James Y., Providence, R. I. (9). Born September 15, 1809. Died in 1876.
Smith, J. V., Cincinnati, Ohio (5).

Smith, Lyndon A., Newark, N. J. (9). Born November 11, 1795. Died December 15, 1865.
Snell, Ebenezer S., Amherst, Mass. (2). Born October 7, 1801. Died in September, 1877.
Sparks, Jared, Cambridge, Mass. (2). Born May 10, 1789. Died March 14, 1866.
Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.
Stone, Samuel, Chicago, Ill. (17). Died in 1876.
Sullivant, Wm. S., Columbus, Ohio (7). Born Jan. 15, 1803. Died April 30, 1873.
Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1).
Taylor, Richard C., Philadelphia, Pa. (1). Born January 18, 1789. Died November 26, 1851.
Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.
Teschemacher, J. E., Boston, Mass. (1).
Thompson, Alexander, Aurora, N. Y. (6).
Thompson, Zadock, Burlington, Vt. (1). Born May 23, 1796. Died Jan. 19, 1856.
Thurber, Isaac, Providence, R. I. (9).
Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York, N. Y. (1). Born Aug. 15, 1796, Died Mch 10, 1873.
Torrey, Joseph, Burlington, Vt. (2). Born Feb. 2, 1797. Died Nov. 26, 1867.
Totten, Joseph G., Washington, D. C. (1). Born August 23, 1788. Died April 22, 1864.
Townsend, Howard, Albany, N. Y. (10). Died in 1867.
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9). Died in 1866.
Troost, Gerard, Nashville, Tenn. (1). Born March 15, 1776. Died Aug. 14, 1850.
Tuomey, Michael, Tuscaloosa, Ala. (1). Born September 29, 1805. Died March 20, 1857.
Tyler, Edward R., New Haven, Conn. (1).

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).

Wadsworth, James S., Genesee, N. Y. (2). Born October 30, 1807. Died May 8, 1864.

Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.
Walker, Timothy, Cincinnati, Ohio (4). Born Dec. 1, 1802. Died Jan. 15, 1856.
Walsh, Benjamin D., Rock Island, Ill. (17).
Wanzer, Ira, Brookfield, Conn. (18). Born April 17, 1796. Died March 5, 1879.
Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.
Watertown, Charles, Wakefield, Eng. (1).
Webster, H. B., Albany, N. Y. (1).
Webster, J. W., Cambridge, Mass. (1).
Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.
Weyman, G. W., Pittsburg, Pa. (6). Died in 1864.
Wheatland, Richard H., Salem, Mass. (13).
Wheeler, Arthur W., Baltimore, Md. (29). Born March, 1859. Died Jan. 6, 1881.
White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.
Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.
Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.
Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.
Willard, Emma, Troy, N. Y. (15). Born Feb. 23, 1787. Died April 15, 1870.
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5). Born Feb. 6, 1826. Died June 11, 1875.
Woodbury, Levi, Portsmouth, N. H. (1). Born Dec. 22, 1789. Died Sept. 4, 1851.
Woodman, John S., Hanover, N. H. (11). Born in 1819. Died May 15, 1871.
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.
Young, Ira, Hanover, N. H. (7).

ADDRESS

OF

PROFESSOR GEORGE F. BARKER,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

SOME MODERN ASPECTS OF THE LIFE-QUESTION.

THE NUMBER OF ROOTS IN OUR EQUATION OF LIFE INCREASES THE DIFFICULTY OF SOLVING IT, BUT BY NO MEANS PERMITS THE ACCEPTANCE OF THE LAZY ASSUMPTION THAT IT IS ALTOGETHER INSOLUBLE OR REDUCES A SAGACIOUS GUESS TO THE LEVEL OF THE PROPHECY OF A QUACK. — *Haughton.*

LADIES AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

THE discovery of new truth is the grand object of scientific work. The exultation of feeling which comes from the possession of a fact, which, now, for the first time, he makes known to men, must ever be the reward of the scientific worker. As investigators and as students of science we are met here to-day at this our annual session. Each of us during the past year has been endeavoring to push outward further into the unknown, the boundary of present knowledge. When, therefore, we thus meet together it is fitting that, from time to time, our attention should be called to the progress which has been made along some one of the various lines of research, and to the milestones which mark the epochs of advance along the way which science has travelled. Moreover, we may profitably sum up at such times the work done in particular directions, and encourage ourselves with pro-

spective and retrospective glances. In these summings up, however, a difficulty arises. The range of modern scientific thought includes an immense area. The field of knowledge is already so vast, that, seen from the vertical distance necessary to make a wide survey, that small portion of it which is familiar to any one individual is scarcely visible. In consequence, to use a mechanical figure, the solid contents of a man's acquirements being given, the depth thereof is inversely as the area covered. He, therefore, who undertakes to speak even for one single department of science distributes his stock of knowledge over so broad a surface that in places it must become dangerously thin. It is, therefore, with a very keen sense of the temerity involved in the undertaking, that I ask your attention during the hour allotted me, to some points which appear to me to have been recently gained in the discussion of the question of life.

My friend and predecessor, Professor Marsh, opened his excellent address at Saratoga with the question "What is Life?" In a somewhat different sense I too ask the same question. But I fear it is only to echo his reply, "the answer is not yet." The result, however, cannot long be doubtful. "A thousand earnest seekers after truth seem to be slowly approaching a solution." And though the *ignis fatuus* of life still dances over the bogs of our misty knowledge, yet its true character cannot finally elude our investigation. The progress already made has hemmed it in on every side; and the province within which exclusively vital acts are now performed, narrows with each year of scientific research.

What now are we to understand by the word "Life" in this discussion? A noteworthy parallel is disclosed in the progress of human knowledge between the ideas of life and of force.[1] Both conceptions have advanced, though not with equal rapidity, from a stage of complete separability from matter to one of complete inseparability. Life is now universally regarded as a phenomenon of matter, and hence of course, as having no separate existence. But there still exists a certain vagueness in the meaning of the term "life." Two distinct senses of this word are in use; the one metaphysical, the other physiological. The former, synonymous with mind and soul, at least in the higher animals, has been evolved from human consciousness; the latter has arisen from a more or less careful investigation of the phenomena of living beings. It need scarcely be said that it is in the sense last men-

tioned that the word "life" is used in science.[2] The conception represents simply the sum of the phenomena exhibited by a living being.

Moreover, the progress which has been made in the solution of the life-question has been gained chiefly by investigation of special functions. But the functions of a vital organism are themselves vital. What then is the meaning of "vital" as applied to a function? Fortunately the answer is not difficult. "Life," says Küss, the distinguished Strasbourg physiologist, "is all that cannot be explained by chemistry or physics."[3] Guided by such a definition the work of the physiological investigator is simple. He has only to test each separate operation which he finds going on in the organism and to declare whether it be chemical or physical. If it be either, then since each function is non-vital, the entire organism must be non-vital also. Hundreds of able investigators, provided with the most effective appliances of research, are now in full cry after the life principle. Naturally, a vast amount of collateral knowledge is accumulated in the process. The quantitative as well as the qualitative relations of things are fixed and many important facts are collected.

With the object in view thus clearly defined, we are not surprised that great progress has been made. A vital process, like the catalytic ones of the older chemistry, was found by such research to be simply a process which, for want of sufficient investigation, is not yet understood.[4] While therefore, undoubtedly, much work yet remains to be done in the realm still called vital, the prophetic vision is already bright which will witness the last traces of inexplicable phenomena vanish and the words expressing them relegated to the limbo of the obsolete.

As a first result of recent work, the living organism has been brought absolutely within the action of the law of the Conservation of Energy. Whether it be plant or animal, the whole of its energy must come from without itself, being either absorbed directly or stored up in the food.[5] An animal like a machine, only transforms its energy. Lavoisier's guinea-pig placed in the calorimeter gave as accurate a heat-return for the energy it had absorbed in its food, as any thermic engine would have done.[6] But the parallel goes further. The mechanical work of an engine is measured by the loss of its heat and not of its substance. So the mechanical or intellectual work of a living being is measured

by the amount of carbon rather than the amount of nitrogen which is excreted.[7] The energy evolved daily by the human body would raise it to a height of about six miles.[8]

But beside heat, work may be the outcome of the organism; and this through the agency of the muscles. Their absolute obedience to mechanical law in their mode of action has been admirably established by Haughton.[9] The work a muscle does, it does in contracting. It is to the mechanism of muscle-contraction that we are indebted for another illustration of our subject.

When work is done by a muscle in contracting, three changes are observed to take place normally in its tissue. First, there is a loss of its electric tension; second, there is an evolution of heat in it; and third, carbon dioxide appears there, and its reaction, before neutral, becomes acid.[10]

Matteucci was the first to observe and to call attention to the remarkable similarity in structure and in the mechanism of operation, between striated muscular fibre and the electric organ of certain fishes.[11] Recently, Marey has repeated and extended his observations.[12] In structure, the electric organ is made up, like the muscle, of columnar masses each separable transversely into vesicular sections. In a torpedo weighing seventy-three pounds, there were 1182 of these columns in each of the two organs, with 150 sections, on an average, in each column.[13] In the muscles which bend the fore-arm, there are 798,000 fibrillæ.[14] As to the mechanism, alike in muscle and in electric organ, an electric current stimulates action on opening and on closing the circuit, but not when it is flowing; the same phenomena take place in both with the direct and with the inverse current; both are reflex; stimulation of the electric nerve produces discharge, as that of the motor nerve causes muscular shock; an entire paralysis follows nerve-section; curare paralyzes both; and tetanus results in both from rapid currents or from strychnine.[15]

Still more striking analogies are furnished by the investigation of the susurrus or muscular sound, first noticed in 1809 by Wollaston.[16] This sound is produced by all muscles when in the state of contraction, the pitch of the note being not far from thirty vibrations per second. It is evidently only the intermittent contraction of the muscular fibre. A single excitation produces a muscular shock. As this production requires from eight to ten hundredths of a second, it is evident that if another stimulus be

applied before the first has disappeared the two will coalesce; and when twenty per second reach the muscle it becomes permanently contracted or tetanized.[17] By means of a very sensitive myograph, Marey has found that in voluntary contraction the motor nerves are the seats of successive acts, each of which produces an excitation of the muscle. In 1877, Marey examined similarly the discharge of the torpedo and found a most complete correspondence between it and muscular contraction.[18] Since electric tension disappears from a muscle during contraction, does not the analogy suggest strongly that, like the discharge of the electric organ of the torpedo, muscular contraction is an electrical phenomenon?[19]

Granting electric discharge to be a necessary concomitant of muscle-contraction, what is the origin of this electricity? That it is not carried to the muscle by the nerves follows from the fact that a muscle will still contract when deprived of all its nerve-fibres, or when these are paralyzed by curare.[20] It must therefore be generated within the muscle itself.[21] To reach a solution of the problem we must obviously follow the analogies of its production elsewhere.

Perhaps no single question in physics has been more keenly discussed than this one of the origin of electric charge. The memorable conflict between Galvani and Volta, between animal electricity and the electricity of metallic contact, succeeded by the even more triumphant overthrow of the latter and the establishment ultimately by Faraday, of the electro-chemical theory; these are facts fresh in all our memories. The justice of time however in this case, if it has been tardy, has been none the less sure. The experiments of Thomson have vindicated Volta and established the contact theory as a *vera causa*. And more curiously still, it now appears to be proved that both contact and chemical action underlie the production of that very animal electricity so stoutly battled for by Galvani and his associates.

Volta's experiments to prove that a difference of potential is developed by the contact of two heterogeneous metals were not crucial. But Thomson, repeating them with the aid of more delicate apparatus, has shown that whenever copper and zinc are brought in contact, the copper becomes negative to the zinc. In proof that the chemical action of atmospheric moisture was not the cause of the phenomenon, he showed that when a drop of water served to connect the copper and the zinc, no charge at all

was produced.[22] The fact may therefore be regarded as established, as the result of numerous and varied experiments, that a difference of electrical potential is always developed at the surfaces of contact of heterogeneous media. Not only is this true of solids in contact with solids, but also of solids in contact with liquids, and of liquids in contact with each other.[23] Of course the production of electricity by contact must result from a loss of energy elsewhere. In the opinion of Cumming, it is the loss of energy which is owing to the unsymmetrical swinging of the molecules on the two sides of the surfaces of contact, which reappears as difference of potential between the solids or as the energy of electrical separation.[24]

But we may carry the sequence yet another step backward. The energy which is thus lost at the surfaces of separation must be heat, and this junction must be cooled thereby. Thus the production of thermo-electricity is seen only to be a special case of a general law, a view to which the well-known Peltier effect gives support. In this phenomenon, when two metals are joined together in the form of a ring and one junction is heated, a current is produced which cools the other junction.[25] From a study of these conditions, Thomson has concluded that the absorption of heat in a thermo-electric circuit varies for different metals with the direction of the current. Thus in iron, the current from hot to cold absorbs heat, while in copper the current which absorbs heat is from cold to hot.[26] In entire accordance with these results, are the conclusions recently reached by Hoorweg. Whenever two conductors come into contact, motion of heat results in the development of electricity, the current produced existing at the cost of heat at one part of the point of contact, and evolving heat at the other for a result. Hence all voltaic currents are thermo-currents.[27]

To return to the muscle, it must now be apparent that the electrical charge which appears in its fibre may have its origin in so purely a physical cause as the contact of the heterogeneous substances of which the tissue is built up; the maintenance of this charge being effected by chemical changes going on constantly in the substance of the muscle, by which the carbon dioxide is produced, which is shown to be a measure of the work done.[28]

Conceding then, that a muscular contraction is conditioned upon an electric discharge, by what mechanism is the contraction effected? Prevost and Dumas supposed each particle of a

muscular fibril to be magnetic. Such a row of particles would indeed attract each other when magnetized and shorten the length of the whole fibril. But the force of contraction would increase as the length diminished; whereas the fact in the case of the muscle is precisely the reverse.[29] Hence Matteucci supposed that electrification of the muscular fibre produces a repulsion between its elements, the subsequent return of this fibre, in virtue of its proper elasticity,[30] constituting the muscular contraction. Radcliffe's theory is of more recent date and is somewhat analogous. He maintains that each fibre of the muscle, together with its sheath, constitutes a condenser. When charged, the attraction of the two electricities compresses the fibre laterally and thus elongates it. When discharge takes place the normal elasticity of the fibre produces the contraction.[31] Assuming, however, that the electrical phenomena take place during the latent period only, other theories of muscular contraction regard these phenomena as simply preparatory, the contraction itself being mechanical. Thus Marey[32] likens the muscular fibre to a string of India rubber which, when stretched, contracts upon warming it; thus transforming heat directly into work. Rouget contends that the muscular fibre is a true spiral spring, which, actively distended during the repose of the muscle, returns upon itself at the moment of contraction; muscular contractility being thus a purely physical property of elasticity. Engelmann, observing that during contraction changes take place in the doubly-refracting power of the alternate disks of the fibril, supposes muscular contraction to result from a passage of water from the isotropic layers into the anisotropic or doubly-refracting ones; thus osmotically increasing the volume of the latter. By this means the ellipsoidal doubly-refracting particles are converted into spherical ones; and since the longer axes of the ellipsoids are parallel to the length of the fibre, the muscle is thereby shortened.[33]

From this brief review, does it not seem probable that the phenomenon of muscular contraction may be satisfactorily accounted for without the assumption of "vital irritability," so long invoked?[34] May it not be conceded that the theory that muscular force has a purely physical origin is at least as probable as the vital theory?

Time would fail me to discuss the many other phenomena of the living body which have been found on investigation to be non-

vital. Digestion, which Prout said it was impossible to believe was chemical,[35] is now known to take place without the body as well as within it, and to result from non-vital ferments.[36] Absorption is osmotic and its selective power resides in the structure of the membrane and the diffusibility of the solution.[37] Respiration is a purely chemical function. Oxyhæmoglobin is formed wherever hæmoglobin and oxygen come in contact and the carbon dioxide of the serum exchanges with the oxygen of the air according to the law of gaseous diffusion.[38] Circulation is the result of muscular effort both in the heart and the capillaries, and the flow which takes place is a simple hydraulic operation.[39] Even coagulation, so tenaciously regarded as a vital process, has been shown to be purely chemical. Upon the hypothesis of Schmidt it results from the union of two proteids, fibrinogen and fibrinoplastic substance.[40] According to the later theory of Hammarsten, fibrin is produced from fibrinogen by the action of a special ferment.[41]

There is another function which should by no means be omitted from our consideration. This function is that of the nervous system. In structure, this system is well known to us all. In composition, it is made up essentially of a single substance, discovered by Liebreich[42] and called protagon, the specific characters of which have lately been confirmed by Gamgee.[43] In function, the nerve-cell and the nerve fibre are occupied solely in the production, the reception and the transmission of energy, which is believed to be electrical. There is evidently a close analogy between the nerve and the muscle, the nerve-fibre like the fibrilla being composed of cells, and having a positive electric charge upon the exterior surface the tension of which is one-tenth of a volt.[44] Indeed a piece of nerve removed from the body exhibits nearly the same electric phenomena as a piece of muscle. Haughton attributes *tinnitus aurium* to the vibration of nerve cells.[45]

The main objection raised to the electrical character of nerve-energy is based upon its slow propagation.[46] Though thirty-six years ago Johannes Müller predicted[47] that the velocity of nerve transmission never could be measured, yet Helmholtz accomplished the feat very soon afterward.[48] His results, like those of subsequent experimenters, show that the velocity of propagation of the nervous influence along a nerve, like that of electric transmission, is only about 26 to 29 metres in a second.[49] But it should be borne in mind, as Lovering has pointed out, that electricity has no

velocity, in any proper sense.[50] That since the appearance of an electrical disturbance at the end of a conductor depends upon the production of a charge, the time of this appearance will be a joint function of the electrostatic capacity of the conductor and of its resistance. Since each of these values is directly proportional to length, it follows that the time of transmission will vary as the square of the length of the conductor. While therefore, in Wheatstone's experiment, he found that electricity required rather more than one-millionth of a second to pass through one-quarter of a mile of wire, it does not follow that it would traverse 288,000 miles in one second, as he assumed. Indeed, as Lovering has shown, its actual velocity would be only 268 miles in an entire second. Hence the marvellous discrepancies which have been observed in the results of experiments made to determine the velocity of electricity on long wires are explained.[51]

In the nerve itself, therefore, the velocity of transmission may be supposed to be the less as its resistance is greater. Now, Weber has shown that animal tissues in general have a conductivity only one fifty-millionth of that of copper.[52] And Radcliffe found that a single inch of the sciatic nerve of a frog measured 40,000 ohms; a resistance eight times that of the entire Atlantic cable.[53] In experimenting to confirm the above law of velocity, Gaugain measured the time of transmission of the electric current through a cotton thread 1.65 metres long and found it to be eleven seconds. Two similar threads placed consecutively, thus forming a conductor twice as long, required forty-four seconds for the passage of the current; or four times as long. From these data the apparent velocity in the short thread is at the rate of only 0.15 metre in one second; and in the long one only about half this rate, of course.[54] The real velocity in both cases is the same, 0.5 metre per second. Hence the fact that the energy of nerve moves at the rate of only 28 metres per second is really no proof that it is not electricity.[55]

But even if it shall appear that the nervous energy is not electrical, the argument is not weakened, since on no theory yet proposed is it assumed to be vital. Du Bois Reymond, while maintaining that "it would be rash entirely to dismiss the notion of electricity being concerned, and even playing an important part, in the internal mechanism of the nerves," yet, assuming its low velocity to be fatal to its electrical character, suggests that

the nervous agent "is some internal motion, perhaps even some chemical change of the substance itself, contained in the nerve-tubes, spreading along the tubes both ways from any point where the equilibrium has been disturbed." And Spencer has advanced the view that "a nervous disturbance travels as a wave of molecular change;" that "a nervous discharge is a wave of isomeric transformation."[56]

The higher functions of the nerve-cell, those connected with mental processes, is a field too vast to be entered at this time. The double telegraph line of nerve, motor and sensor in their effect, but, as is now conceded,[57] precisely alike in function, are the avenues of ingress and egress. In the acts denominated reflex, the stimulus reaches the spinal cord only and the action is automatic and unconscious. Should the impression ascend higher to the sensory ganglia, the action is now conscious though none the less automatic. Finally, should deliberation be required before acting, the message is sent to the hemispheres by the sensory ganglia and will operates to produce the act. Based on principles already established by investigation, a true psychology is coming into being, developed by Bain, Maudsley, Spencer and others.[58] A physiological classification of mental operations is being formed which uses the terms of metaphysical psychology, but in a more clearly defined sense.[59] Emotion, in this new science, is the sensibility of the vesicular neurin to ideas. Memory, the registration of stimuli by nutrition. Reflection is the reflex action of the cells in their relations in the cerebral ganglia. Attention is the arrest of the transformation of energy for a moment. Ratiocination is the balancing of one energy against another. Will is the reaction of impressions outward. And so on through the list.

Among the physical aspects of the mind-question, the problem of the quantitative changes which take place in the organism is a very curious and interesting one. That the energy of the brain comes from the food will be disputed by no one in these days. Hence, the brain must act like a machine and transform energy. There is then a purely physiological representation of mental action, concerned with forces which are known and measurable. The researches of Lombard long ago showed the concomitant heat of mental action.[60] Recent researches are equally interesting, which show that mental operations are not instantaneous but require a distinct time for their performance. By accurate chronographic

measurement, Hirsch and Donders have shown that an irritation on the head is answered by a signal with the hand only after one-seventh of a second; that a sound on the ear is indicated by the hand in one-sixth of a second; and that when light irritates the eye, one-fifth of a second elapses before the hand moves.[61] The mechanism of such a process is the following:[62] Suppose the sound "A" is heard by the ear. After a latent period it is translated to some nerve cells and hence to the brain. From the brain it goes to other cells, ganglion cells, and to other nerves, and then to the different muscles of the chest and larynx, and then follows the audible response "A." Now since this whole process requires only one-sixth of a second, the question arises, how much of it is psychical. To answer it, the experiment is repeated but with this difference, that the particular sound to be used is unknown to the experimenter. Before the sound can be repeated by him therefore, a distinct act of discrimination is required, and the time taken is longer. Calling the time in the first experiment *a*, and in the second *b*, the difference *b*—*a* is the time required for two distinct actions: one, that of distinguishing the sound, and the other, that of willing the corresponding movement. If now it be agreed that only the sound "A" shall be responded to when called, these may be separated since no other sound being responded to the latter action is eliminated. If the time now required be called *c*, the difference *c*—*a* represents the time required for forming a judgment, and *c*—*b* the time required for a volition. In making these measurements, Donders used an instrument devised by him, called a noëmotachograph, and also a modification of it called a noëmotachometer.[63] By these instruments different points of the body can be irritated, different sounds can be produced, and different colors or letters can be shown, all by the electric spark. By subtracting the simple physiological time from the time given in any experiment, the time necessary for recognition may be obtained. By an addition to the apparatus, a second stimulus may be made to follow the first, either on the same or on a different sense; thus enabling the time necessary for a simple thought to be determined. As a result of his experiments, Donders found that the value *b*—*a* in the case of a simple dilemma was seventy-five thousandths of a second, this being the time required for recognition and subsequent volition. In the same way *c*—*a* has been shown to be forty-thousandths of a

second, being the time required for simple recognition; there is left thirty-five thousandths of a second as the time required for volition. Moreover, by independent measurement with the noëmotachometer, exactly the same time, one twenty-fifth of a second, is found necessary to enable a judgment to be formed about the priority of two impulses acting on the same sense. If they act on different senses, more time is necessary. So also more time is required to recognize a letter by seeing its form than by hearing its sound. A man of middle age then, thinking not so very quickly, requires one twenty-fifth of a second for a simple thought.

Another important fact concerning nervous action is that its amount may be measured by the quantity of blood consumed in its performance. Dr. Mosso of Turin has devised an apparatus called the Plethysmograph — drawings of which were exhibited at the London Apparatus Exhibition of 1876 — designed for measuring the volume of an organ.[64] The fore-arm, for example, being the organ to be experimented on, is placed in a cylinder of water and tightly enclosed. A rubber tube connects the interior of the cylinder with the recording apparatus. With the electric circuit by which the stimulus was applied to produce contraction, were two keys, one of which was a dummy. It was noticed that, after using the active key several times, producing varying current strengths, the curve sank as before on pressing down the inactive key. Since no real effect was produced, the result was caused solely by the imagination, blood passing from the body to the brain in the act. To test further the effect of mental action, Dr. Pagliani, whose arm was in the apparatus, was requested to multiply 267 by 8, mentally, and to make a sign when he had finished. The recorded curve showed very distinctly how much more blood the brain took to perform the operation. Hence the plethysmograph is capable of measuring the relative amount of mental power required by different persons to work out the same mental problem. Indeed Mr. Gaskell suggests the use of this instrument in the examination room, to find out, in addition to the amount of knowledge a man possesses, how much effort it causes him to produce any particular result of brain-work. Dr. Mosso relates that while the apparatus was set up in his room in Turin, a classical man came in to see him. He looked very contemptuously upon it and asked of what use it could be, saying that it couldn't do anybody any good. Dr. Mosso replied, "Well now, I can tell you

by that whether you can read Greek as easily as you can Latin." As the classicist would not believe it, his own arm was put into the apparatus and he was given a Latin book to read. A very slight sinking of the curve was the result. The Latin book was then taken away and a Greek book was given him. This produced immediately, a much deeper curve. He had asserted before that it was quite as easy for him to read Greek as Latin and that there was no difficulty in doing either. Dr. Mosso, however, was able to show him that he was laboring under a delusion. Again, this apparatus is so sensitive as to be useful for ascertaining how much a person is dreaming. When Dr. Pagliani went to sleep in the apparatus, the effect upon the resulting curve was very marked indeed. He said afterward that he had been in a sound sleep and remembered nothing of what passed in the room—that he had been absolutely unconscious; and yet, every little movement in the room, such as the slamming of a door, the barking of a dog, and even the knocking down of a bit of glass, were all marked on the curves. Sometimes he moved his lips and gave other evidences that he was dreaming; they were all recorded on the curve, the amount of blood required for dreaming diminishing that in the extremities. The emotions too left a record. When only a student came into the room, little or no effect appeared in the curve. But when Professor Ludwig himself came in, the arteries in the arm of the person in the apparatus contracted quite as strongly as upon a very decided electrical stimulation.

In an address of the retiring President of this Association, delivered but a few years ago, I find this sentence: "Thought cannot be a physical force, because thought admits of no measure."[65] In the light of the rapid advances lately made in investigating mental action, we see that in two directions at least, in its rate of action and of its relative energy, we may already measure thought, as we measure any other form of energy, by the effects it produces.

Passing now to the consideration of the general question of the transformation of energy which is effected by living beings, attention may be called to one or two points in general physics, as bearing upon its solution. The great law of the dissipation of energy, as modified by Thomson from the statement of Clausius, is thus stated: "The entropy of the universe tends to zero."[66] In other words, the energy of the universe available for transmutation

is approaching extinction. This conclusion is based upon the fact that while every form of energy can be completely converted into heat, heat cannot be completely converted into other forms of energy, nor these into each other. Hence it arises that energy is being gradually dissipated as heat. Moreover, since transformation can only result when heat passes from a higher to a lower temperature, it follows that when that perfect equilibrium of temperature is reached toward which events are tending, there can be no other energy than heat; and this absolutely inconvertible, irrecoverable. To apply this law to the present case, the muscle, for example, is a machine for transforming the energy of food into work. Since, consequently, this conversion is not complete, it follows that heat must appear as a necessary result of muscular action. The heat of animal life, consequently, is not heat especially provided; it is simply the heat which inevitably results from an incomplete conversion of energy.

Again, the form of chemical action generally assumed by physiologists to account for the energy of the living animal has been chemical union, oxidation.[67] But the science of thermo-chemistry, as developed in late years by Berthelot and Thomsen,[68] has proved that direct union of chemical substances may not only not evolve heat, but may actually absorb it. It appears, too, that thermal changes accompany all forms of chemical change, those of decomposition and exchange as well as those of synthesis. The animal absorbs highly complex substances as food, capable of innumerable stages of retrogressive metamorphosis before elimination. In each of these stages energy may be evolved, this energy being that successively stored up by the plant when these changes were repeated in the inverse order.

Another point of interest has reference to the modern views of capillarity. In 1834, J. W. Draper showed that capillarity is an electrical phenomenon.[69] Quite recently, Lippmann has developed and extended this view and fully confirmed it.[70] Whenever the free surface of a liquid, curved by capillary action, is electrified it changes its form; and conversely, when such a surface is made by mechanical means to change its form, an electromotive force is developed. Based upon this principle Lippmann constructed a capillary reversible engine and an extremely sensitive capillary electrometer. The former, when a current of electricity was applied to it, developed mechanical work and ran as a motor.

When turned by hand, it became an electromotor. In the animal organism there are it is true but a few free surfaces where this action can take place. But Gore has shown that the same phenomenon appears between two liquids in contact, their boundary being altered in character by electrification.[71] Indeed, when we consider the production of electricity by osmose, and of heat and electricity both, by imbibition, both capillary phenomena,[72] the wonder is not that so much electrical energy is evolved by the organism, but that it is so little. If the physical and chemical changes which take place within the body took place without it, there would be an abundant evolution of energy. Can we doubt that these changes are the cause of the energy exhibited by the organism?

Thus far, when we have spoken of a living being, we have had reference to the organism as a whole, and this of a rather complex kind. In this view of the case, however, we find that biological microscopists do not agree with us. "The cell alone," says Küss, "is the essentially vital element."[73] Says Beale,—"There is in living matter nothing which can be called a mechanism, nothing in which structure can be discerned. A little transparent colorless material is the seat of these marvellous powers or properties by which the form, structure and function of the tissues and organs of all living things are determined."[74] And again, "However much organisms and their tissues in their fully formed state may vary as regards the character, properties and composition of the formed material, all were first in the condition of clear, transparent, structureless, formless living matter."[75] So Ranvier:[76] "Cellular elements possess all the essential vital properties of the complete organism." And Allman, in his address as President of the British Association last year, is still more explicit.[77] "Every living being," he says, "has protoplasm as the essential matter of every living element of its structure." "No one who contemplates this spontaneously moving matter can deny that it is alive. Liquid as it is, it is a living liquid; organless and structureless as it is, it manifests the essential phenomena of life." "Coextensive with the whole of organic nature — every vital act being referable to some mode or property of protoplasm, it becomes to the biologist what the ether is to the physicist." From these quotations it would seem that even in the highest animal there is nothing living but protoplasm or germinal matter, "transparent,

colorless, and, as far as can be ascertained by examination with the highest powers of the microscope, perfectly structureless. It exhibits these same characters at every period of its existence." Neither the contractile tissue of the muscle, the axis-cylinder of the nerve, nor the secreting cell of the gland, is living, according to Beale.[78] Hence it would be fair to draw the inference that vital force should not be required to explain the phenomena of the non-living matter of the body, such as the contraction of the muscle or the function of the nerve. If this be conceded it is a great point gained; since the phenomenon of life becomes vastly simplified when we have to account for it only as exhibited in this one single form of living matter, protoplasm. In describing its properties, Allman includes this remarkable mobility, these spontaneous movements, and says that they result "from its proper irritability, its essential constitution as living matter." "From the facts there is but one legitimate conclusion, that life is a property of protoplasm." Beale, however, will not allow that life is "a property" of protoplasm. "It cannot be a property of matter," he says, "because it is in all respects essentially different in its actions from all acknowledged properties of matter."[79] But the properties of bodies are only the characters by which we differentiate them. Two bodies having the same properties would only be two portions of the same substance. Because life, therefore, is unlike other properties of matter, it by no means follows that it is not a property of matter.[80] No dictum is more absolute in science than the one which predicates properties upon constitution. To say that this property exhibited by protoplasm, marvellous and even unique though it be, is not a natural result of the constitution of the matter itself, but is due to an unknown entity, a *tertium quid*, which inhabits and controls it, is opposed to all scientific analogy and experience. To the statement of the vitalist that there is no evidence that life is a property of matter, we may reply with emphasis that there is not the slightest proof that it is not.

Chemistry tells us that complexity of composition involves complexity of properties. The grand progress which Organic Chemistry has made in recent times has been owing to the distinct recognition of the influence of structure upon properties. Isomerism is one of its most significant developments. The number of possible isomers increases enormously with the complexity of

the molecule. Granted that we now know several of the proteid group of substances:[81] how many thousands may there be yet to know? Bodies of such extreme complexity of constitution may well have an indefinite number of isomers. Not only does chemistry not say that there cannot be such a thing but she encourages the expectation that there will yet be found the precise proteid of which the phenomena of protoplasm are properties. The rapid march of recent organic synthesis makes it quite certain that every distinct chemical substance of the living body will ultimately be produced in the laboratory;[82] and this from inorganic materials. Given only the exact constitution of a compound, and its synthesis follows. When therefore, the chemist shall succeed in producing a substance constitutionally identical with the protoplasmic mass, there is every reason to expect that it will exhibit all the phenomena which characterize its life; and this equally whether protoplasm be a single substance or a mixture of several representative substances.

But here a word should be said concerning a remarkable physical condition assumed by matter in organized beings. Graham, in 1862, drew the sharp line which separates colloid from crystalloid matter.[83] His researches have proved, says Maudsley,[84] "the necessity of considerable modification in our usual conception of solid matter. Instead of the notion of inert impenetrable matter, we must substitute the idea of matter which in its colloidal state is penetrable, exhibits energy, and is widely susceptible to external agents; 'its existence being a continued metastasis.' This sort of energy is not a result of chemical action, for colloids are singularly inert in all ordinary chemical relations, but is a result of its unknown molecular constitution; and the undoubted existence of colloidal energy in organic substances, which are usually considered inert and called dead, may well warrant the belief of its larger and more essential operation in organic matter in the state of instability of composition in which it is when under the condition of life. Such energy would then suffice to account for the simple uniform movements of the homogeneous substance of which the lowest animal consists; and the absence of any differentiation of structure is a sufficient reason for the absence of any localization of function and of the general uniform reaction to different impressions." Graham himself says that the colloidal state may be looked upon "as the probable primary source of the force appearing in the phenomena

of vitality."[85] The colloidal condition is the dynamical state of matter; the crystalloidal the static. The former, which is the rule in the organic kingdom of nature, is the exception in the inorganic. Aluminum and ferric hydrates, silicic acid and a few other inorganic substances, exist in the colloid condition. From analogy there would seem to be but little doubt that the colloid state of these bodies differs from their crystalloid state merely in the size of the molecule. In other words opal, which is colloid silica, is a polymer of quartz. If this theory be true there can be no doubt of the vastly greater complexity of a colloidal proteid molecule than of a crystalloid one. Now it is a very significant fact, in this connection, that not a single organic colloid has ever been synthesized. Gelatin, which is one of the best examples of a colloid, has a comparatively simple structure. And, although Hunt suggested, many years ago, that gelatin was probably an amido-derivative of the sugar group,[86]—a theory subsequently partially confirmed by Gibbs—yet no inverse process has yet given us this substance. That matter in the crystalloid and the colloid forms may be chemically identical, differing only in the size of its molecule, may be quite possible. But it is also possible that the difference may be a physical one. To produce the colloid state from the crystalloid is by no means beyond the power of science. We qualify our previous statement then only so far as to say that when the chemist produces a body in the colloidal form, having the identical constitution of protoplasm, there is every reason to believe that it will have the vital properties of protoplasm.

The important question now arises whether, since the protoplasm of animals is identical with that of vegetables, and the latter is the food of the former, any energy whatever is stored up by the animal as such. That this identity exists would seem satisfactorily established. Though the protoplasm of vegetables is enclosed within a cellulose bag, it is, says Allman, only a closely imprisoned rhizopod.[87] In the Nitella, it shows all its characteristic irritability, and from Vaucheria it escapes to exhibit all its amœboid movements. Spores swim about by cilia or flagella, and the cell-division of the one kingdom is the same as that of the other. In plants, however, protoplasm seems to be associated with chlorophyll, whose function was for a long time supposed to be to decompose carbon dioxide under the influence of sunlight. But Draper in 1872, showed that this decomposition took place before the chlo-

rophyll was formed.[88] Recent researches suggest that the function of chlorophyll may be wholly protective.[89] The assimilative power of the protoplasm reaches its maximum in the orange and yellow rays. Now Bert has shown that the absorption band in the chlorophyll spectrum is in the exact position of this maximum.[90] Hence, Gautier believes that this substance acts as a regulator of plant respiration, the greater or less amount of luminous energy thus absorbed and transformed, being utilized by the protoplasm and stored up.[91] Growth and cell-division, however, are independent of orange light and hence of chlorophyll. In the higher plants, these functions are performed by a separate and deep-lying set of cells. But in the lower, the same cell discharges both functions, assimilation going on in it during the day, and growth chiefly at night.[92] Sachs had already proved that the maximum growth of plants takes place just before daylight and the minimum in the afternoon. This retarding action of sunlight upon growth is as curious as it is unexpected. It now appears that in orange light plants assimilate—absorb carbon dioxide and evolve oxygen—but do not grow and are not heliotropic; while in blue light they are strongly heliotropic but do not give off oxygen.[93] Chlorophyll, however, is not confined to vegetables; infusoria, hydras, and certain planarian worms are green from the presence of this substance, and Geddes has shown that such animals, placed in the sunlight, give off a gas which is more than half oxygen.[94] These cells, moreover, contain starch granules.[95]

A still more striking evidence of this intimate relationship has been developed by Darwin, in his researches upon insectivorous plants.[96] Not only do these plants possess a mechanism for capturing insects, but they secrete a gastric juice which digests them. Nägeli has shown the presence of pepsin in yeast cells,[97] and attention has lately been called by Wurtz and others to the juice of the *Carica papaya* which contains a pepsin-like substance capable of peptonizing fibrin completely.[98] Moreover, there is the closest similarity between diastase and ptyalin;[99] and the milk of the cow-tree, recently examined by Boussingault[100] and found to resemble cream closely in composition, shows the presence of emulsifying agents in the vegetable kingdom analogous to those found in the pancreas of the animal.

Another most curious proof of the identity of animal and vegetable protoplasm has been given by Claude Bernard, who has shown

that both are alike sensitive to the influence of anæsthetics.[101] A sensitive plant exposed to ether no longer closes its leaflets when touched. Assimilation and growth, as well as germination, are arrested by chloroform. The yeast plant when etherized no longer decomposes sugar to produce alcohol and carbon dioxide; while the inversive and non-vital ferment still acts to convert the cane-sugar into glucose; precisely as under these circumstances, the diastasic ferment converts the starch of the seed into sugar.[102] By arresting anæsthetically the process by which carbon dioxide is absorbed and oxygen evolved, the true respiratory process, being less affected, now appears; and Schutzenberger has proved that the fresh cells of the yeast plant breathe like an aquatic animal.[103]

It would seem then that the protoplasmic life of animals is identical with that of plants; a certain measure of destructive metamorphosis taking place in each, evolving energy and producing carbon dioxide and water. When, however, this function is examined quantitatively, its maximum is seen to be reached in the animal. While the assimilative function characterizes the plant, the destructive function distinguishes the animal. Hence it is the function of the plant to store up energy, to produce the highly complex protoplasm. This, consumed by the animal as his food, continues his existence as a living being, the energy gradually set free by its successive steps of retrogressive metamorphosis, appearing as the work which he performs. If this view be correct, it would follow that every individual substance found in the animal—save only those which result from degradation—must be found in the plant upon which it feeds, and this is the fact. The myosin which Kühne[104] has shown to be the distinctive proteid of muscle, Vines has found in the aleuron grains of the lupine and the castor oil plant, along with vitellin, the special proteid of the vitellus.[105] The researches of Weyl and Bischoff have proved that gluten is formed in the dough of wheat flour by the action of a ferment upon the globulin-substance or plant-myosin which it contains,[106] precisely as Hammarsten has shown fibrin is produced in the action of a similar ferment upon fibrinogen.[107] Not only this; Hoppe Seyler has extracted from maize the identical substance which has been shown by Liebreich to be the essential chemical constituent of nerve tissue, protagon.[108]

The evidence then would seem to be conclusive that, since the protoplasm of the animal is identical with that of the vegetable,

the former must be derived from the latter. Hence the animal itself, though perhaps reconstructing more or less of the protoplasm of its food, really synthesizes none of this material. No energy therefore is stored up by the animal as such. Its total protoplasmic energy exists already in its food, in which it was stored up originally by the plant. Two inferences seem naturally to follow from this conclusion: 1st, that all the properties of animal protoplasm, and of the animal organism of which it constitutes the essential part, may be studied in the protoplasm of the plant; and 2d, that hence the solution of the life-question in the Myxomycetes will solve the life-problem for the highest vertebrate.[109]

Another consideration which must not be left out of the account in any discussion of the life-question is the potent influence of environment. Ordinary examples of this influence pass before our eyes every day. Heat necessitates the germination of the seed, and light causes plant assimilation. Gravity obliges the root to grow downward and the stem to ascend. Certain sensations from without excite inevitably muscular contraction; and a ludicrous idea may provoke laughter in defiance of the will. Epidemic and epizootic diseases show the dependence of function upon external conditions, and the germ theory illustrates the utter disproportionality of the cause to the effect.[110] The remarkable similarity in the periodicity observed between sunspots and the weather has been extended to include the appearance of locusts and the advent of the plague.[111] Even the body politic feels its influence, Jevons having established a coincident periodicity for commercial crises.[112]

The modern theory of energy, however, puts this influence in a still stronger light. As defined hitherto, energy is either motion or position; is kinetic or potential.[113] Energy of position derives its value obviously from the fact that in virtue of attraction it may become energy of motion. But attraction implies action at a distance; and action at a distance implies that matter may act where it is not. This of course is impossible; and hence action at a distance, and with it attraction and potential energy, are disappearing from the language of science. But what conception is it which is taking its place? By what action does the sun hold our earth in its orbit? The answer is to be found in the properties of the ether which fills all space. The existence of this ether,

the phenomena of light and electricity abundantly prove. While so tenuous that Astronomy has been taxed to prove that it exerts an appreciable resistance upon the least of the celestial bodies, its elasticity is such that it transmits a compression with a well nigh infinite velocity.[114] On the one hand, Thomson has determined its inferior limit, and finds that a cubic mile of it would weigh only one thousand-millionth of a pound;[115] on the other, Herschel has calculated that, if an amount of it equal in weight to a cubic inch of air be enclosed in a cubic inch of space, its reaction outward would be upward of seventeen billions of pounds.[116] Instead of being represented as is our air, by the pressure of a homogeneous atmosphere five miles in height, such a pressure would represent just such a homogeneous atmosphere five and a half billions of miles high, or about one-third the distance to the nearest fixed star! In Herschel's own words: "Do what we will, adopt what hypotheses we please, there is no escape in dealing with the phenomena of light, from these gigantic numbers; or from the conception of enormous physical force in perpetual exertion at every point through all the immensity of space."[117]

Now, as Preston has suggested,[118] if we regard this ether as a gas, defined by the kinetic theory that its molecules move in straight lines, but with an enormous length of free path, it is obvious that this ether may be clearly conceived of as the source of all the motions of ordinary matter. It is an enormous storehouse of energy, which is continually passing to and from ordinary matter, precisely as we know it to do in the case of radiant transmission. When potential energy becomes kinetic, the ether loses and the matter gains motion. When kinetic energy becomes potential, the lost energy of the matter is the motion gained by the ether. Before so simple a conception as this, both potential energy and action at a distance are easily given up. All energy is kinetic energy, the energy of motion. Giving now to the ether its storehouse of tremendous power, and giving to it the ability to transfer this power to ordinary matter upon opportunity, and we have an environment compared with which the strongest steel is but the breath of the summer air. In presence of such an energy it is that we live and move. In the midst of such tremendous power do we act. Is it a wonder that out of such a reservoir the power by which we live should irresistibly rush into the organism and develop the transmuted energy which we recognize in the phe-

nomena of life? Truly, as Spinoza has put it, "Those who fondly think they act with free will, dream with their eyes open."[119]

Such now are some of the facts and the theories to be found in the science of to-day concerning the phenomena of life. Physiologically considered, life has no mysterious passages, no sacred precincts into which the unhallowed foot of science may not enter. Research has steadily diminished day by day the phenomena supposed vital. Physiology is daily assuming more and more the character of an applied science. Every action performed by the living body is sooner or later, apparently, to be pronounced chemical or physical. And when the last vestige of the vital principle as an independent entity shall disappear from the terminology of science, the word "Life," if it remain at all, will remain only to signify, as a collective term, the sum of the phenomena exhibited by an active organized or organic being.

I cannot close without speaking a single word in favor of a vigorous development in this country of physiological research. What has already been done among us has been well done. I have said with diffidence what I have said in this address, because I see around me those who have made these subjects the study of their lives, and who are far more competent to discuss them than I am. But the laborers in the field are all too few, and the reasons therefor are not far to seek. One of these undoubtedly is the high scientific attainment necessary to a successful prosecution of this kind of investigation. The physiological student must be a physicist, a chemist, an anatomist and a physiologist all at once. Again, the course of instruction of those who might fairly be expected to enter upon this work, the medical students of the country, is directed toward making them practitioners rather than investigators. In the third place the importance of physiological studies in connection with zoölogical research is only beginning in this country to receive the share of attention it deserves. I well remember the gratification I experienced in 1873 upon receiving a letter from Professor Louis Agassiz, announcing his intention to have lectures at Penikese upon physiological chemistry; a new departure for those times. In this view of the case it seems very appropriate that a new subsection of this Association should be just now in process of formation. We welcome warmly the body of men who form it and we predict that from the new subsection of Anatomy and Physiology most valuable contributions will be received for our proceedings.

It is a beautiful conception of science which regards the energy which is manifested on the earth as having its origin in the sun. Pulsating awhile in the ether-molecules which fill the intervening space, this motion reaches our earth and communicates its tremor to the molecules of its matter. Instantly all starts into life. The winds move, the waters rise and fall, the lightnings flash and the thunders roll, all as subdivisions of this received power. The muscle of the fleeing animal transforms it in escaping from the hunter who seeks to use it for the purpose of his destruction. The wave that runs along that tiny nerve-thread to apprise us of danger transmutes it, and the return pulse that removes us from its presence is a portion of it. The groan of the weary, the shriek of the tortured, the voicéd agony of the babeless mother, all borrow their significance from the same source. The magnificence of the work of a Leonardo da Vinci or a Michael Angelo; the divine creations of a Beethoven or of a Mozart; the immortal Principia of a Newton and the Méchanique Celeste of a Laplace,—all had their existence at some point of time in oscillations of ether in the intersolar space. But all this energy is only a transitory possession. As the sunlight gilds the mountain top and then glances off again into space, so this energy touches upon and beautifies our earth and then speeds on its way. What other worlds it reaches and vivifies, we may never know. Beyond the veil of the seen, science may not penetrate. But religion, more hopeful, seeks there for the new heavens and the new earth, wherein shall be solved the problems of a higher life.

NOTES.

[1] *H. Bence Jones*, Croonian Lectures on Matter and Force, London, 1868.

[2] *Herbert Spencer*, Principles of Biology, New York, 1871, I, 60.

[3] *Küss*, Lectures on Physiology, Edited by Duval and Translated by Amory, Boston, 1875, 2.

[4] "Among the phenomena of life those which are intelligible to us are precisely of the physical or mechanical order."—*Marey*, Animal Mechanism, New York, 1874, 7. "All action of which we are immediately cognizant is but the result of the operation of the solar heat upon and through independent and correlative existences." "Conservation of energy makes more and more doubtful the existence of a vital principle, and tends to bring the phenomena of living bodies more and more within the domain of pure physical necessity."—*Acland*, Medicine in Modern Times, London, 1869, 23.

[5] "An animal can no more generate an amount of force capable of moving a grain of sand, than a stone can fall upwards or a locomotive draw a train without fuel.

Frankland, Phil. Mag. IV, xxxii, 182; Proc. Roy. Inst., June 8, 1866; Am. J. Sci., II, xlii, 393, Nov., 1866.

[6] *Haughton*, Medicine in Modern Times, 107. *Lavoister*, Ib., 113; also Phys. Chem. Schriften, 1785, Bd. III.

[7] *Bischoff* and *Voit*, Die Gesetze der Ernährung des Fleischfressers, Munich, 1860. "They found that the amount of urea eliminated was not in proportion to the exercise of force, but the amount of carbonic acid was so."—Sci. Conf., 174. *Lawes* and *Gilbert*, South Kensington Science Conferences, London, 1876, Biology, 173, 174. "I believe it is now accepted that the elimination of urea is no measure of the muscular force exerted within the body. *Haughton*, loc. cit., 108, "No greater mistake is possible in Physiology than to suppose that the products of the changes in the blood, by which mechanical or intellectual work is done, are themselves merely the result of the waste of the organs whether muscles or brain, on the exercise of which that work depends." *Theodor*, Zeitschr. f. Biol., xiv, 51–56; J. Chem. Soc., xxxvi, 74. *Voit*, Ib., 57–100; Ib., 75. Also, Untersuchungen über den Einfluss des Kochsalzes, des Kaffees, und der Muskelbewegungen auf den Stoffwechsel, Munich, 1860. *Fick* and *Wislicenus*, Phil. Mag., IV, xxxi, 485. *Smith, E.*, Phil. Trans., 1859, 709; 1861, 747. *Parkes*, Proc. Roy. Soc., xv, 339; xvi, 44. *Noyes*, Am. J. Med. Sci., Oct., 1867. *M. Foster*, Textbook of Physiology, 3d ed., London, 1880, 471.

[8] *Haughton*, loc. cit., 120.

[9] *Haughton*, Animal Mechanics, London, 1873. On Law of Fatigue, see Proc. Roy. Soc., 1879, 1880; Nature, xxii, 128, 1880; Am. J. Sci., III, xx, 147.

[10] *Du Bois Reymond*, Untersuch. ü. thierische Elektricität, 1848–1860. *Marey*, loc. cit. 22, 50. *Radcliffe*, Dynamics of Nerve and Muscle, London, 1871, 26. *Hermann*, Nature, xix, 561. *Haughton*, Animal Mechanics, 5. *Maudsley*, Physiology and Pathology of Mind, 2d ed., London, 1868, 42. *Voit* and *Theodor*, J. Chem. Soc., xxxvi, 951. The conclusions are: 1st, the muscles are the centre of the formation of CO_2, in health 403 grams being excreted, and in a paralytic 250 grams. 2d, lowering of the temperature from 14.3° to 4.4° increases the CO_2 from 155.0 to 210.7 grams. 3d, the urea was not increased, showing that the non-nitrogenous matter was burned. *Foster*, op. cit., 66, 116, et seq. *Gamgee*, Physiol. Chemistry Anim. Body, London, 1880, 345 et seq.

[11] *Matteucci*, Physical Phenomena of Living Beings, London, 1847, 205, 216, 321.

[12] *E. J. Marey*, loc. cit., 53; Nature, xix. 295, 320; C. R., lxxxiv, 190, 354, 1877.

[13] Students' Text-Book of Electricity, *Noad*, 4th ed., edited by Preece, London, 1879, 159.

[14] *Donders* and *Buys Ballot*, Over de Elasticiteit der Spiren, Utrecht, 1863, 47. Quoted in Animal Mechanics, 2.

[15] *Radcliffe, C. B.*, Dynamics of Nerve and Muscle, London, 1871, 29. *Matteucci*, loc. cit., 216, "The analogies between muscular contraction and the discharge of the torpedo are complete; what destroys, augments and modifies the one, acts equally upon the other." *Marey*, "1st, the rapidity of the nervous agent in the electrical nerves of the torpedo seems evidently to be the same as that of the nervous agent producing motion in the frog. 2d, the phenomenon called by Helmholtz lost time exists also in the electric apparatus of the torpedo and lasts about the same time as in the muscle. 3d, the discharge of the torpedo is not instantaneous but is prolonged about 0.14 of a second; which is in a remarkable degree equal to the duration of the shock of the frog's muscle." Animal Mechanism, 57.

[16] *Wollaston*, Phil. Trans., 1809.—*Haughton*, Anim. Mechanics, 16. "It resembles most nearly the deep hum produced by the blowing fan of a large foundry." It is obtained by gently inserting the extremity of the finger into the ear, bringing at the same time the muscles of the hand and forearm into strong contraction. Brunton places the ball of the strongly contracted thumb against the ear. Sci. Conf., 193.

[17] *Weber*, Muskelbewegung, Wagner's Handwörterbuch. *Minot, C. S.*, Jour. Anat. Physiol., xii, 297, 1878. *Lauder Brunton*, Sci. Conf., Biology, 192–4. *Marey*, Animal Mechanism, 47.

[18] *Marey*, Compte Rendu des Travaux du Laboratoire de M. Marey, Paris, 1877, iii. "1st, a torpedo's discharge is not a continuous current. It is formed of a series of successive waves, added one upon another. 2d, each electric wave presents a phase

of suddenly increasing intensity followed by a phase of gradually decreasing intensity, 3d, currents induced by a torpedo discharge are all produced at the beginning of each wave. 4th, there are currents induced on the completion of a circuit, the inverse of the inducing currents, as is shown by the electrometer. 5th, the discharge of the torpedo is analogous to muscular tetanus; every electric wave in the discharge corresponds to a muscular shock." Thus we see that a muscle-shock, like an electric discharge, is produced by a single excitation, the delay being the same, about seven hundredths of a second. The wave, like the shock, increases more abruptly than it decreases, alike in muscle and electric organ. The same agents modify the wave and shock similarly. Heat renders both more energetic up to a certain point, from which both disappear. Cold diminishes both and they both cease at zero. In both, the waves and the shocks run into each other in the same manner. Both suffer from fatigue alike and both are alike affected by poisons. "Does the fact that a voluntary discharge of the torpedo is a complex act not prove that the voluntary contraction of the muscles is also a complex act? Very certainly the comparison of the voluntary contraction of the muscles with the tetanic phenomena produced by electricity, or strychnine, the existence of a muscular sound during the contraction, the quivering or dissociation of the shocks which are produced under the influence of cold,—all these seem arguments in favor of the theory which considers muscular contraction as the result of very frequent shocks; but the complexity of the voluntary discharge of the torpedo, the manner in which the waves composing it succeed each other and are added together, forms a very important confirmation of the numerous presumptions already made." Abstract by Francois Franck, Nature, xix, 295, 320.

[19] *Feddersen*, Pogg. Ann., cliii, 69. *O. N. Rood*, Am. J. Sci., II, xlviii, 153, 1869. *A. M. Mayer*, Ib., III, viii, 136, 1874. The electric spark in air, and probably also the discharge through conductors, is intermittent and consists of a great number of oscillatory movements. In a private communication from Dr. T. A. Edison, he states that muscular tetanus can be produced by rapid vibrations of a purely mechanical character; the effect closely resembling that which results from the secondary current of an induction coil.

[20] *Matteucci*, loc. cit., 195, 252. *M. Foster*, Physiology, 44-46. "When a frog is poisoned with urari, the nerves may be subjected to the strongest stimuli without causing any contractions in the muscles to which they are distributed; yet even ordinary stimuli applied directly to the muscle readily cause contractions." "The activity of contractile protoplasm is in no way essentially dependent on the presence of nervous elements."

[21] *Matteucci, loc. cit.*, 200. "The origin of this current resides in the electric conditions which are produced by the chemical actions of the nutrition of the muscle." *Marey*, loc. cit., 55. "As to the origin of the electric force, we think no one can now see anything in it but the result of chemical actions produced in the interior of the apparatus."

[22] *Prof. Sir Wm. Thomson*, Electrostatics and Magnetism, 1872, 317. *Fleeming Jenkin*, Electricity and Magnetism, 44. "If two metals (as copper and zinc) be plunged in water, the copper, the zinc, and the water forming a galvanic cell, all remain at one potential and no charge of electricity is observed on any part of the system."

[23] *Peclet*, Ann. Chim. Phys., III, ii, 233. *Kohlrausch*, Pogg. Ann., lxxxviii, 465; lxxii, 353; lxxxii, 1. *Buff*, Ann. Chem. Pharm., xlii, 5; xlv, 137. *Becquerel*, Ann. Chim. Phys., xxv, 405; C. R., xxii, 677. *Hankel*, Pogg. Ann., cxxvi, 286. *Gerland*, Ib., cxxxiii, 513. *Du Moncel*, C. R., xc, 964; La Lumière Electrique, ii, 357, 1880. *Pellat*, C. R. xc, 990. *Ayrton* and *Perry*, Proc. Roy. Soc., 1878-9; Phil. Trans., 1879; Nature, xix, 498, 1879. *Everett*, Units and Physical Constants, 146-150, 1879. *Gore*, Nature, xxii, 21. *Edelmann*, Rep. f. Exp. Physik., xvi, 464, 1880. *B. O. Peirce, jr.*, Inaug. Diss., Leipzig, 1879.

[24] *L. Cumming*, Theory of Electricity, London, 1876, 120. *et seq.*

[25] *Peltier*, Ann. Chim. Phys., lvi, 371, 1834. *Tyndall*, Phil. Mag., IV, iv, 419. *Lenz*, Pogg. Ann., xliv, 342. *Bouty*, C. R., xc, 917, 987, 1880.

[26] *Prof. Sir Wm. Thomson*, Phil. Mag., IV, iii, 529, 1852; viii, 62-69; Phil. Trans., iii, 661, 1856. *Jenkin*, Electricity and Magnetism, 187.

[27] *Hoorweg*, Wied. Ann., II, ix, 552, Apr., 1880; Nature, xxii, 90.

[28] *Matteucci*, loc. cit., 200. *Marey*, loc. cit., 55. *M. Foster*, op. cit., 474. "If we admit that the energy of muscular contraction (and with that the energy of all other vital manifestations) arises from an explosive decomposition of a complex substance which we may call real protoplasm, and that this complex protoplasm is capable of reconstruction within limits which may be very wide, we acquire a conception of physiological processes, which if not precise and definite is at least simple and consistent and moreover a first step toward a future molecular physiology."

[29] *Matteucci*, loc. cit., 332. *Prevost* and *Dumas*, *Schwann*, Quoted by *Lauder Brunton*, Sci. Conf., Biol., 191, 192.

[30] *Matteucci*, Op. cit., 332. *Brunton*, Sci. Conf., 191. *Ritter*, Thèse de concours, Strasbourg, 1863. *Küss*, Physiol., 70. *Foster*, Physiol., 61.

[31] *Radcliffe*, loc. cit., viii, ix, 20, 98. *Quincke*, Nature, xxii, 206, 1880.

[32] *Marey*, Op. cit., 39.

[33] *Rouget*, *C. R.*, June, 1867. "The lengthening is produced by a moving cause which is developed in the act of nutrition and is correlative to heat if it be not heat itself." *Engelmann*, Pflüger's Archiv, vii, 33-71; 155-188; xviii, 1-24. Also Hoffman and Schwalbe's Jahresbericht for 1878, 71, 72. [I am indebted for these views of Engelmann to Dr. C. S. Minot of Boston.—*B.*]

[34] *Schäfer*, Sci. Conf., Biol. 171. *Marey*, C. R., lxxxix, 203. *Levon*, Ib., 242. *Richet*, Ib., 792, 956; Nature, xx, 103, xxi, 76. *Couty* and *Lacerda*, C. R., lxxxix, 794, 1034; Nature, xxi, 76.

[35] *Prout*, "This agency is vital and its nature is completely unknown. It is impossible to imagine that the agency of the stomach can be chemical." Bridgewater Treatise, 1834, 493. Quoted by Bence Jones, loc. cit., 59.

[36] *Kühne*, Physiologische Chemie, 1866. *Haidenhain*, Nature, xix, 544. *Bernhard*. J. Chem. Soc., xxxiv, 82. *Defresne*, C. R., lxxxix, 737, 1070; J. Chem. Soc., xxxviii, 330. *Richet*, J. Chem. Soc., xxxiv, 520. *Seegen*, J. Chem. Soc., xxxvi, 548, 549. *Harth*, J. Chem. Soc., xxxvi, 660. See also Am. Chem. Journal, ii, 204-212.

[37] *Matteucci*, loc. cit. "Thin plates of slate or of baked clay" show the phenomena of osmose (page 35); and those of transpiration take place "with tubes of vein, artery, clay, pasteboard and wood" (page 83). *Nasse*, J. Chem. Soc., xxxiv, 519. See also xxxviii, 414.

[38] *Setschinoff*, J. Chem. Soc., xxxiv, 519. See also *Grehant*, Nature, xviii, 103. *Gaule*, Nature, xix, 474. *Fredericq*, The venous blood of the squid (*Octopus vulgaris*) and lobster (*Homarus*) contains a colorless albuminoid cupriferous body, which he calls hæmocyanin. In the gills it forms an unstable compound with oxygen, oxyhæmocyanin, being of a beautiful blue color. The venous blood is colorless; but when the animal respires aerated water, the arterial blood becomes blue. Like hæmoglobin, hæmocyanin breaks up into a proteid substance free from copper, and a cupriferous body analogous to hæmatin. J. Chem. Soc., xxxv, 333; Nature, xxi, 370.

[39] *Marey*, Travaux du Laboratoire, i, 100; ii, 1, 1875.

[40] *Schmidt*, Reichert u. Du Bois Reymond's Archiv, 1861, 545; 1862, 428, 533.

[41] *Hammarsten*, J. Chem. Soc., xxxv, 472; xxxviii, 172; Pflüger's Archiv, xiv, 211; xvii, 413; xviii, 38; xix, 563. See also *Gamgee*, op. cit., 42-53.

[42] *Liebreich*, Ann. Chem. Pharm., cxxxiv, 29.

[43] *Gamgee* and *Blankenhorn*, $C_{100}\ H_{208}\ N_5\ PO_{35}$, J. Chem. Soc., xxxvi, 950; Nature, xxi, 387; Zeitschr. phys. Chem., iii, 260, 1879.

[44] *Radcliffe*, loc. cit., 17. *Du Bois Reymond*, Gesammelte Abhandl., ii, 232, 1877. *Engelmann*, Pflüger's Archiv, xv, 211, 1877.

[45] *Haughton*, Anim. Mechanics, 18, Note.

[46] *Herbert Spencer*, Principles of Psychology, I, 81, et seq. *Clifford*, Seeing and Thinking, London, 1879, 12-17. See also Sci. Conf., Biology, 224.

[47] *Donders*, Sci. Conf., Biology, 224. *Müller, J.*, Handbuch der Physiologie des Menschen, Coblentz, 1844, i, 581.

[48] *Helmholtz*, Müller's Archiv, 1850, 276; 1852, 199. *Du Bois Reymond*, Royal Inst. Lecture, 1868. *Haughton*, Anim. Mechanics, 14. Along the motor nerve of the frog, Helmholtz found the velocity of transmission at the rate of 88 feet per second. Along the sensor nerves of man Schelske found it to be at the rate of 97 feet.

[49] *Marey*, Anim. Mechanism, 41, 43. See also Du Bois Reymond's Lecture, "On the Time required for the Transmission of Sensation and Volition through the Nerves," Proc. Roy. Inst., 1866. *Garver*, Am. J. Sci., III, xv. 413, 1878; xx, 189, 1880.

[50] *Lovering*, Proc. A. A. A. S., xxiv, 37, 1876.

[51] See *Preece's* edition of *Noad's* Electricity, 67. On aërial wires the observed speed has varied from 112,680 miles per second on a wire 179 metres long to 816 miles per second on a cable 1,020 kilometres in length.

[52] *Weber*, Quoted by Radcliffe, loc. cit., 18.

[53] *Radcliffe*, loc. cit., 18.

[54] *Gaugain*, Quoted by Lovering, loc. cit., 37.

[55] *Marey*, "We know that electric phenomena are produced in the nerve when it has been excited in a certain way, and that their propagation throughout the nervous cord seems to have precisely the same speed as that of the transference of the nervous energy itself." Anim. Mechanism, 41. *Bernstein*, Untersuch. ü. d. Erregungsvorgang im Nerven- und Muskel-systeme, 1871, has shown that the negative variation or current of action passes along a muscle or nerve from the spot stimulated in the form of a wave, travelling in the nerve at the same rate as the nervous impulse, in the muscle at the same rate as the contraction. See Foster, op. cit., pp. 77, 105.

[56] *Clifford*, Seeing and Thinking, 26. *Du Bois Reymond*, Roy. Inst. Lect., 1866. *Spencer*, Psychology, I, 81.

[57] *Bert*, C. R., lxxxiv, 173. *Foster*, op. cit., 77, 126, 503.

[58] *Bain*, The Senses and the Intellect, 3d ed., New York, 1872. *Maudsley*, Body and Mind, London, 1870. Physiology and Pathology of Mind, 2d ed., London, 1868. *Spencer*, Principles of Psychology, New York, 1871.

[59] *Maudsley*, Body and Mind, 18; Physiology and Pathology, 42, 44, 49, 138. *Bain*, loc. cit., 12. *Clifford*, loc. cit., 77. *Allman*, Nature xx, 393. "Every phenomenon of mind is the result as manifest energy, of some change, molecular, chemical or vital, in the nervous elements of the brain."—"The performance of an idea, like the performance of movement, is a retrograde metamorphosis of the organic element." "Mental action is as surely dependent on the nervous system as the liver function is on the hepatic."—*Maudsley*. "No fact in our constitution can be considered more certain than this, that the brain is the chief organ of mind and has mind for its principal function."—*Bain*. "That consciousness is never manifested except in presence of cerebral matter or something like it, there cannot be a question."—*Allman*.

[60] *J. S. Lombard*, New York Medical Journal, v, 198, June, 1867.

[61] *Hirsch*, Determination telegraphique de la difference de longitude entre les Observatoires de Genève et de Neuchatel. Genève et Bale, 1864. *Donders*, Reichert ü. Du Bois Reymond's Archiv, 1868, 657; Sci. Conf., Biol., 225. *Haughton*, Anim. Mechanics, 15.

[62] *Donders*, Science Conferences, Biology, 226.

[63] *Donders*, lb., 227.

[64] *Gaskell*, Science Conferences, Biology, 186. *Angelo Mosso*, "Sopra un nuovo metodo per scrivere i movimenti dei vasi sanguigni nell' uomo;" Atti della Reale Accademia della Scienze di Torino, xi, Nov. 14, 1875.

[65] *Barnard*, Proc. A. A. A. S., xvii, 93, 1868.

[66] *Clausius*, Pogg. Ann., Dec., 1854. *Thomson*, Proc. Roy. Soc. Edin., 1852; Phil. Mag., 1852; Feb., 1853. *Tait*, Thermodynamics, Edinburgh, 1868, 29, 58. "No known natural process is exactly reversible and whenever an attempt is made to transform and retransform energy by an imperfect process, part of the energy is necessarily transformed into heat and dissipated, so as to be incapable of further useful transformation. It therefore follows that as energy is constantly in a state of transformation there is a constant degradation of energy to the final unavailable form of uniformly diffused heat; and that this will go on as long as transformations occur, until the whole energy of the universe has taken this final form." *Maxwell*, Theory of Heat, 188. *Balfour Stewart*, Conservation of Energy, New York, 1874, 141-154.

[67] *Hermann*, Nature, xix, 531, 1879; Unters. ü. d. Stoffwechsel, Berlin, 1867.

[68] *Berthelot*, Essai de Méchanique Chimique fondée sur la Thermochimie, Paris, 1879. C. R., xc, 1240, 1880. *Thomsen*, Various papers in Pogg. and Wied. Ann., Ber. Berl. Chem. Ges., etc.

[69] *J. W. Draper*, Jour. Frank. Inst., Sept., 1834; Scientific Memoirs, New York, 1878, 346.

[70] *Lippmann*, Ann. Chim. Phys., V, v, 494, Aug., 1875.

[71] *Gore*, Proc. Roy. Soc., Apr. 22, 1880; Nature, xxii, 21.

[72] *Pouillet*, Ann. Chim. Phys., II. xx, 141. *Becquerel*, Ann. Chim. Phys. xxiv, 342. *Matteucci*, loc. cit., 30. *Quincke*, Pogg. Ann., cvii, 1, 1859; cx, 38, 1860.

[73] *Küss*, loc. cit., 3.

[74] *Beale*. Protoplasm, or Life, Matter and Mind, London, 1870, 108. "This transparent material possesses a remarkable power of movement. It may thus transport itself long distances and extend itself so as to get through pores, holes, and canals too minute to be seen even with the aid of very high powers. There are creatures of exquisite tenuity which are capable of climbing through fluids and probably through the air itself—creatures which climb without muscles, nerves, or limbs—creatures with no mechanism, having no structure; capable when suspended in the medium in which they live, of extending any one part of the pulpy matter of which they consist beyond another part, and of causing the rest to follow."

[75] *Beale*, op. cit., 48.

[76] *Ranvier*, C. R., lxxxix, 318. "Cellular elements possess all the essential vital properties of the complete organism. A primitive fasciculus of striated muscle, which is a cellule, possesses sensibility, motricity and contractility. A gland-cellule (and all cellules are more or less glandular), has the same. Lymphatic cellules digest starch, proteids, and fatty matters which have been absorbed, in consequence of their amœboid activity."

[77] *Allman, G. J.*, Pres. Linnean Society, Nature, xx, 384, 1879.

[78] *Beale*, Op. cit., 33, 55. "Nothing that lives is alive in every part. ****In man and the higher animals the free portion of the nails and hair, the outer part of the cuticle and a portion of the dental tissues are evidently lifeless. **Of the internal tissues a great part is also in a non-living condition." "This contractile tissue [of the muscle] is not, like the germinal matter which produced it, in a living state." "The nerve fibre is composed of formed material." "The non-living tissue which is thus spun off as they [these oval masses of germinal matter] become separated, is the nerve."

[79] *Beale*, Op. cit., 106.

[80] *Allman*, loc. cit., 392. "Irritability, the one great character of all living beings, is not more difficult to be conceived of as a property of matter than the physical phenomena of radiant energy." "There is no greater difficulty in conceiving of contractility as a property of protoplasm, than there is of conceiving of attraction as a property of the magnet."

[81] *Schützenberger, J.* Chem. Soc., xxxvi, 542. *Foster*, Op. cit., 726–748.

[82] *Acland*, Medicine in Modern Times, 28. "There are virtually no limits to the substances which can be made" [by chemistry]. *Odling*, Animal Chemistry, 1866, 58. "Already hundreds of organic principles have been built up from their constituent elements and there is now no reason to doubt our capability of producing all organic principles whatsoever in a similar manner."

[83] *Thos. Graham*, "On Liquid Diffusion applied to Analysis." Phil. Trans. 1862.

[84] *Maudsley*, Phys. and Path., 46; Body and Mind, 161. See also Nature, xxi, 586, 1879.

[85] *Graham*. loc. cit.

[86] *T. Sterry Hunt*, Am. J. Sci., II, v, 74, 1848; vii, 109, 1849. Chemical and Geological Essays, 179, 180. See also *Dusart*, C. R., May, 1861, 974. *Schoonbroodt*, Ib., May, 1860, 856. *Fischer* and *Boedeker*. Ann. Chem. Pharm., cxvii, 111. *Wolcott Gibbs*, Am. J. Sci., II, xxv, 31, 1858.

[87] *Allman*, Address, Nature, xx, 387.

[88] *J. W. Draper*, Proc. Am. Phil. Soc., May, 1843; Scientific Memoirs, 410; Am. J. Sci., III, iv. 161, Dec., 1872; Nature, xxii, 29, 1880.

[89] *Vines*, Nature, xviii, 110. *Boussingault*, Ib., xviii, 672. *Pringsheim*, Ib., xxi, 85. *Lankester*, Nature, xxi, 557. *Boehm*, J. Chem. Soc., xxxiv, 84, 162. *Macagno*, Ib., xxxiv, 90, 162. *Corenwinder*, Ib., xxxiv, 595; C. R., lxxxvi, 608.

[90] *Bert*, C. R., lxxxvii, 695; J. Chem. Soc., xxxvi, 336.

[91] *Gautier, Trecul, Chevreul*, C. R., lxxxix, 861, 883, 917, 972, 989.

[92] *Allman*, loc. cit., 390.

[93] *Thistleton Dyer*, Sci. Conf., Biology, 162. *Kraus*, J. Chem. Soc., xxxviii, 57. *Wiesner*, Nature, xix, 161.

[94] *Geddes*, C. R., lxxxvii, 1095, Dec., 1878; Proc. Roy. Soc., xxviii, 449.

[95] *Allman*, loc cit., 390.

[96] *Darwin*, Insectivorous Plants, New York, 1875, 85–135.

[97] *Nägeli*, Ber. Ak. München, 1878.

[98] *Wurtz and Bouchut*, C. R., lxxxix, 425. *Wittmak*, J. Chem. Soc., xxxvi, 1048. *Peckolt*, Ib., xxxviii, 128. Twenty-eight centigrams of this ferment, which Peckoldt calls papayotin, dissolved twenty centigrams of meat in ten minutes.

[99] *Bernhard*, J. Chem. Soc., xxxiv, 82. *Defresne*, Ib., xxxviii, 330. *Baswitz*, Ib., 132. *Krauch*, Ib., 175.

[100] *Boussingault*, C. R., lxxxvii, 277; J. Chem. Soc., xxxv, 73.

[101] *Claude Bernard*, Phenomènes de la vie commun aux Anim. et Veget., Paris, 1879.

[102] *Claude Bernard*, op. cit.

[103] *Schützenberger*, C. R., lxxxviii, 287, 383, 593. *Jamieson*, Nature, xviii, 539.

[104] *Kühne*, Lehrbuch der Physiologischen Chemie, Leipzig, 1866, 274. *Bleunard*, C. R., xc, 612, 1080.

[105] *Vines*, Proc. Roy. Soc. May 13, 1880; Nature, xxii, 91. *Barbieri*, J. prak. Ch., II, xviii, 102; J. Chem. Soc., xxxvi, 272; xxxviii, 342.

[106] *Weyl* and *Bischoff*, Ber. Berl. Chem. Ges., xiii, 367.

[107] *Hammarsten*, J. Chem. Soc., xxxv, 472; xxxviii, 172; Pflüger's Archiv, xvii, 413; xviii, 38; xix, 563.

[108] *Hoppe-Seyler*, Medicinisch-Chemische Untersuchungen, 1866, 162.

[109] *Gamgee*, op. cit., 4, "The proteids of the animal body are all derived directly or indirectly from vegetable organisms which possess the power of constructing them out of the comparatively simple chemical compounds which serve as their food. Such a synthesis never takes place in the animal body, though the latter possesses the power of converting any vegetable or animal proteid into the various proteids which are characteristic of its solids and liquids." *Foster*, op. cit., 474, "The whole secret of life may almost be said to be wrapped up in the occult properties of certain nitrogen compounds." "Pflüger has drawn some very suggestive comparisons between the so-called chemical properties of the cyanogen compounds and the so-called vital properties of protoplasm."

[110] *Pasteur*, C. R., May 3, 1880; Nature, xxii, 48, 1880.

[111] *Archibald*, Nature, xix, 145. *Jevons*, Ib., 338. *Chambers*, Ib., xviii, 567, 619. *Stewart*, Ib., 616; Conservation of Energy, New York, 1874, 98.

[112] *Jevons*, Nature, xix, 33, 97, 196, 588; xviii, 483.

[113] *W. K. Clifford*, "Energy and Force," Nature, xxii, 122, 1880. *S. T. Preston*, "Physical Aspects of the Vortex Atom Theory," Nature, xxii, 56, 1880.

[114] *Clifford*, Seeing and Thinking, 39. "The luminiferous ether is not a fluid like water, but it is a solid something like a piece of jelly." See also *Hall* and *Harkness*, Report on Encke's Comet, Washington, 1871.

[115] *Thomson, Sir Wm.*, Phil. Mag., IV, ix, 36–40. *Stewart* and *Tait*, The Unseen Universe, 104.

[116] *J. F. W. Herschel*. "Familiar Lectures on Scientific Subjects," London, 1867, 282.

[117] *Herschel*, Op. cit., 282. *Sir Wm. Thomson*, Phil. Mag., IV, ix, 36. "The mechanical value of a cubic mile of sunlight [at the earth's distance from the sun] is consequently 12050 foot-pounds, equivalent to the work of one horse power for a third of a minute."

[118] *S. Tolver Preston*, Phil. Mag., V, ix, 356, May, 1880.

[119] *Spinoza*, Quoted by Maudsley, Phys. and Path., 170–172.

REPORTS OF COMMITTEES.

Report of the Committee upon Forestry.

At the meeting of this Association held at Saratoga, in August last, a Resolution was introduced, and referred by the Standing Committee, to the Committee upon Forestry, in which, in view of the great importance of the matter of Forest-protection and Wood-culture, the desire was expressed, that some Report looking towards definite action be received from this Committee, at the next meeting of the Association.

Your published proceedings show, that two reports have already been made by the Committee upon Forestry; but in order to present the subject in a connected form, we will here briefly review the action that has been taken in pursuance of the object mentioned.

At the session held at Portland, in 1873, it was

"*Resolved*, That a Committee be appointed by the Association, to memorialize Congress and the several State Legislatures upon the importance of promoting the cultivation of Timber, and the preservation of Forests, and to recommend proper legislation for securing these objects."

It was understood at the time, that an amendment was adopted, directing this Committee to enter into correspondence with Forestry Associations of other countries, with the view of more effectually promoting the interests involved in this subject, but the proceedings as published do not show this fact.

A few days afterwards, a preliminary meeting was held at the house of Mr. George B. Emerson, in Boston, at which a majority of the Committee was present, and an interchange of views was had, as to the best means for carrying into effect the wishes of the Association, in the matter under their charge. As the interests involved concern no particular State, but pervade the whole coun-

try, it was thought proper to bring the subject first before Congress; and a sub-committee consisting of George B. Emerson and F. B. Hough was appointed to give personal attention to this duty.

A memorial was prepared and sent in printed form to each member of the Committee, and to other persons known to feel an interest in the subject, inviting their suggestions, and asking them to propose any modifications in the memorial that they might deem proper.

In January, 1874, this sub-committee repaired to Washington, and for preliminary consultation, a meeting was held at the Smithsonian Institution, at which several members of Congress and others attended.

As the result of this conference, it was thought best to ask for the appointment of a Commission, similar to that previously created in the interest of Fisheries, for the collection of facts, and the publication of information upon the subject.

This being cordially approved by the President, the memorial of the sub-committee was transmitted by him to both Houses of Congress, with the strong approval of the Commissioner of the General Land Office, and of the Secretary of the Interior.

This special message of the President was referred in each House to the Committee on the Public Lands,[1] and an arrangement was made between the Chairmen of these Committees, to the effect that the Committee of the House should first examine the subject in detail, and that the Senate Committee should reserve its action until the former had reported.

At this stage of the proceedings, the senior member of the sub-committee was recalled by private affairs,—the other member remaining until near the close of the session.

After unforeseen delays, caused by other business having precedence in the Committee, an opportunity for a hearing was given, and the question was referred to a sub-committee, consisting of Messrs. Dunnell of Minnesota, Phillips of Kansas, and Herndon of Texas. These gentlemen, having faithfully examined the subject in detail, adopted a Report which, being presented to the general Committee and approved by them, was laid before the House with an accompanying Bill, on the 17th of March following.[2]

[1] Senate Ex Doc., 23; 1st sess., 43rd Cong.

[2] Report No. 250, H. R.; 1st sess., 43rd Cong. An extra edition of 5,000 copies of this Report was ordered by Congress.

A hearing was afterwards given by the Senate Committee, but the subject did not reach them for their action, which would doubtless have been unanimously in its favor,—judging from the opinions expressed in conversation by each of the members.

In this Report of the House Committee, they say:—"That they have given their attention to the subject, and learn that the memorial above referred to was prepared by a Committee of the American Association for the Advancement of Science, as the result of a discussion induced by a communication read before them at their annual session in Portland, Maine, on the 21st of August, 1873, on 'the Duty of Governments in the Preservation of Forests.'

"A subject endorsed by an Association, embracing within its membership the highest scientific talent of the country, must commend itself to our notice as worthy of attention. More especially is this notice due, when their action takes the form of a recommendation to Congress, upon a subject alleged to involve the duty of the Government, upon questions that vitally affect the interests of the whole country, and especially those of Agriculture, Manufactures and Commerce.

"When it is further affirmed, that without timely provision of law, these great agencies of civilization and elements of wealth, will, in the near future, be materially impaired, we cannot hesitate to give the recommendation a most careful examination, to the end that, if well founded, the measures best calculated for averting these injuries may be devised, and the remedies most effectually applied.

"After as full an investigation of the question as present opportunities allow, we are convinced that the statements of the memorial are substantially true, and that it is the duty of the Government to take immediate measures for ascertaining the condition and prospects of our timber supply, to the end that the future wants of the country, with regard to these great interests, both in their scientific and practical relations, should be thoroughly investigated and made widely known."

The Bill accompanying this Report provided for the appointment of a Commissioner of Forestry, and afforded facilities for his researches, but failed at that session to become a law. It was not opposed in any manner, but was simply laid aside by the pressure of other measures then before Congress.

At the Hartford meeting of this Association, held in 1874, a Report, signed by a majority of your Committee, was presented by Mr. George B. Emerson, and will be found to give the above proceedings in fuller detail.[3]

Early in the 44th Congress, the Hon. Mark H. Dunnell, who had taken particular interest in this subject, introduced upon notice, a similar Bill, which received the same reference as before. An opportunity for a hearing was given, but no report was made. Before the end of this session, this gentleman, however, secured the adoption of an amendment to an act making appropriations for the current expenses of the Government, in which the Commissioner of Agriculture was directed to appoint a person for prosecuting these inquiries relating to Forestry, his powers and duties being similar to those specified in the Bill just mentioned. This appointment was given to Dr. Franklin B. Hough, and on the 8th of December, 1877, he presented to the Commissioner of Agriculture a Report upon a part of the subjects specified in the act under which this appointment was made. It was transmitted to Congress by the President on the 13th of December, and referred to the House Committee upon Agriculture.

An edition of 25,000 copies of this Report was ordered, but with a restriction introduced by the Printing Committee, which limited its extent to 650 pages, and compelled the omission of a part of the Report.[4]

At the meeting of this Association held at St. Louis, in 1878, a second Report, prepared by the Chairman of your Committee, was presented, in which further details are given of these proceedings.[5] It also embodies his views concerning the subject of Forestry, as applied to the United States, and recites in detail the measures already adopted in various states for the encouragement of planting, with such suggestions as to him appeared most effectual for the promotion of these interests.

The Commissioner of Agriculture, deeming the authority granted under the act above mentioned, as sufficient, gave directions for a continuance of these inquiries, and found means from the funds under his control for defraying the expenses. A second

[3] Proceedings of A. A. A. S., 23d meeting, Aug., 1874, pp. 36 to 45.

[4] Report upon Forestry, prepared under the direction of the Commissioner of Agriculture, pursuant to an Act of Congress, approved August 15, 1876. By Franklin B. Hough. 8vo. pp. 650.

[5] Proceedings of A. A. A. S., 27th meeting, St. Louis, pp. 29 to 40.

Report was presented in January, 1879, and transmitted to Congress by the President early in February of that year. From various causes, wholly foreign to the subject, but chiefly from the occupation of the members of the Committee upon Agriculture (to which it was referred), in other business more urgent, no session was held by the Committee, at which a hearing could be obtained, from the time when this Report was laid before them till the end of the session. In fact, the Committee met but once for business during this time, and then it was wholly occupied upon another subject. The Report was therefore withdrawn at the last moment of the session, and returned to the Department.

Early in 1880, a Resolution was introduced in Congress, calling for a return of the Report above mentioned, and it was again transmitted by the President, and at once ordered to be printed.

The Committee upon Agriculture, to whom it was referred, gave the subject a prompt and attentive hearing, and agreed unanimously to recommend the publication of 100,000 extra copies. This resolution was still pending when Congress adjourned, and lies over till next session. In the meantime, the stereotyping has been going forward to completion, making a volume of 618 pages.

By the terms of law under which these labors have been done, the Commissioner of Agriculture was directed, among other things, to cause a Report to be made upon the "Importation and Exportation of Timber and other Forest Products;" and the second Report upon Forestry will be found to embrace an exhaustive statement of the subject in its commercial aspect. It gives a complete summary of the exportation and importation of forest products and wood manufactures, through the period of ninety years, beginning with the government in its present form in 1789, and coming down to the close of the last fiscal year, June 30, 1879, as reported annually by the Secretary of the Treasury from the returns of Collectors of Customs. The arrangement of the tables will be found as uniform as the data would permit, and ample summaries, percentages and deductions from these tables have been prepared.

The intimate relation between the timber interests of the United States and of Canada, scarcely allows us to consider one without taking into account the other. It is well known that for many years, the lumber products of Canada have found an extensive

and ready sale within the United States, and at the same time, that a large amount of the hewn timber from within the United States, in the country bordering upon our northern lakes, and intended for foreign markets, has been taken down the lakes and the St. Lawrence, for shipment at Quebec. In fact, there has always existed a certain community of interest in the forests, between the inhabitants of the United States and of Canada, and it may be reasonably predicted, that, so long as lumber and timber are produced in either of these countries, they will go to supply the wants of both.

It has therefore been deemed advisable, in presenting a full view of the commerce in wood-products for the United States, and in order that this branch of the subject should be as complete as possible, to enter upon a careful study of the Canadian timber trade from data published by the Dominion and the Provincial Governments, beginning with the date of Confederation in 1867, and coming down to the end of the last fiscal year. In some branches of this inquiry, the statistics are carried back to an earlier period, especially in regard to the passage of timber through the government slides and canals, and the inspection of lumber in the timber-market of Quebec.

A careful analysis has been made of the results of inquiries instituted in recent years by committees of the Dominion Parliament, with the view of ascertaining the condition and future prospects of the timber resources of the country, and the means best adapted for maintaining these supplies, and for economizing their use.

A full account is also given of the systems of management, with respect to the timber upon the Crown Lands, the results of experience in former methods, and the modifications which this experience has pointed out, in perfecting the systems now in use. In this we may find suggestions applicable in some instances to the public lands of the United States, in the methods of conservation and management that may be hereafter adopted upon our public domain; in fact, from a similarity of circumstances, we have more to learn from the experience gained in American forests than from the Forest Administrations of Europe, where the tenure of the lands, the systems of government, forest servitudes and the rights of communes and of individuals, are so widely different.

The Crown Lands of Manitoba and of the Northwestern Territories of Canada belong to the Dominion Government and are in care of the Department of the Secretary of State, in a branch of which there is a "Dominion Lands Office."

In other parts of Canada, the Crown Lands belong to the Provinces in which they lie, and separate systems of management are provided. In Ontario and Quebec these lands are in charge of a Commissioner of Crown Lands; in New Brunswick they are under the Surveyor General; in Nova Scotia, under the Attorney General; and in British Columbia, under a Chief Commissioner of Lands and Works.

In perfecting the Report, application was made personally, or by correspondence, to the several Departments of the general and local governments of Canada, having official cognizance of the subject, and full series of every class of Reports bearing upon the forest-question were collected from them all. More than this, substantial assistance was furnished in several cases, in the way of copies of documents not published, and manuscript statements explanatory of the reports. In short, these inquiries met with the most courteous attentions, indicating the great interest which the question of timber-supply is awakening in Canada, and affording the ground for hope, that effectual measures will ere long be realized in that country for turning their remaining resources to the best account.

If they will go a step farther, and secure adequate means for re-production, the wants of the future will be assured. As an indication that one of the means most certain to secure this desirable end is well understood, we may mention, that in the Province of Quebec, it is no longer permitted to cut pine trees upon the public lands, measuring less than twelve inches in diameter at the stump. If they could, besides this, secure adequate measures for the prevention of forest fires and other waste, and apply the reservations to other kinds of timber, and especially the spruce, they would have a useful system of Forestry in operation.

For this second Report that we are describing, a series of graphic illustrations was prepared by a skilful draftsman at the time when the statistical tables were first submitted to the Department. They would fill about a hundred pages, and it was intended to reproduce them by a photo-engraving process. The addition of three more years, since made to these tables, would render it

necessary to reconstruct these drawings, which cannot now be done, at least for the first issue of the Report.

Besides the statistical details and generalities above mentioned, the Report under notice embraces the recent legislation of the General, the State and the Territorial Governments, upon the subject of timber-planting and other interests of Forestry, with statements relating to the operation of these laws.

It is a significant fact, that in the three States where planting is perhaps of greatest need, viz.: in Kansas, Nebraska and Iowa, the premiums formerly offered for planting trees upon prairie lands were, after a few years' experience, wholly or in part, withdrawn. This should not be regarded as a sign that the interest in this subject is declining—but rather that a realization of the necessity for plantations is increasing; and that, in fact, it has become so evident and convincing, that it does not need a motive, beyond the pecuniary interest of the owner of the land.

Furthermore, these acts, intended for encouragement, have everywhere led to fraudulent claims for premiums and exemptions, the work of planting being often done in the most superficial and indifferent manner, by those whose chief object was to secure the present benefit of these laws, rather than the more remote but substantial profits of a successful timber-growth.

In the state of Nebraska, after repealing a law extravagantly liberal in its provisions, and remaining some years without any, the Legislature has recently enacted a new statute upon this subject which, after the experience of the past, may be presumed to approach nearly the standard of greatest utility at the least expense. It is entitled "An Act to encourage the Growth and Cultivation of Timber in the State of Nebraska," and was approved February 21, 1873. Its leading provisions are as follows:

[illegible] and one-half dollars per acre is to be paid [illegible] to the extent [illegible] of six rows of [illegible]

[illegible]

and circumstances of some localities might render protection desirable from other points besides the north, especially in the remote prairie states, that often suffer from the dry and heated winds from the southwest.

It would be better to leave the direction and density of these timber-belts to the discretion of the planter, as his circumstances might indicate.

By another statute of recent date, the State of Nebraska, in pursuance of the authority expressed in its Constitution, now exempts, from taxation, the *increased value* of lands by reason of live-fences, fruit and forest trees grown and cultivated thereon, without regard to the area planted, or the time that the plantation is maintained. We deem this a simple and effectual mode of encouragement, well worthy of careful trial in the prairie states; but there should evidently be some limit to the period of exemption, to prevent the public burdens from bearing unequally upon property, for otherwise there would eventually arise a just cause of complaint from other interests. The limit might perhaps be extended to the time when the plantation would begin to bring a revenue to the owner.

We may here remark, that this accumulation and comparison of experience in the legislation of the country, for the benefit of forest-culture, may prove one of the most effectual means for bringing about that uniformity and that degree of excellence that secure the best results. We should profit from experience, where it leads to error, not less than when it is crowned with success.

Without further notice of this second Report, which will soon be before the public, we will only add, that Congress at its late session, after due deliberation, made provision for the continuance of these labors;—that plans for special investigations in several departments of Forestry are in progress, and that a third Report is in course of preparation.

It will be seen from the foregoing, that prominence has been given, in the Report now in press, to the practical interests of the lumber trade. Although the question of supply is one of vital importance to the country,—the one wherein most is demanded, and the only one in which the country generally will take great interest; it should be steadily borne in mind, that the best service

for the promotion of the material interests of Forestry can be rendered through the aid of science. The prominent duties of the branch of the public service, having charge of this subject, should therefore be to advance, by every means of inquiry and research within its power, our knowledge of the laws of nature concerned in forest-growth, and carefully to investigate the various circumstances that lead to best results. We should have definite knowledge of the casualties that may happen, and the means by which they may be mitigated or avoided, and upon all occasions seek to utilize the discoveries of science, wherever they can be applied.

In the national census of the present year, provision is for the first time made for a special investigation of our forest resources, and in the hands of Professor C. S. Sargent and his assistants, we may expect that this work will be well done.

The only State Forestry Association, yet organized, is that of Minnesota, which is doing good service in the promotion of tree-planting in the prairie regions of that State. The project of a School of Forestry has been proposed in Minnesota, and a land-grant for its endowment has been asked from Congress,—but hitherto, as we understand, without success.

We deem it of the first importance that a better knowledge of the principles of arboriculture should be more widely diffused. It remains a question of the future, as to how far a special education in this branch of applied knowledge would, at present, find adequate remuneration, unless associated with capital, and sustained by an intelligent appreciation of the importance of forest culture, by those having money to invest in this enterprise, which is as yet too seldom found.

In speaking of the results of associated effort, in the interest of sylviculture, we should not fail to notice our State Agricultural and Horticultural Societies, many of which now recognize the importance of this subject, and admit discussions bearing upon tree-culture, at their meetings and in their published Transactions. This is particularly the case in Iowa, where much prominence is given to questions relating to the planting of groves and hedges, and premiums are offered tending to improvements in management, and extension of amount done. In some of the older States, and especially in Massachusetts, much attention has been given,

in recent years, to this subject; but in none of the states have we seen statistics of the results obtained by the offer of these premiums of the later period.

Within the last three years, the Secretary of the Interior, in whose Department is vested the care of the Public Lands, has been making commendable efforts to arrest the unlawful cutting of timber upon these lands. In this he has met with great opposition from interested parties, through the political and other influences that they have been able to bring in Congress, and from delays in the courts.

The shameful extent to which these depradations have been going on, through a long period of years, and in fact, until the practice had gained from long indulgence the semblance of a right, will hereafter be looked upon as a striking evidence of the barbarism of the age.

These efforts to repress a criminal practice, long sanctioned by *custom*, have led to recent legislation in Congress upon the timber question, some of which will tend to increase rather than prevent the waste we have described. We refer particularly to an act passed two years since, granting to the inhabitants of certain regions, where conservation is of the first importance, the unrestrained use of timber upon the public domain, for all mining and domestic purposes, without so much as the pretext of a report as to the amount taken, the least check upon its limit, or the least payment for the privilege.[6]

Among these recent statutes relating to our public domain, we still look in vain for the first indication of a policy tending to provide for future supplies by reservation of timber lands — except to limited extent and for naval use alone. We find no limitation in the cutting of small trees, and no protection of lands with the view of affording a new growth of timber, where it has been taken away; much less do we yet find any measures for planting upon the public lands, or any stipulations requiring this to be done by settlers, except in the still recent timber-culture acts, that have as yet scarcely passed through the trial of experience.

The first of these timber-planting acts was passed in 1873, and amendments have been since made, as deemed necessary. If faithfully administered, the law cannot fail to prove of inestimable

[6] "An act authorizing the citizens of Colorado, Nevada and the Territories, to remove Timber on the Public Domain, for mining and domestic purposes." Approved June 3, 1878.

value to the prairie states. According to this act as it now stands, a man may acquire the absolute title to a quarter-section, or 160 acres of land, by planting and cultivating ten acres of timber, there being not less than 2,700 trees to the acre. The patent is not issued until eight years after the claim is entered. The mode of preparation of the land is prescribed, and proof of successful planting must be shown.

According to the latest Reports, 5,157,681 acres had been entered under this act within three years, chiefly in Kansas, Dakota, Nebraska and Minnesota. Undoubtedly some of these lands have been taken up for speculative purposes, and as little labor as possible will be spent upon them in the way of planting and improvement, while in many other instances, the undertaking is assumed in good faith, and with a desire to realize as much profit as possible from the cultivation.

Passing from these statements relating to American Forestry, let us notice some facts of interest bearing upon the subject in Europe. As is well known, every government of continental Europe has now in operation a system of forest management, the best of which provides (so far as concerns the forests owned by general and local governments, and by institutions) for a perpetual supply, to the full limit of their capacity for timber-growth.

From this grade of excellence, requiring a special education and thorough training of agents for its maintenance, fully equal to that for any branch of the public service, we have various degrees of efficiency down to that of a mere police regulation for the prevention of fires, and the restraint of waste upon timber on the public domain. Yet these systems, however they may differ in details, agree in this:—that unless the public interest is concerned, the owners of private estates are generally allowed to cut or plant upon their own premises, as their interest decides. The exceptions to this rule are,—along a frontier, where woodlands are needed for the public defence, the banks of a river liable to inundation—where materials should be at hand for the construction of barriers,—upon mountains liable to erosion of torrents, or on drifting sands on the seashore. There may be a few other exceptions, but as a general rule, the government does not often interfere with the timber upon private estates, even where it requires a notice of intention to be given before clearing is begun.

Yet upon these private lands, large forests are sometimes grown

for profit, and their management is often placed in the care of agents who have received the highest grade of special education for this particular service. Except in Great Britain, we believe that facilities have been provided for this special instruction in every country in Europe, either in academies where Forestry is taught alone, or in institutions where agriculture and other practical industries receive a share of attention.

It may be proper to notice here a change that has been taking place in recent years, in the organization of these Schools of Forestry in Europe. In Austria, a first class School of Forestry at Mariabruun was, after more than forty years' existence, merged in 1875 in a High School of Agriculture and Forestry in Vienna. In Bavaria, the Central Forest Academy at Aschaffenburg, still older than the one above mentioned, has been more recently united with the University of Munich, and discussions tending to further changes with the view of consolidation are now in progress in other forest academies. In Prussia, the two institutions at Eberswalde and at Münden still maintain a separate existence, and the former has recently commemorated the fiftieth year of its history.

In looking at the organization of these institutions, we notice a marked change in their plan, particularly in Germany. In the preparatory studies, and especially in the natural sciences and in physics, so far as they in the least concern the forester, we find a more careful division of labor, and a more earnest purpose to make these sciences to their whole extent available in their profession, instead of the elementary studies in chemistry and botany which formerly satisfied the requirements for graduation.

In all of these institutions, excursions and practical exercises form a regular feature in the course of education, and microscopic studies now receive much more attention than formerly.

The science at present receives a substantial support from various experimental stations in Germany, Austria, France and other countries, in which both practical and scientific questions are carefully investigated, and the results published. Among these we may prominently mention the Foresters' Experimental Union in Germany, and the experimental labors of the Austrian Ministry of Agriculture, under the direction of Baron von Seckendorff.

In recent years much interest has been manifested among in-

vestigators in Europe, in determining by instrumental observations, made at comparative stations in the open fields and in the forests, the influence of the latter upon the atmosphere in their vicinity, and upon the climate of the country generally.

That forests tend to render the climate of a country colder and more humid, has been known from general observation for many centuries; and the climate of Gaul, as described by Julius Cæsar, compared with that of France at the present day, has been often cited as an instance of this fact. Yet it is only within a very recent period that anything like a full and systematic comparison has been attempted by instrumental observations.[7]

In selecting the location of these stations, one set of instruments is placed in the open fields, and the other in the woods, the former being as much as possible free from the influence of the forest, and the latter fully under its protection. Care is taken that the soil, altitude and other circumstances, except the forest-shade, are alike.

Their outfit generally consists of open-air thermometers — psychrometers — rain and snow gauges, barometers, wind-vanes, and instruments for observing the evaporation from surfaces variously covered as compared with a free water surface — the percolation of water through various soils, ozone records, and observations upon the periodical phenomena of animal and vegetable life generally, so far as influenced by the seasons. In some cases the temperature of the interior of trees is recorded, and in some of the later systems of observation, it has been proposed to ascertain the temperature and humidity not only at the level of the tree-tops, but at a considerable height above them, the latter being obtained by the aid of masts, and by captive balloons that admit of the elevation of instruments.

Without going into details of the results of these observations, we may here remark, that records more or less complete in their plan have been established as follows: —

In *Saxony*, since 1862–3, under the direction of Professor Krutzsch, of Tharand, at nine stations, embracing records of temperature, humidity, rain, snow, etc.

In *Bavaria*, since 1868, seven stations have been established

[7] An account of investigations in this line down to 1872 was published in that year by the Baron von Löffelholtz-Colberg, under the title of "*Die Bedeutung und Wichtigkeit des Waldes.*"

under Dr. Ernst Ebermayer, who in 1873 published very fully the results of four years' records,[8] and in 1876 another work upon the formation of soil from the litter of woodlands, and the chemical statics of forest culture.[9] It was his intention at the end of ten years to publish the results of that period in more extended form, but his recent removal from Aschaffenburg to Munich has apparently postponed, if it has not disappointed, this expectation.

In *Prussia*, a system of observations, nearly similar to those in Bavaria, was begun in 1874 under Professor A. Müttrich, of Eberswalde, and now includes fourteen stations in Prussia, Brunswick and Alsace-Lorraine. The results are published monthly[10] and annually,[11] and a careful study of so much as relates to the temperature of the soil has recently appeared.[12]

In *France*, observations upon the rainfall in the woods and open fields were made under the direction of Marshal Vaillant, in 1866, near Paris, and published in the *Atlas Meteorologique* of the Imperial Observatory of Paris for 1867. In the same year, observations of more extended character were commenced at two forest stations and one agricultural station in the vicinity of Nancy, near the eastern border of France, by Professor A. Mathieu of the school of Forestry at Nancy, and have since been continued. A summary of eleven years' observations was published in 1878.[13]

Upon a general suggestion of the Congress of Agriculturists and Foresters at Vienna, in 1873, M. Fautrat, a sub-inspector in the French Forest Service, with the approval and aid of the Department, established two stations for comparative observations, one in the deciduous forest of Halatte near Fleurines, and

[8] Die physikalischen Einwirkungen des Waldes auf Luft und Boden, und seine klimatologische und hygienische Bedeutung, 1 Vol., 8vo, pp. 266 and 251 (with an atlas). Berlin, 1873.

[9] Die gesammte Lehre den Waldstreu, mit Rücksicht auf die chemische Statik des Waldbaues, I Vol., 8vo, pp. 300 and 116. Berlin, 1876.

[10] Beobachtungs Ergebnisse der im Königreich Preussen, im Herzogthum Braunschweig und in den Reichslanden eingerichteten forstlich-meteorologischen Stationen, 1 bis 5 Jahrgang, 1875–1879.

[11] Jahresbericht über die Beobachtungs-Ergebnisse der im Königreich Preussen und in den Reichslanden eingerichteten forstlich-meteorologischen Stationen. 1 bis 4 Jahrgang, 1875–1878.

[12] Beobachtungen der Erdbodentemperatur auf den forstlich-meteorologischen Stationen, in Preussen, Braunschweig und Elsass-Lothringen. Von A. Müttrich. Separat-Abdruck, aus der Festschrift zur 50 Jahrigen Jubelfeier der Forstakademie, Eberswalde. 4to. pp. 33. Berlin, 1880.

[13] Météorologie Comparée, Agricole et Forestière, 4to, Paris, pp. 70. Published by the Forest Administration, in connection with the Paris Exposition of 1878.

and ready sale within the United States, and at the same time, that a large amount of the hewn timber from within the United States, in the country bordering upon our northern lakes, and intended for foreign markets, has been taken down the lakes and the St. Lawrence, for shipment at Quebec. In fact, there has always existed a certain community of interest in the forests, between the inhabitants of the United States and of Canada, and it may be reasonably predicted, that, so long as lumber and timber are produced in either of these countries, they will go to supply the wants of both.

It has therefore been deemed advisable, in presenting a full view of the commerce in wood-products for the United States, and in order that this branch of the subject should be as complete as possible, to enter upon a careful study of the Canadian timber trade from data published by the Dominion and the Provincial Governments, beginning with the date of Confederation in 1867, and coming down to the end of the last fiscal year. In some branches of this inquiry, the statistics are carried back to an earlier period, especially in regard to the passage of timber through the government slides and canals, and the inspection of lumber in the timber-market of Quebec.

A careful analysis has been made of the results of inquiries instituted in recent years by committees of the Dominion Parliament, with the view of ascertaining the condition and future prospects of the timber resources of the country, and the means best adapted for maintaining these supplies, and for economizing their use.

An account is also given of the systems of management, with reference to the timber upon the Crown Lands, the results of experience in forest methods, and the modifications which this experience has pointed out, in perfecting the systems now in use. [illegible] suggestions applicable in some instances to the [illegible] United States, in the matters of conservation [illegible] may be derived and acted upon our public [illegible] and [illegible] we have [illegible] the experience gained in American forests [illegible] Forest [illegible] where the tenure [illegible] systems of government forest services [illegible] and [illegible] are so widely [illegible]

The Crown Lands of Manitoba and of the Northwestern Territories of Canada belong to the Dominion Government and are in care of the Department of the Secretary of State, in a branch of which there is a "Dominion Lands Office."

In other parts of Canada, the Crown Lands belong to the Provinces in which they lie, and separate systems of management are provided. In Ontario and Quebec these lands are in charge of a Commissioner of Crown Lands; in New Brunswick they are under the Surveyor General; in Nova Scotia, under the Attorney General; and in British Columbia, under a Chief Commissioner of Lands and Works.

In perfecting the Report, application was made personally, or by correspondence, to the several Departments of the general and local governments of Canada, having official cognizance of the subject, and full series of every class of Reports bearing upon the forest-question were collected from them all. More than this, substantial assistance was furnished in several cases, in the way of copies of documents not published, and manuscript statements explanatory of the reports. In short, these inquiries met with the most courteous attentions, indicating the great interest which the question of timber-supply is awakening in Canada, and affording the ground for hope, that effectual measures will ere long be realized in that country for turning their remaining resources to the best account.

If they will go a step farther, and secure adequate means for re-production, the wants of the future will be assured. As an indication that one of the means most certain to secure this desirable end is well understood, we may mention, that in the Province of Quebec, it is no longer permitted to cut pine trees upon the public lands, measuring less than twelve inches in diameter at the stump. If they could, besides this, secure adequate measures for the prevention of forest fires and other waste, and apply the reservations to other kinds of timber, and especially the spruce, they would have a useful system of Forestry in operation.

For this second Report that we are describing, a series of graphic illustrations was prepared by a skilful draftsman at the time when the statistical tables were first submitted to the Department. They would fill about a hundred pages, and it was intended to reproduce them by a photo-engraving process. The addition of three more years, since made to these tables, would render it

dentally the effect of woodlands in maintaining the amount, we may notice as particularly worthy of attention, the publications of Counsellor Gustavus Wex,[18] overseer of the works of improvement in the Danube, at Vienna. These memoirs are not only of interest from the facts that they embody, but also for their citations to other works, making them in some degree, a bibliography of the subject. They have been referred to commissions appointed by various academies of science, and the conclusions that they present have been approved by the highest authorities. They in fact confirm, by numerical statements, the observations of every one who has had an opportunity to notice the amount of water flowing in streams before and after the clearing of the district in which they rise, and through which they run. This amount affects the interests concerned in the hydraulic power of the larger streams, the supply of water to cities, and the navigation of rivers and canals.

As this diminution of water-supply is the result of man's acts, so also in a great degree is the remedy of control within his power, and by means of planting and with reservoirs he can often recover and maintain the advantages lost, perhaps as fully as they existed under the primeval forests.

Among the means for the advancement of forest-science in Europe, we might mention as worthy of notice, the growing interest in various societies and associations, formed for the discussion of new questions as they arise, the comparison of experience, and the promotion of that personal acquaintance so conducive to the general welfare. These meetings are usually enhanced in interest by excursions to forests affording an illustration of methods, or other subjects of practical interests. The more important of the essays and discussions, which these meetings call out, are published with their official proceedings, and through the medium of these publications, and the various journals of forestry, the more important discoveries become widely known.

Returning to the original resolution under which the Committee upon Forestry was appointed by this Association, in 1873, there

[18] *Ueber die Wasserabnahme in den Quellen, Flüssen und Strömen, bei gleichzeitiger Steigerung der Hochwässer in den Culturländern*, von GUSTAVUS WEX, K. K., Ministerialrath und Oberlautier der Donauregulirung bei Wien. 4to, pp. 43, with 7 plates.

Zweite Abhandlung, etc., 4to, pp. 30, with 6 plates.

There are separate imprints from the *Zeitschrift des österreichischen Ingenieur, und Architekten-Vereins*, 1873 and 1879.

and circumstances of some localities might render protection desirable from other points besides the north, especially in the remote prairie states, that often suffer from the dry and heated winds from the southwest.

It would be better to leave the direction and density of these timber-belts to the discretion of the planter, as his circumstances might indicate.

By another statute of recent date, the State of Nebraska, in pursuance of the authority expressed in its Constitution, now exempts, from taxation, the *increased value* of lands by reason of live-fences, fruit and forest trees grown and cultivated thereon, without regard to the area planted, or the time that the plantation is maintained. We deem this a simple and effectual mode of encouragement, well worthy of careful trial in the prairie states; but there should evidently be some limit to the period of exemption, to prevent the public burdens from bearing unequally upon property, for otherwise there would eventually arise a just cause of complaint from other interests. The limit might perhaps be extended to the time when the plantation would begin to bring a revenue to the owner.

We may here remark, that this accumulation and comparison of experience in the legislation of the country, for the benefit of forest-culture, may prove one of the most effectual means for bringing about that uniformity and that degree of excellence that secure the best results. We should profit from experience, where it leads to error, not less than when it is crowned with success.

Without further notice of this second Report, which will soon be before the public, we will only add, that Congress at its late session, after due deliberation, made provision for the continuance of these labors;—that plans for special investigations in several departments of Forestry are in progress, and that a third Report is in course of preparation.

It will be seen from the foregoing, that prominence has been given, in the Report now in press, to the practical interests of the lumber trade. Although the question of supply is one of vital importance to the country,—the one wherein most is demanded, and the only one in which the country generally will take great interest; it should be steadily borne in mind, that the best service

for the promotion of the material interests of Forestry can be rendered through the aid of science. The prominent duties of the branch of the public service, having charge of this subject, should therefore be to advance, by every means of inquiry and research within its power, our knowledge of the laws of nature concerned in forest-growth, and carefully to investigate the various circumstances that lead to best results. We should have definite knowledge of the casualties that may happen, and the means by which they may be mitigated or avoided, and upon all occasions seek to utilize the discoveries of science, wherever they can be applied.

In the national census of the present year, provision is for the first time made for a special investigation of our forest resources, and in the hands of Professor C. S. Sargent and his assistants, we may expect that this work will be well done.

The only State Forestry Association, yet organized, is that of Minnesota, which is doing good service in the promotion of tree-planting in the prairie regions of that State. The project of a School of Forestry has been proposed in Minnesota, and a land-grant for its endowment has been asked from Congress,—but hitherto, as we understand, without success.

We deem it of the first importance that a better knowledge of the principles of arboriculture should be more widely diffused. It remains a question of the future, as to how far a special education in this branch of applied knowledge would, at present, find adequate remuneration, unless associated with capital, and sustained by an intelligent appreciation of the importance of forest culture, by those having money to invest in this enterprise, which is as yet too seldom found.

In speaking of the results of associated effort, in the interest of sylviculture, we should not fail to notice our State Agricultural and Horticultural Societies, many of which now recognize the importance of this subject, and admit discussions bearing upon tree-culture, at their meetings and in their published Transactions. This is particularly the case in Iowa, where much prominence is given to questions relating to the planting of groves and hedges, and premiums are offered tending to improvements in management, and extension of amount done. In some of the older States, and especially in Massachusetts, much attention has been given,

in recent years, to this subject; but in none of the states have we seen statistics of the results obtained by the offer of these premiums of the later period.

Within the last three years, the Secretary of the Interior, in whose Department is vested the care of the Public Lands, has been making commendable efforts to arrest the unlawful cutting of timber upon these lands. In this he has met with great opposition from interested parties, through the political and other influences that they have been able to bring in Congress, and from delays in the courts.

The shameful extent to which these depradations have been going on, through a long period of years, and in fact, until the practice had gained from long indulgence the semblance of a right, will hereafter be looked upon as a striking evidence of the barbarism of the age.

These efforts to repress a criminal practice, long sanctioned by custom, have led to recent legislation in Congress upon the timber question, some of which will tend to increase rather than prevent the waste we have described. We refer particularly to an act passed two years since, granting to the inhabitants of certain regions, where conservation is of the first importance, the unrestrained use of timber upon the public domain, for all mining and domestic purposes, without so much as the pretext of a report as to the amount taken, the least check upon its limit, or the least payment for the privilege.[6]

Among these recent statutes relating to our public domain, we still look in vain for the first indication of a policy tending to provide for future supplies by reservation of timber lands — except to limited extent and for naval use alone. We find no limitation in the cutting of small trees, and no protection of lands with the view of affording a new growth of timber, where it has been taken away; much less do we yet find any measures for planting upon the public lands, or any stipulations requiring this to be done by settlers, except in the still recent timber-culture acts, that have as yet scarcely passed through the trial of experience.

The first of these timber-planting acts was passed in 1873, and amendments have been since made, as deemed necessary. If faithfully administered, the law cannot fail to prove of inestimable

[6] "An act authorizing the citizens of Colorado, Nevada and the Territories, to remove Timber on the Public Domain, for mining and domestic purposes." Approved June 3, 1878.

newer States, there are no lands whatever, adapted to this use, now belonging to either the State or the Local Governments — the whole being owned by private citizens, and subject to no conditions, but such as their Representatives in a Legislative capacity may establish for the general welfare.

It is to the Owners of the Land, that we must therefore look for the adoption of measures tending to avert the injuries in prospect; and in furtherance of this end, we deem it within the province of a State Legislature to encourage the enterprise, which may be done in a good degree in the following manner:

1. By a Law protecting Trees planted along the Highways, and by encouraging such plantations by deductions from Highway Taxes. It may, in particular cases, be proper to require such plantations to be made at the public expense, with the view of protecting roads from drifting snows, or for other local benefits, and in a manner best calculated for securing these objects.

2. By a Law that shall exempt from taxation the increased value of land, from the planting of trees, where none were growing, for such period as may appear proper, or until some profit may be realized from the plantation.

3. By the appropriation of money to Agricultural and Horticultural Societies, to be applied as premiums for tree-planting, regard being had to greatest areas planted, and the most successful management. Reports should be required, giving details of the methods found most effectual in obtaining these results.

4. By prizes for the best Essays and Reports upon subjects of practical Forest-culture, to be awarded by competent Judges, and those approved, to be published for distribution among those who would be the most benefited by this information.

5. By encouraging Educational Institutions within the State to introduce a course of instruction, having reference to practical sylviculture. This object may be promoted by the aid of Collections, and by correctly labelled Plantations of the various species of Forest Trees adapted to the soil and climate. At Agricultural Colleges, and at higher Institutions of Learning, stations might be advantageously established under State patronage, for experiments and observations in cultivation and acclimatization. The distribution of seeds and plants affords a direct and efficient aid, in the promotion of an interest in this subject.

6. By laws tending to prevent Forest-fires, by imposing penal-

ties against the wilful or careless setting of such fires, and by enlarging and defining the powers of Local Officers in calling for assistance, and in adopting measures for suppressing them. The waste from this cause, in some years, greatly exceeds the amount of timber used, and there is no question connected with forest supplies, that demands more serious attention. Our main reliance appears to depend upon vigilant precautions, enforced by adequate penalties, and sustained by a strong public sentiment.

7. Under favorable circumstances, Model Plantations might be established and maintained by a State Government, under the care of persons specially trained to the profession of Forestry. Their location should be chosen with a view of affording convenient opportunity to those who might wish to learn approved methods of management, by the study of a work worthy of imitation.

8. The appointment of a Commission of Forestry under State authority (analogous to the Commission of Fisheries in many of the States) might prove of great service in promoting efficient measures for the advancement of this interest. The Members of such a Commission, who would doubtless be selected on account of their influential standing, and their known intelligence upon this subject, would be able to study the conditions and requirements of their State, and devise means for most effectually securing the object in view.

In the questions arising upon this subject, we depend much upon the intelligence of our fellow citizens, who are generally not slow to appreciate advantages, or to foresee a real danger where the indications are apparent. When this danger is fully realized, we believe that no time will be lost in seeking to apply the remedy. The measures we recommend will tend to awaken an interest in the subject, and lead to an intelligent understanding of the means for meeting the dangers that may arise from undue exhaustion of our forest supplies. They will diffuse the benefits gained by experience, for the good of all, and educate public opinion to a degree that will sustain more energetic measures, as their necessity may be hereafter more fully known.

Chairman of the Committee,
A. A. A. S.

Report of Committee on Science Teaching in the Public Schools.

The Committee appointed at the Saratoga Meeting of the American Association, on Science Teaching in the Public Schools, respectfully submit the preliminary Report.

The repeated appointment, by this body, in successive years, of committees to look into the scientific education of the public schools, must be taken as showing that such an inquiry is regarded as both legitimate and important. Yet the duties of such a committee have not been defined by the Association, nor have any of our predecessors opened the way to a consideration of the subject. It was probably expected that we would furnish a digest of information from many quarters, as to what sciences are taught in the public schools, with what facilities, and to what extent; accompanied by such recommendations regarding the increase of scientific studies as the results might suggest. But our course has not proved to be so clear. We have been arrested at the outset by a question of the quality of the science teaching in these schools which demands the first consideration. There are certain radical deficiencies in current science teaching, the nature and extent of which must be understood before any measures of practical improvement can be intelligently taken up. We shall here confine ourselves to this preliminary inquiry.

The investigation has interest from the immense extent, and rapidly increasing influence, of the American public schools. There are now nearly a hundred and fifty thousand of these schools, supported at an annual expense of probably seventy or eighty million dollars. Maintained by state authority, they are firmly established in the respect and confidence of the community. Under the influence of normal schools, teachers' institutes, systematic superintendence, school boards, regulative legislation, and an extensive literature devoted specially to education, they have become organized into a system which is gradually growing settled and unified in its methods. With unbounded means and unlimited authority, these schools have undertaken to form the mental habits of the great mass of the youth of this country. They prescribe the subjects of study, the modes of study, and the extent and dura-

tion of studies for all the pupils that come under their charge. The sphere of their operations is, moreover, steadily extending. They are everywhere encroaching upon the province of higher education, everywhere trenching upon private schools and diminishing the interest in home education.

It may be assumed that the time has fully come when this system must be measured by the standards of science, and approved or condemned by the degree of its conformity to what these standards require. Science has become in modern times the great agency of human amelioration, the triumphs of which are seen on every hand and felt in all experience. Grave subjects are brought successively under its renovating and reconstructive influence; and latest and most important among them is the subject of education. Our inquiry now is how far the public school system has availed itself of the valuable aid that science offers in the proper cultivation of the minds of the young.

The interest and necessity of such an investigation will hardly be denied; but there may be a query as to its relevancy to the appropriate work of this society. The making of science popular was not among the objects for which our association was formed. Not that its founders were unmindful of the importance of widely diffusing the results of research; but they recognized that the interests of science are so vast, as to be only efficiently promoted by division of labor. Under the operation of this principle it was made the distinctive purpose of the association to contribute to the extension of original science by the discovery of new scientific truth, leaving its dissemination to the schools, the press, and the various agencies of public enlightenment. Nor does your committee understand that it is now proposed to depart from this policy; for the inquiry before us is really most pertinent to our special objects. It certainly cannot be a matter of indifference to this body, from its own point of view, how science is dealt with in the great system of schools which has undertaken the task of moulding the youthful mind of the country. We aim to advance science by the promotion of original investigation, which depends upon men prepared for the work; do the schools of the nation, by their modes of scientific study, favor or hinder this object? Do they foster the early mental tendencies that lead to original thought; or do they thwart and repress them? We have an undoubted concern in this matter, and it is, moreover, strictly iden-

tical with that of the community at large; for there can be no better test than this of the real character of a school system. When we ask whether a mode of teaching and a manner of study are calculated to awaken the spirit of inquiry, to cultivate the habit of investigation, and rouse independent thought, our question goes to the root of all true education.

All sciences are the products of a method of thinking, and it is that method which concerns us when we propose to regard it as a means of mental cultivation. Science is an outgrowth of common knowledge, and the scientific method is but a development of the ordinary processes of thought that are employed by everybody. The common knowledge of people is imperfect because their observations are vague and loose, their reasoning hasty and careless, their minds warped by prejudice and deadened by credulity, and because they find it easier to invent fanciful explanations of things than to discover the real ones. For thousands of years the knowledge of nature was rude and stationary because the habits of thought were so defective. But with a growing desire to understand how the world around is constituted, men improved their processes of thinking. They began, and were compelled to begin, by questioning accepted facts, and doubting current theories. The first step was one of self-assertion, implying that degree of mental independence which led men to think for themselves. They learned to make their own observations and to trust them against authority. It was found, as a first and indispensable condition of gaining clear ideas that the mind must be occupied directly with the subject to be investigated. In this way scientific inquiry at length grew into a method of forming judgments which was characterized by the most vigilant and disciplined precautions against error. Of the mental processes involved in research it is unnecessary here to speak; we are only concerned to know that the scientific method is simply a systematic exercise in truth-seeking, and is the only mode of using the human mind when it is desired to attain the most accurate and perfect form of knowledge. The whole body of modern scientific truth, disclosing the order of nature and guiding the development of civilization, must be taken as an attestation of the validity of the scientific method of thought by which these results have been established. We here get rid of all cramping limitations. The scientific method is applicable to all subjects whatever that involve con-

stancy of relations, causes and effects, and conform to the operation of law. It is applicable wherever evidence is to be weighed, error got rid of, facts determined, and principles established. Our public schools, unhappily, make but little use of this method in the work of mental cultivation, and we shall find some explanation of this by referring to the way they grew up.

The American public school system originated in the theory that the state owes to every child the rudiments of a common education, or an elementary knowledge of reading, writing and arithmetic as implements of after mental improvement. But it was early found difficult to separate this primary use of tools from the acquisition of knowledge. Mr. Everett said: "I will thank any person to show why it is expedient and beneficial in the community to make public provision for teaching the elements of learning, and not expedient or beneficial to make similar provision to aid the learner's progress toward the mastery of the most difficult branches of science and the choicest refinements of literature." Under the influence of such considerations the rudimentary studies rapidly developed into courses of study embracing a variety of subjects. This led to the systematizing of instruction and the grading of schools, so that in nearly all the towns of the United States the public schools have been divided into primaries for the younger pupils and grammar-schools for older pupils; while within twenty-five years a third grade has arisen known as the high schools for the most advanced students. In each division there are sub-grades, and wherever improvements in public school education are attempted, the principle of gradation is fundamental. So essential is it considered, that no aid is granted from the Peabody fund except to graded schools. As regards the plan of studies adopted there was no guiding principle. All sorts of subjects, and these for all sorts of reasons were taken up, and among them the sciences which are now regular parts of public school study. Classes are formed in physics, chemistry, mineralogy, geology, physiology, botany and zoology. There are text-books upon all these branches, graded to the varying capacities of learners. Teachers prepare in them, and in many cases apparatus is provided, and there are lectures with experiments, specimens, maps, and charts for illustrations.

The old ideal of a school is a place where knowledge is got from books by the help of teachers, and our public school system grew

up in conformity with this ideal. The early effect of grading was to fix and consolidate imperfect methods. The sciences were assimilated to the old practice, and the science teaching falls short at just the points where it was inevitable that it should fall short. The methods of school-teaching, and the habits of the teachers, had grown rigid under the régime of book-studies. As a consequence the science teaching in the public schools is generally carried on by *instruction*. Through books and teachers the pupil is filled up with information in regard to science. Its facts and principles are explained as far as possible, and then left in the memory with his other school acquisitions. He learns the sciences much as he learns geography and history. Only in a few exceptional schools is he put to any direct mental work upon the subject-matter of science, or taught to think for himself.

As thus treated the sciences have but little value in education. They fall below other studies as means of mental cultivation. Arithmetic rouses mental reaction. The rational study of language, by analytical and constructive tasks and the mastery of principles, strengthens the mental processes; but the sciences are not employed to train the faculties in the various ways to which they are severally adapted. They are not made the means of cultivating the observing powers, stimulating inquiry, exercising the judgment in weighing evidence, nor of forming original and independent habits of thought. The pupil does not know the subjects he professes to study by actual acquaintance with the facts, and he therefore becomes a mere passive accumulator of second-hand statements. But it is the first requirement of the scientific method, alike in education and in research, that the mind shall exercise its activity directly upon the subject-matter of study. Otherwise scientific knowledge is an illusion and a cheat. As science is commonly pursued in book descriptions the learners cannot even identify the things they read about. As remarked by Agassiz "the pupil studies Nature in the school-room and when he goes out of doors he cannot find her." This mode of teaching science, which is by no means confined to the public schools, has been condemned in the most unsparing manner by all eminent scientific men as a "deception," a "fraud," an "outrage upon the minds of the young," and "an imposture in education."

Nor has this criticism of bad practices been without its effect. We are met by the statement that much has been done in the

[illegible] science. The [illegible] introduced into [illegible] illustrating the [illegible] that this [illegible] into action [illegible] of words. But [illegible] and is in [illegible] education- [illegible] out. [illegible] not [illegible] if they [illegible] the properties of [illegible] to its [illegible] quick- [illegible]. The scientific [illegible] and accurate [illegible] by [illegible] no such [illegible] but they are [illegible] as a matter of fact open the [illegible]. This is their test [illegible]. When the primary pupils have gone over [illegible] are passed on to a [illegible] are suddenly dropped [illegible]. In the technical [illegible] education [illegible] is now to be carried on by [illegible]. This method as extensively [illegible] grammar grades of the public schools is everywhere [illegible] we are once more told that it is a [illegible] book-studies. It is chiefly applicable to [illegible] and the general idea is instruction without a text-book. [illegible] but it is delusive. The method does not remove the book that the pupil may come at the phenomena, but it removes the book that the teacher may take its place. Oral-teaching is [illegible] instruction, in which information is imparted in a familiar [illegible] with the view of awakening the interest of the class. But [illegible] is concerned it is doubtful if this method is [illegible] than the one it replaces. Following the maxim of [illegible] that "the teacher is the school," it was

assumed that when apathy prevails in the school-room it is solely the teacher's fault. Oral exercises enable them to escape this reproach by giving animation to school work. It is said that this is a "live system" in contrast to the old humdrum routine of lessons and recitations. But science gets no real help. There is only the substitution of a superficial class-activity for the more deliberate work of the individual pupil. More mental effort is required on his part to get a lesson from a book than to listen to a lesson given by the teacher. The teacher is to do everything and stands in the place not only of the book but of the pupil also. Is this not a step backward in education? The teacher is magnified at the expense of close study, and science is cheapened by the method. Oral-teaching implies a fertility, a versatility, and a proficiency in scientific knowledge on the part of teachers which that class of persons does not possess. It is a premium on tutorial smattering and cramming by which the voluble teacher with superficial acquisitions and a ready memory becomes the model teacher. There may be benefits in this method but science does not gain them. Judicious oral assistance, as in the physical, chemical, or natural history laboratory, given by a competent master to a pupil at work, is invaluable for stimulus and guidance; but the aid must be discreet and the skilful teacher will not talk too much. But where it is all talk and no work, and text-books are filtered through the very imperfect medium of the ordinary teacher's mind, and the pupil has nothing to do but to be instructed, every sound principle of education is outraged and science is only made ridiculous.

This failure to gain the benefits of real scientific study has its source deep in the constitution of the public schools. In dealing with masses of children, classification became necessary which gave rise, as we have seen, to grading and an elaborate mechanical system. The working of children in lots seems to be a necessity of the public schools but it strengthens the practice of verbal instruction recitations and lesson-giving. It is well fitted to impress the public with the idea that there is much done in the schools. There is a prescribed routine of operations and a display of order that is admired. But teacher and learner are subordinated to the system. It is machine work and machines make no allowances. Gradation assumes and enforces a uniformity among pupils which is not according to the facts. Wide

public schools to escape the evils of mere book science. The method of object lessons has been extensively introduced into primary schools with the professed purpose of cultivating the powers of observation in childhood. It is claimed that this is a beginning in science; and, as it brings the mind into action upon things, is a corrective of the inordinate study of words. But object teaching has not yielded what was expected of it; and is in no true sense a first step in science. Nothing is gained educationally by barely having an object in hand when it is talked about. Myriads of objects are present to the senses of people but no insight follows. The observing faculties must be tasked if they are to be trained. The pupil is not to have the properties of objects pointed out, but he is to find them out. Science will do its work of educating the observing faculties only as they are quickened and sharpened by exercise in discrimination. The scientific aim is to replace vague confused impressions by clear and accurate ideas. Skill in the detection of nice distinctions is only gained by prolonged and careful practice. Object lessons afford no such cultivation. We do not say that they are useless, but they are not the A B C of science and do not as a matter of fact open the way to the proper study of the special sciences. This is their test and their condemnation. When the primary pupils have gone over their prescribed course of object lessons and are passed on to a higher grade, strange to say the "objects" are suddenly dropped as if the objective method had been exhausted. In the technical phrase *perceptive* education is to be replaced by *conceptive* education. Instruction in elementary science is now to be carried on by what is known as oral-teaching. This method as extensively practised in the grammar grades of the public schools is everywhere growing in favor and we are once more told that it is a successful revolt against book-studies. It is chiefly applicable to the sciences and its cardinal idea is instruction without a text-book. This looks fair but it is delusive. The method does not remove the book that the pupil may come at the phenomena, but it removes the book that the teacher may take its place. Oral-teaching is class instruction, in which information is imparted in a familiar manner with the view of awakening the interest of the class. But so far as real science is concerned it is doubtful if this method is not worse than the one it replaces. Following the maxim of certain German educators that "the teacher is the school," it was

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personal differences of capacity, aptitude, attainment and opportunity, not only exist among children but they are the prime data of all efficient mental cultivation. In the graded schools, just in proportion to the perfection of the mechanical arrangements, individuality disappears. Special original capacity, the main thing, counts for nothing. The mind cannot be trained in such circumstances to originate its own judgments. The exercise of original mental power or independent inquiry is the very essence of the scientific method and with this the practice of the public schools is at war. Moreover, a system which deals with the average mind and does not get at the individual mind breaks down at the point where all true education really begins, that is, in promoting self-culture. The value of educational systems consists simply in what they do to incite the pupil to help himself. Mechanical school-work can give instruction, but it cannot develop faculty because this depends upon self-exertion. Science, if rightly pursued, is the most valuable school of self-instruction. From the beginning men of science have been self-dependent and self-reliant because self-taught; and it is a question whether they have been most hindered or helped by the schools. De Candolle, in his valuable book on the conditions which favor the production of scientific men, says that the discoverers, the masters of scientific method have chiefly appeared in small towns where educational resources have been scanty; and that they have often been most helped by the very poorness of their teaching which threw them back upon themselves. It was to their advantage that the schools were not so perfect as to extinguish individuality and thus destroy originality.

Our strictures are here upon the general working of the public school system; but we recognize that there are many exceptional teachers who do what they can to deal with science in the true spirit, while multitudes of instructors are chafing under present restrictions and groping after something better. The bad system is moreover continued chiefly from the lack of knowledge as to the possibilities of a better. But the better method of teaching science has been proved entirely practicable. The institution where we meet and many other science schools have shown it. A large number of teachers have demonstrated that various branches of science can be taught to the young by the true as well as by the false method. What is now most urgently needed

is to gather from these experiences practical plans of improvement in science teaching for the benefit of those who desire better guidance than they now have.

In his address as Rector of the University of Aberdeen, Professor Huxley said: "I would not raise a finger to introduce more book-work into every art curriculum in the country." We concur in this view as applied to the present science teaching in our public schools. We would not raise a finger to extend it.

President Barnard of Columbia College, in a public address reprobating in severe terms the common method of teaching science as being an inversion of the true order of cultivating the mental faculties, referred to the great benefits which must arise "when our systems of education shall have been remodelled from top to bottom." That result may come about in the fulness of time but it is wise to expect only a slow and gradual improvement. Vice President Grote, in his St. Louis address, pointed out the guiding principle in this case as a substitution of real knowledge for second-hand information by a necessary law of mental advancement. In obedience to this principle, the cultivators of original science should do what they may to raise the standard of our prevalent science teaching; and we respectfully ask that the Association will assign to a committee the duty of reporting at our next meeting on the best modes of improving the teaching of science in our public schools.

E. L. YOUMANS,
A. R. GROTE,
J. W. POWELL,
N. S. SHALER,
J. S. NEWBERRY,
} Committee.

Eulogy on Joseph Henry. By Alfred M. Mayer.

At the meeting of this Association in 1878 a committee, composed of Professors Baird, Newcomb and myself, was appointed to prepare a eulogy on our revered and lamented colleague and former president, Joseph Henry.

This, I will not say labor, but duty of affection, has devolved on me alone. I would that the other members of this committee had laid before you their tributes to his memory, because for years they had been closely associated with him in his social and professional life in Washington. Yet, while Professor Henry had been the friend of their manhood, he was the friend of my boyhood; and during twenty-five years he ever regarded me—as was his wont to say—with a "paternal interest." To his disinterested kindness and wise counsels is due much, very much, of whatever usefulness there is in me. Hence, I have said that it is a duty of affection for me to speak to you about one who was my beloved friend.

I shall not, however, attempt a biography of Joseph Henry, nor shall I speak of his administrative life as Director of the Smithsonian Institution, for this is known and valued by the whole world. His best eulogy is an account of his discoveries, for a man of science, *as such*, lives in what he has *done*, and not in what he has said; nor will he be remembered in what he proposed to do. I will, therefore, with your permission, confine myself chiefly to Henry *as the Discoverer;* and I do this the more willingly because I am familiar with his researches, and also because Professor Henry, from time to time, took pleasure in giving me accounts of those mental conceptions which preceded his work, led him to it, and guided him in it.

To appreciate a discoverer rightly we should not look at his work from our time, but go back and regard it from his time; we should not judge his work in the fulness of the light of present knowledge, but in the dim twilight which alone illuminated him to then unknown—but now well-known—facts and laws. I will, therefore, endeavor first to present you with a clear, but necessarily very concise, view of the state of our knowledge of electricity when Henry began his original researches in that branch of science,

and then point out the value of his discoveries by showing what they added to knowledge, and how they instigated and influenced the discoveries and inventions of other men.

Henry began his electrical researches at the age of twenty-eight, in the year 1827, while he was professor of mathematics and natural philosophy in the Albany Academy. At these he continuously worked till 1832, when, at the age of thirty-three, he moved to the College of New Jersey (Princeton). After a year's break in his work, caused by the preparation of his course of lectures for the college, he is again at original research, and continues his contributions to electrical discoveries till 1842. Thus, during fourteen years, while between the ages of twenty-eight and forty-three, he was a constant and fertile worker. What he did in these years will be given after a review of what had been already discovered up to the time he began his original experiments.

Through the labors of Gilbert, Boyle, Otto von Guericke, Newton, Wall, Gray, Franklin, Æpinus, and Volta, it had been discovered that all matter could be electrically excited, and that bodies differed greatly in permitting the diffusion of electricity over their surfaces; the facts of electric attraction and repulsion, of electric induction, the action of points, and the identity of lightning and electricity had been discovered; and these facts had been explained and bound together in a body of doctrine by the hypothesis of Dufay or by that of Franklin; while Coulomb and Poisson, in a series of beautiful experimental and mathematical labors, had given us the knowledge of the laws of the actions at a distance of electric attraction and repulsion, and had shown in what manner electricity diffuses itself over conductors of various forms.

About 1820, men of science spoke of electrical knowledge as almost complete. The mathematical consequences of the laws discovered by Coulomb and others having been, they thought, fully developed; electricity was hardly to be regarded as an experimental science, but henceforth might be grouped with mechanics. Such opinion was so general that Faraday (in 1831), when he began his ever remarkable series of discoveries, was influenced by this prevailing feeling to style his papers "*Experimental Researches in Electricity.*"

It seemed almost impossible that any discovery could again give an impulse to electrical studies equal to that produced by the

brilliant and most fertile researches of Volta; yet to the universal surprise of the scientific world this happened. In the winter of 1819, Oersted announced that he had at last discovered a correlation of actions between electricity and magnetism in his celebrated experiment of the deflection of a magnet athwart the conjunctive wire of a battery when the latter was laid parallel to the direction of the magnet.

During the month of July, 1820, the news of Oersted's discovery reached Paris. It at once profoundly excited the ever active and versatile mind of Ampère. This man, already celebrated as a mathematician, was now destined to show greater genius as an experimenter. He at once began a series of researches in the field opened by the discovery of Oersted; and with astonishing rapidity reached results of such importance that they gained him the title of the Newton of electro-dynamics; and justly, for he did for this branch of science even more than Coulomb had previously done for electro-statics.

On the 18th of September, 1820, Ampère read his first paper on electro-dynamics, before the Academy of Sciences of Paris. In this he shows that the battery exerts an electro-magnetic action as well as its conjunctive wire, and he gives a rule by which one can readily predict the direction in which a magnet will be deflected by a voltaic current. He supposes a current to flow from the copper to the zinc plate of the battery; then, says he, if you imagine yourself at full length and facing the wire, the current entering your heels and passing out at your head, the north pole of the magnet is always deflected toward your left hand. In the same paper, he says that he will soon experiment with spirals and helices of wire which, he predicts, will have the same properties as magnets as long as a current of electricity flows through them. He then gives his well-known hypothesis of the nature of a magnet. He says that if we assume a magnet to consist of an assemblage of minute currents of electricity whirling all with the same direction of rotation around the steel molecules and in planes at right angles to the axis of the bar, we shall have an hypothesis which will account for all the known properties of a magnet. Ampère constructed his spirals and helices, and to the astonishment of the scientific world made magnets formed only of spools of copper wire traversed by electric currents. We can readily

imagine the intense interest awakened by this discovery; a discovery which caused Arago to exclaim: "What would Newton, Halley, Dufay, Æpinus, Franklin, and Coulomb have said if one had told them that the day would come when a navigator would be able to lay the course of his vessel without a magnetic needle and solely by means of electric currents?"

"For several weeks physicists of France and from abroad crowded Ampère's humble study in Rue Fossée Saint Victor, to see with astonishment a suspended loop of wire, in the circuit of a battery, take a definite position through the directive magnetic action of the earth."

This hypothesis of Ampère had a powerful hold on Henry's mind; and, as I know that he used it as a guiding light in his researches, it may here be well to give Arago's account of how Ampère was led to its conception:

"Thanks to the profound researches of Ampère, the law which governs celestial movements, the law extended by Coulomb to the phenomena of electricity at rest or in tension, and then, though with less certainty, to magnetic phenomena, becomes one of the characteristic features of the powers exercised by electricity in motion. The general formula which gives the value of the mutual actions of the infinitely small elements of currents once understood, the determination of the combined actions of limited currents of different forms becomes a simple problem of integral analysis. Ampère did not fail to follow out these applications of his discoveries. He first tried to discover how a rectilinear current acts on a system of circular closed currents, contained in planes perpendicular to the rectilinear current. The result of the calculation, confirmed by experiment, was that the planes of the circular currents would, supposing them movable, arrange themselves parallel to the rectilinear current. If like transverse currents pass over the whole length of a magnetic needle, the cross direction which, in the experiment of Oersted, seemed an inexplicable anomaly, would become a natural and necessary fact. Is it not then evident to all how memorable would that discovery be which should rigorously establish the fact that to magnetize a needle is to excite, to put in motion around each molecule of the steel, a small circular, electrical vortex? Ampère fully realized the wide reach of the ingenious generalization that had taken possession

of his mind; and he hastened to submit it to experimental proofs and numerical verifications, which, in our day, are the only processes considered entirely demonstrative."

About this time Arago found that the conjunctive wire of the battery had the property of causing iron filings to arrange themselves around it in concentric rings. Guided by Ampère's discovery that a helix conducting a voltaic current had properties similar to those of a magnet, Arago inferred that these properties could be given to iron and steel by placing wires or bars of these substances in the interior of one of Ampère's helices. Experiment showed that his inference was correct. The same effects he obtained by passing electrical discharges from an ordinary frictional electrical machine, or from a Leyden jar, through a helix inclosing a steel needle.

In subsequent memoirs, exhibiting great philosophic acumen and marked ability in the application of mathematical analysis to the elucidation of physical phenomena, Ampère developed the consequences of the general laws he had previously discovered.

In 1821, six years before Henry began his work, Faraday—then thirty years of age, and as yet an assistant of Davy—published his first paper on electrical research. In this he shows that a wire conveying an electrical current can be made to rotate around the pole of a magnet. He then reverses the action, and holding the wire at rest makes the magnetic pole rotate around the wire. These phenomena were shown by Ampère to be entirely conformable to his hypothesis of the electro-dynamic nature of a magnet.

While Ampère, in 1820, was pursuing his researches, Schweigger, of Halle, invented his galvanometer. This he formed by inclosing a suspended magnetic needle in several turns and layers of an insulated wire. This instrument excited a powerful influence in electrical researches, and the contemplation of its action led Henry to make his first trials as an original experimenter.

The history of another research is now in order as bearing directly on one of Henry's investigations—and one which he ever regarded with considerable pride. In 1827, Savary began experiments on the magnetizing actions of the discharge of the Leyden jar on steel needles. These needles, of various lengths, diameters, and degrees of hardness, were placed at right angles to the wire conveying the electric discharge. They were also put in the interior of Ampère's helices, after the manner of Arago's original experi-

ments. The phenomena thus observed were found to be of the most complex characters. It was found that the direction of the polarity in the needle and the intensity of its magnetization depended on its distance from the wire, on the diameter of the needle, on the potentiality of the discharge, and on the resistance of the wire through which the discharge took place. Similar phenomena were observed when the needles were placed in one of Ampère's helices, through which the discharge was thrown. After a long and tedious research Savary concluded that these facts could only be explained by the supposition that the discharge of a Leyden jar was not continuous, but consisted of a series of rebounds or reflections to and from the two coatings of the jar. In 1842, Henry, apparently ignorant of this research of Savary, went over the same ground, and arrived independently at the same inference which Savary had formed fifteen years before—an inference directly confirmed by the experiments of Feddersen, who, in 1862, got the life history of the electric spark of the Leyden jar by photographing its image reflected from a concave mirror revolving 800 to 1,000 times in a second.

Two years previous to Savary's work, *i. e.*, in 1825, William Sturgeon, of Woolwich, England, improved on Arago's experiment of magnetizing steel and iron with the voltaic current. Sturgeon's improvement consisted in bending the straight rods used by Arago into U-shaped pieces, and then, coating them with shellac varnish, he wound them with uncovered copper wire. The coils of the wire were separated, so that the current flowed through the wire around the surface of the iron. This magnet, in proportion to its weight, was the most powerful made up to this date. It certainly did not require great mental effort or acumen on the part of Sturgeon to bend a straight bar magnet into the then common U form of the permanent steel magnet known as the horse-shoe magnet; yet his experiments with this magnet mark an important point of departure in electric science, and evidently led Henry to his first and most important scientific research.

I have now given as much of the history of electrical research as is requisite to the understanding of Henry's position as a discoverer in this branch of knowledge when, in 1827, he began to make original experiments in electricity.

As with many other men of originality, Henry's first essays were in the direction of improving the means of illustrating well-

established scientific facts and principles. His first paper, of October, 1827, is interesting because it was his first. In it he improves on the usual apparatus which had been used by Ampère and others to show electro-dynamic actions, by employing several turns of insulated wire, instead of one, as had previously been the practice. Thus, for example, to show the directive action of the earth's magnetism on a freely-moving closed circuit, Henry covered copper wire with silk and then made out of it a ring about twenty inches in diameter, formed of several turns of the wire. The extremities of this wire were soldered to zinc and copper plates. The coil was then suspended by silk filaments. On plunging the metal plates into a glass of dilute acid the ring rotated around its point of suspension till its plane took a permanent position at right angles to the magnetic meridian. By a similar arrangement of two concentric coils, one suspended within the other, he neatly showed the mutual actions of voltaic currents flowing in the same or in opposite directions; which facts are the foundations of Ampère's celebrated law.

We now reach a period when Henry appears as a discoverer, and truly one of no mean order. As I remember his narration to me in the year 1859, it was as follows: He said that one evening he was sitting in his study in Albany with a friend, when, after a few moments of reverie, he arose and exclaimed, "To-morrow I shall make a famous experiment!" For several months he had been brooding over Ampère's electro-dynamic theory of magnetism, and he was then deeply interested in the phenomena of the development of magnetism in soft iron as shown in the experiments of Arago and Sturgeon. At the moment he had arisen from his chair it had occurred to him that the requirements of the theory of Ampère were not fulfilled in the electro-magnets of Arago and of Sturgeon, but that he could get those conditions which the theory required by covering the enveloping wire with a non-conductor like silk, and then wrapping it closely around the soft iron bar in several layers; for the successive layers of wire coiling first in one direction and then in the other would tend to produce a resultant action of the current at right angles to the axis of the bar; and furthermore, the great number of convolutions thus obtained would act on a greater number of molecules of the bar and therefore exalt its magnetism. "When this conception," said Henry, "came into my brain I was so pleased with

it that I could not help rising to my feet and giving it my hearty approbation."

Henry did go to work the next day, and to his great delight and encouragement discoveries of the highest interest and importance revealed themselves to him week after week. When he had finished his newly conceived magnet he found that it supported several times more weight than did Sturgeon's magnet of equal size and weight. This was his first original discovery.

I will now give, as far as possible, Henry's own words in narrating the subsequent investigations of these very interesting phenomena:

"The maximum effect, however, with this arrangement and a single battery was not yet obtained. After a certain length of wire had been coiled upon the iron the power diminished with a further increase of the number of turns. This was due to the increased resistance which the longer wire offered to the conduction of electricity. Two methods of improvement, therefore, suggested themselves. The first consisted not in increasing the length of coil, but in using a number of separate coils on the same piece of iron. By this arrangement the resistance to the conduction of the electricity was diminished and a greater quantity made to circulate around the iron from the same battery. The second method of producing a similar result consisted in increasing the number of elements of the battery, or, in other words, the projectile force of the electricity, which enabled it to pass through an increased number of turns of wire, and thus, by increasing the length of the wire, to develop the maximum power of the iron.

"To test these principles on a larger scale, an experimental magnet was constructed. In this a number of compound helices were placed on the same bar, their ends left projecting, and so numbered that they could be all united into one long helix, or variously combined in sets of lesser length.

"From a series of experiments with this and other magnets it was proved that, in order to produce the greatest amount of magnetism from a battery of a single cup, a number of helices are required; but when a compound battery is used, then one long wire must be employed, making many turns around the iron; the length of wire, and consequently the number of turns, being commensurate with the projectile power of the battery.

"In describing the results of my experiments the terms *intensity*

and *quantity* magnets were introduced to avoid circumlocution, and were intended to be used merely in a technical sense. By the *intensity* magnet I designated a piece of soft iron so surrounded with wire that its magnetic power could be called into operation by an *intensity* battery; and by a *quantity* magnet a piece of iron so surrounded by a number of separate coils that its magnetism could be fully developed by a *quantity* battery.

"I was," says Henry, "the first to point out this connection of the two kinds of the battery with the two forms of the magnet, in my paper in *Silliman's Journal*, January, 1831, and clearly to state that when magnetism was to be developed by means of a compound battery one long coil was to be employed, and when the maximum effect was to be produced by a single battery, a number of strands were to be used."

Here is Henry's description of one of his quantity magnets: "A bar of iron 21 inches long and 2 inches square with rounded corners was bent into a U form, having legs about nine inches long. This bar weighed twenty-one pounds. Its armature was formed of a piece of a similar bar and weighed seven pounds. Nine coils of copper bell-wire, each sixty feet in length, were wrapped in sections on the iron. These coils were not continued around the whole length of the bar, but each strand of wire, according to the principle before mentioned, occupied about two inches, and was coiled several times backward and forward over itself; the several ends of the wire were left projecting and all numbered, so that the first and last end of each strand might be readily distinguished. In this manner was formed an experimental magnet on a larger scale, with which several combinations of wire could be made by merely uniting the different projecting ends. Thus, if the second end of the first wire be soldered to the first end of the second wire, and so on through all the series, the whole will form a continued coil of one long wire. By soldering different ends the whole may be formed into a double coil of half the length, or into a triple coil of one-third the length, etc. The horse-shoe was suspended in a rectangular wooden frame 3 feet 9 inches high and 20 inches wide.

"In order to ascertain the effect of a very small galvanic element on this large quantity of iron, a pair of plates, exactly one square inch, was attached to all the wires: the weight lifted was eighty-five pounds. To find out the greatest supporting power of the magnet, with all of its nine coils in a circuit, a small battery

formed of a plate of zinc 12 inches long and 6 inches wide, and surrounded by copper, was substituted for the galvanic element used in the former experiments: the weight lifted was seven hundred and fifty pounds."

The most powerful of Henry's magnets was constructed while he was at Princeton, and is thus described by his successor in the chair of Natural Philosophy, Professor Richard S. McCulloh: "It is formed of a bar of rounded iron nearly four inches in diameter, weighing about one hundred pounds, and surrounded with thirty strands of copper bell-wire, each about forty feet long. With a calorimotor on Dr. Hare's plan consisting of twenty-two plates of zinc each 9 inches by 12, alternating with plates of copper of the same size, it supports 3,500 pounds, or more than a ton and a half.

"After the connection with the battery is broken, this magnet supports a thousand pounds for several minutes, and from year to year the lifter adheres with a force which is overcome only by a weight of several hundred pounds. When however the lifter is detached, nearly all the magnetism disappears."

On a recent visit to the College of New Jersey by the electrician Mr. Frank L. Pope, he examined this magnet. "There," he says, in his admirable and justly appreciative eulogy on Henry, "there, too, was the reversing commutator or pole-changer, a device first invented by Professor Henry, with which he was accustomed to delight and astonish his pupils, by suddenly reversing the polarity of his large magnet, causing it to drop its armature and seize it again before it had passed beyond the sphere of attraction, a principle which we see exemplified in every stroke of the neutral relay of the quadruplex telegraph of to-day."

We will now return to Henry's study of the properties of his intensity magnet. This magnet was formed of a piece of iron one-fourth of an inch in diameter, bent in the U form and wound with eight feet of insulated wire. His batteries were two,—one formed of a single element with a zinc plate 4 inches by 7, surrounded by copper and immersed in dilute acid; the other, a Cruikshank's battery, or *trough*, with twenty-five double plates. The plates of this battery were joined in series, and altogether had exactly the same surface of zinc as that in the single-cell battery.

The magnet was now connected directly to the single cell. The magnet held up seventy-two ounces. Then five hundred and thirty

feet of number 18 copper wire led the current from the cell to the magnet; it now supported only *two ounces*. Five hundred and thirty feet more of the wire were introduced into the circuit, and then the magnet held but *one ounce*. In these facts Henry faced the same results as confronted Barlow five years before, and caused Barlow then to say: "In a very early stage of electro-magnetic experiments it had been suggested [by Laplace, Ampère and others] that an instantaneous telegraph might be established by means of conducting wires and compasses; * * * but I found such a sensible diminution with only two hundred feet of wire, as at once to convince me of the impracticability of the scheme;" and such, at that day, seemed to be the common opinion of men of science. But this opinion is presently to be shown by Henry to be ill-founded, by reason of the ignorance of the relations which have of necessity to exist between the *kind* of battery and the *kind* of magnet in order to produce electro-magnetic action at a distance: relations which Henry was the first to discover. This accomplishment justly entitles him to be regarded as a man of genius and a discoverer of no mean order. This discovery will always remain the one important fact that was to be known, to be understood, and to be applied, before it was possible to have constructed any form of electro-magnetic telegraph.

Let us see how Henry made this discovery. After ending the experiments with the one-cell battery and reaching results which seemed to confirm the opinion of Barlow as to "the impracticability of the scheme" of an electro-magnetic telegraph, Henry attached his magnet to the second battery formed of twenty-five cells, arranged in series. The current from this battery was sent to the magnet through 1,060 feet of the same wire as had been used in the experiments with the first battery of one cell. The magnet now lifted eight ounces. It had held up only *one* ounce when with the same length of interposed wire the battery of one cell was used.

He now attached his electro-magnet directly to the poles of the 25-cell battery, when to his astonishment it only held seven ounces. The same magnet, it will be remembered, when attached to the one-cell battery, supported seventy-two ounces.

Here were facts of the highest significance, and Henry was not slow to seize them in all their bearings. Referring to these experiments, he says: "It is possible that the different states of the

trough, with respect to dryness, may have exerted some influence on this remarkable result; but that the effect of a current from a *trough* (*i. e.*, a series of cells) is at least not sensibly diminished by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph, and it is also of material consequence in the construction of the galvanic coil.

Henry speaking, in 1857, of these, his first gatherings into the garner of science, says: "These steps in the advance of electro-magnetism, though small, were such as to interest and astonish the scientific world. With the same battery used by Mr. Sturgeon, at least a hundred times more magnetism was produced than could have been obtained by his experiment. These developments were considered at the time of much importance in a scientific point of view, and they subsequently furnished the means by which magneto-electricity, the phenomena of dia-magnetism, and the magnetic effects on polarized light were discovered. They gave rise to the various forms of electro-magnetic machines which have exercised the ingenuity of inventors in every part of the world, and were of immediate applicability in the introduction of the magnet to telegraphic purposes. Neither the electro-magnet of Sturgeon, nor any electro-magnet ever made previous to my investigations, was applicable to transmitting power to a distance."

Henry however was not satisfied with the mere statement that his discovery was "directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph;" he actually constructed an electro-magnetic telegraph. Sometime during the year 1831, "I arranged," says he, "around one of the upper rooms of the Albany Academy a wire of more than a mile in length, through which I was enabled to make signals by sounding a bell. The mechanical arrangement for effecting this object was simply a steel bar, permanently magnetized, of about ten inches in length, supported on a pivot, and placed with its north end between the two arms of a horse-shoe magnet. When the latter was excited by the current, the end of the bar thus placed was attracted by one arm of the horse-shoe and repelled by the other, and was thus caused to move in a horizontal plane and its further end to strike a bell suitably adjusted."

This was the first electro-magnetic telegraph which had worked through so great a length of wire; it was the first electro-magnetic

telegraph in which an electro-magnet had worked successfully; it was the first "sounding" electro-magnetic telegraph.

On this occasion we have not the time to enter into a discussion of the relative parts played by Henry and Morse in the invention of the electro-magnetic telegraph; nor do I think such a course necessary. Henry's own words as given in his "Statement in relation to the history of the electro-magnetic telegraph," and published by the Regents of the Smithsonian Institution in 1857, give all that is required to a just understanding of the relations of these two distinguished men to this invention.

"The principles," says Henry (referring to his discoveries in electro-magnetism of which I have just given an account), "I had developed were applied by Dr. Gale to render Morse's machine effective at a distance." This statement seems to be as direct, as clear, as truthful, and as comprehensive as one can desire. They are Henry's own words, and we all receive them as entirely satisfactory. "The principles I had developed were applied by Dr. Gale to render Morse's machine effective at a distance." Observe, Henry does not claim to have had any part in rendering Morse's machine effective when near the battery; no, because that was the condition of the machine before Morse called in the assistance of Dr. Gale in the winter of 1836–'37; but Henry *does* claim this: by his discoveries to have given Dr. Gale the *principles* which Dr. Gale applied to Morse's machine and *rendered it effective at a distance;* nor does Henry claim Morse's ingenious marking machine—a lever, one of whose ends is attracted by the electro-magnet against an opposing spring, while the other end of the lever makes a mark on a moving surface. Nor does Henry claim any of the other ingenious mechanical combinations invented by Morse. Henry's claim is the claim of a *discoverer* not of an *inventor;* for he says: "The *principles* I had developed were applied by Dr. Gale to render Morse's instrument effective at a distance."

Henry does not claim that his own telegraphic machine (which was undoubtedly an original invention) had been appropriated by Mr. Morse; certainly not, because it is an entirely different invention. And here let me call your attention to an important fact, viz.: neither Henry nor Morse could lay claim to having originated the *idea* of causing a voltaic current to produce electro-magnetic actions at a distance; yet the majority of persons, who have not

examined into the history of telegraphy, think that this is the very point at issue between Henry and Morse.

Finally, I will take the liberty of remarking that had Henry taken out a patent in which he claimed as his invention an electro-magnet *formed of two or more layers of insulated wire*, Morse's patent would not have been so valuable. Remember, I speak not of the merit of the invention, but of the merit of the patent; for the invention, so far as Morse is concerned, would have remained the same, because one essential part of a Morse telegraph is Henry's intensity magnet, and certainly Morse never invented that.

Let us pause here awhile from following Henry in his career of discoverer and examine a little more curiously into what he has just done. I said, in the beginning of this discourse, that to judge rightly of a discoverer's achievements we should view them in the light of the knowledge of his time. What was that knowledge? I have already sketched it sufficiently to show how much Henry was indebted to knowledge then existing, at least in so far as he was guided thereby in his work. In this light his achievements appear indeed remarkable, and as admirable as those of any philosopher of his time.

Simultaneously with Henry's first publication in 1827, on the improvement of electro-magnetic apparatus by increasing the length of the galvanic conductor and the number of its coils, Ohm published at Berlin, his mathematical law of galvanic circuits, in a book entitled *Galvanische Kette, mathematisch bearbeitet*. This publication was not only received with indifference, but almost with contempt by his countrymen. Professor H. W. Dove, of Berlin, says that "in the Berlin *Jahrbücher für wissenschaftliche Kritik*, Ohm's theory was named a web of naked fancies, which can never find the semblance of support from even the most superficial observation of facts; 'he who looks on the world,' proceeds the writer, 'with the eye of reverence must turn aside from this book as the result of an incurable delusion, whose sole effort is to veil from the Deity of nature.'"

Ohm's researches were based avowedly on a thoughtful study of the works of Fourier on heat, published in 1822, and of the galvanic researches of Poisson, published in 1820-'21, as applicable to the subject of heat. Fourier in 1822, and Poisson's series of ingenious [illegible] in the years 1823-'26 were then completed by

the full announcement of his discoveries, January 1, 1831. At that time, no writer or physicist appears to have had any just conception of the consequences flowing from Ohm's announcement,—particularly of that most important deduction, viz.: that the interpolar resistance should equal the internal resistance of the battery, in order to obtain the maxima of electro-magnetic effects. This theory or law of Ohm,—utterly neglected at home,—unknown to Wheatstone, to Faraday, or to Roget,—could hardly make its way abroad in the garb of a foreign tongue, and reach Henry in Albany. Henry could not read German, and Ohm's papers were first published in English in *Taylor's Scientific Memoirs*, vol. ii, London, 1841. From the very manner in which Henry worked at his problems and viewed the results of his experimenting it is evident that, at that date, he had no knowledge of Ohm's law; otherwise he would not have been so astonished at the results when his "intensity magnet" was connected with his "intensity battery."

Henry, now in possession of the powerful magnets of his own creation, turned his thoughts to the uses to which he might put these instruments as aids in making other discoveries. He began with work on a problem which had baffled many able men before him. He tried to do the reverse of what he had already done. He had made his great magnet by the action of the electric current, he now tried to obtain an electric current from the magnetism of his great magnet,—and he succeeded.

It is not generally known or appreciated that Henry and Faraday independently discovered the means of producing the electric current and the electric spark from a magnet. Tyndall, in speaking of this great discovery of Faraday, says: "I cannot help thinking while I dwell upon them that this discovery of magneto-electricity is the greatest experimental result ever obtained by an investigator. It is the Mont Blanc of Faraday's own achievements. He always worked at great elevations, but higher than this he never subsequently attained."

The history of Henry's connection with this notable discovery is, I think, best given in Henry's own words, which I take from *Silliman's Journal* of July, 1832. Referring to Faraday's discovery, he says: "No detail is given of the experiments, and it is somewhat surprising that results so interesting, and which certainly form a new era in the history of electricity and magnetism,

should not have been more fully described before this time in some of the English publications. The only mention I have found of them is the following short account from the *Annals of Philosophy* for April, under the head of Proceedings of the Royal Institution: 'Feb. 17, Mr. Faraday gave an account of the first two parts of his researches in electricity, namely, volta-electric induction and magneto-electric induction. * * * If a wire, connected at both extremities with a galvanometer, be coiled in the form of a helix around a magnet, no current of electricity takes place in it. This is an experiment which has been made by various persons hundreds of times, in the hope of evolving electricity from magnetism. But if the magnet be withdrawn from or introduced into such a helix, a current of electricity is produced *while the magnet is in motion*, and is rendered evident by the deflection of the galvanometer. If a single wire be passed by a magnetic pole a current of electricity is induced through it which can be rendered sensible.' [Henry continues:]

"Before having any knowledge of the method given in the above account, I had succeeded in producing electrical effects in the following manner, which differs from that developed by Mr. Faraday, and which appears to me to develop some new and interesting facts: A piece of copper wire about thirty feet long, and covered with elastic varnish, was closely coiled around the middle of the soft-iron armature of the galvanic magnet described in vol. xix of the American Journal of Science, and which, when excited, will readily sustain between six hundred and seven hundred pounds. The wire was wound upon itself so as to occupy only about one inch of the armature, which is seven inches in all. The armature thus furnished with the wire was placed in its proper position across the ends of the galvanic magnet, and there fastened so that no motion could take place. The two projecting ends of the helix were dipped into two cups of mercury, and these connected with a distant galvanometer by means of two copper wires each about forty feet long. This arrangement being completed, I stationed myself near the galvanometer, and directed an assistant at a given word to immerse suddenly in a vessel of dilute acid the galvanic battery attached to the magnet. At the instant of immersion the north end of the needle was deflected 30° to the west, indicating a current of electricity from the helix surrounding the armature. The effect however appeared only as a single impulse,

for the needle, after a few oscillations, resumed its former undisturbed position in the magnetic meridian, although the galvanic action of the battery, and consequently the magnetic power, still continued. I was however much surprised to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction, when the battery was withdrawn from the acid, and again deflected to the west when it was re-immersed. This operation was repeated many times in succession, and uniformly with the same result, the armature the whole time remaining immovably attached to the poles of the magnet, no motion being required to produce the effect, as it appeared to take place only in consequence of the instantaneous development of the magnetic action in one and the sudden cessation of it in the other. * * * From the foregoing facts it appears that a current of electricity is produced for an instant in a helix of copper wire surrounding a piece of soft iron whenever magnetism is induced in the iron; and a current in an opposite direction when the magnetic action ceases; also that an instantaneous current in one or the other direction accompanies every change in the magnetic intensity of the iron."

I will now give Henry's account of the experiment by which he obtained a spark from the magneto-electric current—certainly the first flash of a magneto-electric current ever seen in this country: "The poles of the magnet," says Henry, "were connected by a single rod of iron bent into the form of a horse-shoe, and its extremities filed perfectly flat so as to come in perfect contact with the faces of the poles: around the middle of the arch of this horse-shoe two strands of copper wire were tightly coiled, one over the other. A current from one of these helices deflected the needle one hundred degrees, and when both were used, the needle was deflected with such force as to make a complete circuit. But the most surprising effect was produced when, instead of passing the current through the long wires to the galvanometer, the opposite ends of the helices were held nearly in contact with each other and the magnet suddenly excited: in this case a small but vivid spark was seen to pass between the ends of the wires, and this effect was repeated as often as the state of intensity of the magnet was changed. * * * It appears from the May number of the Annals of Philosophy, that I have been anticipated in this experiment of drawing sparks from the magnet by Mr. James D. Forbes,

of Edinburgh, who obtained a spark on the 30th of March, my experiments being made during the last two weeks of June. A simple notification of his result is given, without any account of the experiment, which is reserved for a communication to the Royal Society of Edinburgh. My result is therefore entirely independent of his, and was undoubtedly obtained by a different process."

A few words now will place Henry in his proper and just relation to these important discoveries. We have seen that all the information he had received about Faraday's discovery was the account of Faraday's production of magneto-electricity by the sudden insertion of a magnet into a helix and by its sudden withdrawal therefrom. This is the experiment described in section No. 39 of Faraday's paper of November, 1831. Henry's experiment is entirely different and certainly was entirely original with him, but it is essentially Faraday's experiment described in sections 27, 28, 29, 30 and 31 of the same paper, and is the first in the order of those which Faraday gives of his various methods of evolving electricity from magnetism. Of this experiment Henry had no knowledge when he obtained the electric current from the magnet, no more than he had of the other experiment in which Faraday moved a permanent steel magnet in a helix. Thus it clearly appears that though Henry cannot be placed on record as the *first* discoverer of the magneto-electric current, yet it *can* be claimed that he stands alone as its *second* independent discoverer.

As to the production of the electric spark from the magneto-electric current, both Henry and Forbes were anticipated by Faraday, who describes an experiment, which in all essentials is the same as Henry's, in section No. 32 of the same paper of November, 1831.

I may have been somewhat tedious in these long quotations and minute narrations of dates, but my object is to place Henry before you as a discoverer and make you appreciate him, and that justly; — not to ask too much for him, for that would injure his fair name.

Henry's next discovery was that of the induction of a current on itself, or of the "extra current," as it is sometimes called. Here he had the good luck to anticipate Faraday by nearly two years and a half in the observation of the fundamental facts of this discovery, Henry publishing his observations in July, 1832,

while Faraday's first appear in the Philosophical Magazine for November, 1834. Therefore, to Henry should be given the honor of having made the first observations of these phenomena; but not in opposition to any claim set up for Faraday, because Faraday expressly states in his paper read before the Royal Society on January 29, 1835, that "the inquiry arose out of a fact communicated to me by Mr. Jenkin, which is as follows: If an ordinary wire of short length be used as the medium of communication between the two plates of an electro-motor consisting of a single pair of metals, no management will enable the experimenter to obtain an electric shock from this wire; but if the wire which surrounds an electro-magnet be used, a shock is felt each time the contact with the electro-motor is broken, provided the ends of the wire be grasped one in each hand." Notwithstanding this explicit statement of Faraday, neither to Henry nor to Jenkin is generally accorded the credit for the original observations, but it is given to Faraday. This is accounted for by the fact that although Henry had the good fortune to anticipate others in the observations, he had not the leisure to follow up these observations to their full explanation till after Faraday had completely unravelled their nature. This was owing to the removal of Henry to Princeton in November of 1832, shortly after he had made his few preliminary experiments; and he did not resume and finish this research till 1834; and in 1835 he gave the results of his work to the American Philosophical Society in a paper "On the Influence of a Spiral Conductor in Increasing the Intensity of Electricity from a Galvanic Arrangement of a Single Pair, etc."

In 1838, after Henry's return from his first visit to Europe, he discovered an entirely new class of phenomena in electrical induction; and as the field was entirely his own he entered into this work with great enthusiasm. In these researches he extends greatly our knowledge of electrical induction. He first showed that an induced current may excite a second induced current in a neighboring closed conductor, and this last may induce a third current in another neighboring closed circuit, and so on. These various induced currents Henry styled currents of the first, second, third, fourth, fifth, etc., orders. He shows that these currents alternate in their direction in the successive orders,—at least when these currents are induced by the discharge of a voltaic battery. He investigates the differences in the properties of these currents

according as they flow through conductors formed of few convolutions of low resistance or through many convolutions of high resistance. He shows that plates of metal, when their surfaces are continuous, screen the inductive action of a current of one order on the succeeding order, but that when a sector is cut out of the metal plate the screening effect disappears. The same phenomena of induced currents of different orders he tracks through the inductive actions of the discharge of the Leyden jar and of the ordinary frictional electrical machine in the most skilful manner and shows in what these phenomena differ from those produced by the inductive actions of the discharges of the voltaic battery.

In the time allotted us it is impossible to give even the most concise abstract of these beautiful investigations. They are however known to you all. They form part of the doctrine of modern physics. These researches into the nature and laws of the induced currents of different orders are the most finished of Henry's works and will ever be regarded as models of careful and thorough scientific work.

We here leave Henry's researches in electricity with the regret that we have been able only to give but meagre and imperfect accounts of them; and that the occasion does not permit me to mention even by their titles several of his investigations in this department of knowledge.

Henry had a versatile mind, and did not confine his attention to the study of electricity. His genius has adorned all departments of Physics. His researches in molecular physics, though not extensive, are remarkable. Here his fertile suggestions and original methods of research have instigated others to follow out the paths which he had pointed out.

In 1839 Henry made a very curious discovery as to the permeability of lead to mercury. So permeable indeed is this metal to the fluid that he found mercury would ascend a lead wire to the height of a yard in a few days. He even made what might be called, so far as their forms are concerned, syphons of lead which would nearly empty a vessel of mercury by gradually drawing the fluid over its sides. Subsequently, in 1845, with the assistance of Mr. Cornelius, of Philadelphia, he succeeded in showing that copper when heated to the melting point of silver would absorb the latter metal. This he distinctly proved by subsequently dissolving

off the surface of the copper plate with zinc chloride, when the absorbed silver made its appearance, having penetrated to a slight distance into the copper.

In 1844 Henry is again at work in molecular physics, investigating the nature of the forces acting in liquid films. This investigation was duly valued by Plateau, who has given us his beautiful researches into the conditions of equilibrium of polyhedra with surfaces formed of films of water, and Plateau chided Henry for having neglected to investigate further into phenomena which he was the first to discover. Of Henry's work in this direction there only remains the record of a scanty verbal communication which he made to the American Philosophical Society in 1844. From this I make the following abstract: "The passage of a body from a solid to a liquid state is generally attributed to the neutralization of the attraction of cohesion by the repulsion of the increased quantity of heat; the liquid being supposed to retain a small portion of its original attraction, which is shown by the force necessary to separate a surface of water from water,— in the well-known experiment of a plate suspended from a scale beam over a vessel of the liquid. It is however more in accordance with all the phenomena of cohesion to suppose, instead of the attraction of the liquid being neutralized by the heat, that the effect of this agent is merely to neutralize the polarity of the molecules so as to give them perfect freedom of motion around every imaginable axis. The small amount of cohesion (52 grains to the square inch), exhibited in the foregoing experiment, is due, according to the theory of capillarity of Young and Poisson, to the tension of the exterior film of the surface of water drawn up by the elevation of the plate. This film gives way first, and the strain is thrown on an inner film, which in turn is ruptured; and so on until the plate is entirely separated; the whole effect being similar to tearing the water apart atom by atom.

"Reflecting on the subject, the author has thought that a more correct idea of the magnitude of the molecular attraction might be obtained by studying the tenacity of a more viscid liquid than water. For this purpose he had recourse to soap-water, and attempted to measure the tenacity of this liquid by means of weighing the quantity of water which adhered to a bubble of this substance just before it burst, and by determining the thickness of the film from an observation of the color it exhibited in comparison

with Newton's scale of thin plates. Although experiments of this kind could only furnish approximate results, yet they show that the molecular attraction of water for water instead of being only about 52 grains to the square inch is really several hundred pounds, and is probably equal to that of the attraction of ice for ice. The effect of dissolving the soap in the water is not, as might at first appear, to increase the molecular attraction, but to diminish the mobility of the molecules and thus render the liquid more viscid.

"According to the theory of Young and Poisson, many of the phenomena of liquid cohesion, and all those of capillarity, are due to a contractile force existing at the free surface of the liquid, and which tends in all cases to urge the liquid in the direction of the radius of curvature towards the centre, with a force inversely as the radius.

"According to this theory the spherical form of a dew-drop is not the effect of the attraction of each molecule of the water on any other, as in the action of gravitation in producing the globular form of the planets (since the attraction of cohesion only extends to an inappreciable distance), but is due to the contractile force which tends constantly to enclose the given quantity of water within the smallest surface, namely, that of a sphere. The author finds a contractile force similar to that assumed by this theory, in the surface of the soap-bubble; indeed, the bubble may be considered a drop of water with the internal liquid removed and its place supplied by air. The spherical form in the two cases is produced by the operations of the same cause. The contractile force in the surface of the bubble is easily shown by blowing a large bubble on the end of a wide tube — say an inch in diameter; as soon as the mouth is removed the bubble will be seen to diminish rapidly, and at the same time quite a forcible current of air will be blown through the tube against the face. This effect is not due to the ascent of the heated air from the lungs with which the bubble was inflated, for the same effect is produced by inflating with cold air, and also when the bubble is held perpendicularly above the face, so that the current is downward.

"Many experiments were made to determine the amount of this force, by blowing a bubble on the larger end of a glass tube in the form of a letter U, and partially filled with water; the contractile force of the bubble, transmitted through the enclosed air, forced down the water in the larger leg of the tube and caused it to rise

in the smaller. The difference of level observed by means of a microscope gave the force in grains per square inch, derived from the known pressure of a given height of water. The thickness of the film of soap-water which formed the envelope of the bubble was estimated as before, by the color exhibited just before bursting. The results of these experiments agree with those of weighing the bubble, in giving a great intensity to the molecular attraction of the liquid; equal at least to several hundred pounds to the square inch. Several other methods were employed to measure the tenacity of the film, the general results of which were the same; the numerical details of them are reserved however until the experiments can be repeated with a more delicate balance.

"The comparative cohesion of pure water and soap-water was determined by the weight necessary to detach the same plate from each; and in all cases the pure water was found to exhibit nearly double the tenacity of soap-water. The want of permanency in the bubble of pure water is therefore not due to feeble attraction, but to the perfect mobility of the molecules, which causes the equilibrium, as in the case of the arch, without friction of parts to be destroyed by the slightest extraneous force."

Another of Henry's investigations in molecular physics, having important practical bearings, should be more generally known than it is. Among his other duties as chairman of the United States Light-House Board was the testing of the various physical properties of the oil submitted to the Government for purchase. Fluidity was one of these properties of which it seemed most difficult to get reliable comparative tests. Henry discarded all the crude instruments and methods which give results in which the different degrees of fluidity of the oils are masked by their various powers of adhesion to the surface over which they flow during the process of testing. Henry very ingeniously applied the theorem of Torricelli, which shows that equal quantities of all liquids—supposing them to be all alike in fluidity—will in equal times flow out of an orifice in the bottom of a vessel. Henry found that equal quantities of mercury and water flowed out of the vessel in equal times; but with different oils the times of flow of equal quantities were different. Thus the rapidity of flow of sperm oil exceeded that of lard oil in the ratio of 100 to 167. I think that this method of experimenting suggested itself to Henry about fifteen years ago. I remember when he was working with this apparatus, and of his

telling me that to his surprise he found that alcohol was less fluid than water.

Henry always took a deep interest in the study of acoustics. His additions to this branch of knowledge were chiefly the results of his experiments in connection with our system of coast fog-signals. He made extensive experiments on various sound-producing instruments, such as bells, cannon, steam whistles, and steam reed and syren fog-horns. He eventually decided in favor of the latter as the most powerful and effective instrument yet invented. He determined that these instruments send their sounds to the greatest distances when they emit a note in the treble part of the musical scale. They are, in fact, tuned very near to the treble *C*. Henry also showed the uselessness of applying reflectors to these instruments. But his principal researches were in the direction of determining the influence of various atmospheric conditions on the audibility and manner of propagation of the sounds of the fog-horns on our northern coasts. The results which he reached, though of great importance, appear to bear a very small relation to the great amount of time spent and fatigue and exposure endured in procuring them.

During eleven years Henry did not cease to labor most devotedly to do all he could to advance the efficiency of our fog-signals by studying the action of these instruments in all kinds of weather. Many facts were collected, and very puzzling were these to explain by any known laws pertaining to the propagation of sound. Thus it was observed that a sound coming to the mariner against the direction of the wind would cease to be audible on the deck of his vessel, while it continued to be heard to a listener on the mast-head. An observation made at Block Island showed this fact in a marked manner. The lens of this light is about 200 feet above the beach at the base of the cliff on which stands the light-house. The wind was blowing seven miles an hour. The vessel sounding its steam whistle steamed away from the light, going in the direction towards which the wind was blowing. The listener on the top of the light-house heard the sound four times longer than the observer on the beach; but when the vessel ran away from the light-house against the wind, the sound disappeared first to the observer on the top of the light-house.

It was also observed that sometimes on approaching a fog-horn from a distance the intensity of its sound would gradually increase,

then die down quite rapidly and become inaudible through a space of from three to four miles, and often would not reappear till the vessel was within a mile of the fog-horn. Often when the sound came to the listener against a moderate wind the fog-horn would become inaudible at a distance of three or four miles, while on other days, when the wind was going with the sound, the listener had to sail away twenty-five miles before the horn ceased to be heard. Observations made at Block Island and Point Judith showed this fact in the following manner: The distance between these fog-horns is seventeen miles, and the sound of one can be distinctly heard at the other when the air is quiet and homogeneous; but if the wind blows from one towards the other the listener at the station from which the wind blows is unable to hear the other horn.

The most remarkable series of Henry's observations was made at Whitehead Station, Maine, situate on a small island about one mile and a half off the coast. The vessel was approaching the station from the south and with the wind. "The belt of silence" was reached and traversed, and then the sound reappeared. This happened whether the vessel was steaming towards or away from the station, the wind remaining all the while southerly. But during these observations on the vessel the sounds of the steamer's whistle were heard without interruption at the station. Now the steamer's course was directed to the other side of the station; and steaming away from the fog-horn and against the wind the whistle at the station was constantly heard by those on the vessel, but those at the station now perceived the steamer's whistle to go into and out of "the belt of silence."

These facts demanded explanation, and for a long time remained enigmas to Henry; till one day he met with a short paper by Professor Stokes, of Cambridge, England, in the Proceedings of the British Association for 1857 "in which the effect of an upper current in deflecting the wave-surface of sound so as to throw it down upon the ear of the auditor, or directing it upward far above his head, is fully explained." In the Report of the Light-House Board for 1874 Henry says: "The explanation [of these phenomena] as suggested by the hypothesis of Professor Stokes, is founded on the fact that in the case of a deep current of air the lower stratum, or that next the earth, is more retarded by friction than the one immediately above, and this again than the one above

it, and so on. The effect of this diminution of velocity as we descend towards the earth is, in the case of sound moving with the current to carry the upper part of the sound-waves more rapidly forward than the lower part, thus causing them to incline toward the earth or in other words, to be thrown down upon the ear of the observer. When the sound is in a contrary direction to the current, an opposite effect is produced,—the upper portion of the sound-waves is more retarded than the lower, which, advancing more rapidly in consequence, inclines the waves upward and directs them above the head of the observer. To render this more clear, let us recall the nature of a beam of sound, in still air, projected in a horizontal direction. It consists of a series of concentric waves perpendicular to the direction of the beam, like the palings of a fence. 'Now, if the upper part of the waves has a slightly greater velocity than the lower, the beam will be bent downward in a manner somewhat analogous to that of a ray of light in proceeding from a rarer to a denser medium. The effect of this deformation of the wave will be cumulative from the sound-centre outward, and hence, although the velocity of the wind may have no perceptible effect on the velocity of sound, yet this bending of the wave being continuous throughout its entire course, a marked effect must be produced. A precisely similar effect will be the result, but perhaps in a considerably greater degree, in case an upper current is moving in an opposite direction to the lower, when the latter is adverse to the sound, and in this we have a logical explanation of the phenomenon observed by General Duane, in which a fog-signal is only heard during the occurrence of a northeast snowstorm. Certainly this phenomenon cannot be explained by any peculiarity of the atmosphere as to variability of density, or of the amount of vapor which it may contain."

Henry's services to the Light-House Board were of great value to the country. The fact that his investigations showed that lard oil when heated to about 250° Fahrenheit is superior in fluidity and illuminating power to sperm oil, caused the substitution of the former for the latter; and thus was saved a dollar on each gallon of illuminating material purchased. This amounted to about one hundred thousand dollars a year in favor of the government.

In light and heat Henry made several interesting investigations which, reluctantly, we are obliged to pass over. One however holds so important a place in the history of science that it cannot

be omitted from any discourse which would treat of Henry as a discoverer. I refer to his application of the thermopile to the determination of the distribution of heat on the optical images of distant objects. It occurred to Henry that images in the foci of mirrors and lenses are formed not alone by converging pencils of *light* coming from corresponding points of the objects placed before these mirrors and lenses, but that images are also formed by the convergence of rays which have no effect on the optic nerve, such as the rays of heat. Indeed Henry looked upon the image as having, on a small scale, the same distribution of physical actions as exist on the surface of the large object, of which this image is the optical reproduction.

He applied this conception in a bold and wonderful experiment; which was no other than to study the distribution of heat on the surface of the sun. In 1845, in company with his brother-in-law, Professor Stephen Alexander, he formed an image of the sun by pointing a telescope to that body and then drawing out the eye-tube of the instrument till the solar image was clearly defined on a screen. In this screen was cut a small aperture, closed by the surface of a thermopile. By motion of the telescope any part of the solar image could be brought on to the surface of the pile. A solar spot of considerable magnitude being then present, he brought it on to the pile and noticed the amount of deflection produced in the needles of the galvanometer by the thermo-electric current. Then the parts of the sun's image adjacent to the spot were brought to the thermopile; and now he observed a greater deflection in the galvanometer than in the previous experiment; thus "clearly proving," as he says, "that the spot emitted less heat than the surrounding parts of the luminous disk."

This new method of research originated with Henry. It was shown to Secchi while he was in this country as Professor in the College of Georgetown. On his return to Europe, Secchi obtained no inconsiderable repute by extending these observations — using the methods of Henry, but, I fear, not giving sufficient credit to the originator of them. But let that pass; for the bread which Henry cast upon the waters has returned to our own shores — thanks to the genius and perseverance of our colleague Langley.

Most reluctantly do I here desist from citing further the works of Henry. It is impossible to crowd into one brief hour the thoughts which were his occupation during more than half a cen-

tury. I have at least endeavored to exhibit before you the more important of the labors of his life. What shall we think of them? Surely they are on as high a plane as those of any of his contemporaries, and show as much originality as theirs in their conception - as much skill in their execution. Yet it has been said that Henry was not a man of genius. As I have not been able to find that the philosophers, who have the special charge of giving from time to time definitions of genius, have been able to come to any satisfactory conclusion among themselves, I will leave their company, and, with your liberty, take my definition from a book which, if we accredit Thackeray, is one of the very best novels ever written in English. After listening to this, you may form your own opinions as to whether Henry did or did not possess genius: "By genius I would understand that power, or rather those powers of the mind which are capable of penetrating into all things within our reach and knowledge, and of distinguishing their essential differences. These are no other than invention and judgment: and they are both called by the collective name of genius, as they are of those gifts of nature which we bring with us into the world. Concerning each of which, many seem to have fallen into very great errors; for by invention, I believe, is generally understood a creative faculty, which would indeed prove most romance writers to have the highest pretensions to it; whereas by invention is meant no more (and so the word signifies) than discovery or finding out; or, to explain it at large, a quick and sagacious penetration into the true essence of all the objects of our contemplation. This, I think, can rarely exist without the concomitancy of judgment, for how we can be said to have discovered the true essence of two things, without discerning their difference, seems to me hard to conceive. Now this last is the undisputed province of judgment; and yet some few men of wit have agreed with all the dull fellows in the world, in representing these two to have been seldom or never the property of one and the same person."

My own judgment, if of any value, would mark the ability of Henry — I do not say his achievements — a little below that of Faraday. Indeed, their laws and their methods of working were strangely alike. Each born in humble circumstances, without any of the accessories and aids of position or wealth, was destined apparently to work out great results. Faraday was at a disadvantage to a boundless [illegible]. Henry seemed to have come equipped under a silver-

smith. Each started in life with moral and benevolent habits, well developed and healthy bodies, quick and accurate perceptions, calm judgment and self-reliance tempered with modesty and good manners,—a good ground surely in which to plant the germs of the scientific life. Each, by innate force of taste and intellect, was impelled to the pursuit of knowledge under obstacles which would have damped the ardor of ordinary youths. Each, endowed with a lively imagination, was in his younger days fond of romance and the drama; and, by a singular similarity of accidents, each had his attention turned to science by a book which chance threw in his way. This work in the case of Faraday was "Mrs. Marcet's Conversations on Chemistry," and the book which influenced Henry's career was "Gregory's Lectures on Experimental Philosophy, Astronomy and Chemistry." Of Mrs. Marcet's book Faraday thus writes:—"My dear Friend,—Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of thirteen, in the year 1804, remaining there eight years, and during the chief part of the time bound books. Now it was in those books, in the hours after work, that I found the beginning of my philosophy. There were two that especially helped me, the 'Encyclopædia Britannica,' from which I gained my first notions of electricity, and Mrs. Marcet's 'Conversations on Chemistry,' which gave me my foundation in that science.

"Do not suppose that I was a very deep thinker, or was marked as a precocious person. I was a lively, imaginative person, and could believe in the 'Arabian Nights' as easily as in the 'Encyclopædia.' But facts were important to me and saved me. I could trust a fact and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first, as one who had conferred great personal good and pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught and inquiring mind.

"You may imagine my delight when I came to know Mrs.

Marcet personally; how often have I cast my thoughts backward, delighting to connect the past and the present; how often, when sending a paper to her as a thank-offering, I thought of my first instructress, and such thoughts will remain with me."

Henry wrote on the inside of the cover of Gregory's work the following words: "This book, although by no means a profound work, has, under Providence, exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book I ever read with attention. It opened to me a new world of thought and enjoyment; invested things before almost unnoticed with the highest interest; fixed my mind on the study of nature, and caused me to resolve at the time of reading it that I would immediately commence to devote my life to the acquisition of knowledge. J. H."

Each of these philosophers worked with simple instruments mostly constructed by his own hands, and by methods so direct that he appeared to have an almost intuitive perception into the workings of nature; and each gave great care to the composition of his writings, sending his discoveries into the world clothed in simple and elegant English.

Finally, each loved science more than money, and his Creator more than either.

There was sympathy between these men; and Henry loved to dwell on the hours that he and Bache spent in Faraday's society. I shall never forget Henry's account of his visit to King's College, London, where Faraday, Wheatstone, Daniell and he had met to try and evolve the electric spark from the thermopile. Each in turn attempted it and failed. Then came Henry's turn. He succeeded; calling in the aid of his discovery of the effect of a long interpolar wire wrapped around a piece of soft iron. Faraday became as wild as a boy, and, jumping up, shouted, "Hurrah for the Yankee experiment!"

And Faraday and Wheatstone reciprocated the high estimation in which Henry held them. During a visit to England, not long before Wheatstone's death, he told me that Faraday and he had, after Henry's classical investigation of the induced currents of different orders, written a joint letter to the council of the Royal Society, urging that the Copley medal, that laurel wreath of science, should be bestowed on Henry. On further consultation with members of the council it was decided to defer the honor till it

would come with greater éclat, when Henry had continued further his researches in electricity. Henry's removal to Washington interrupted these investigations. Wheatstone promised to give me this letter, to convey to Henry as an evidence of the high appreciation which Faraday and he had for Henry's genius; but Wheatstone's untimely death prevented this.

Both Faraday and Henry gave much thought to the philosophy of education, and in the main their ideas agreed. I may, in this connection be excused from reading abstracts from a letter from Henry soon after he had received the news I had given my son his name. He says—what may be news to most of you: "I did not object to Henry as a first name; although I have been sorry that my grandfather, in coming from Scotland to this country, substituted it for Hendrie, a much less common, and therefore more distinctive name." He then proceeds: "I hope that both his body and his mind will be so developed by proper training and instruction, that he may become an efficient, wise, and good man. I say efficient and wise, because these two characteristics are not always united in the same person. Indeed, most of the inefficiency of the world is due to their separation; wisdom may know what ought to be done, but it requires the aid of efficiency to accomplish the desired object. I hope that in the education of your son due attention may not only be given to the proper development of both these faculties, but also that they will be cultivated in the order of nature: that is, doing before thinking; art before science. By inverting this order much injury is frequently done to a child, especially in the case of the only son of a widowed mother, in which a precocious boy becomes an insignificant man. On examination, in such a case, it will be generally found that the boy has never been drilled into expertness in the art of language, of arithmetic, or of spelling, of attention, perseverance, and order, or, in other words, of the habits of an active and efficient life."

Henry was a man of extensive reading, and often surprised his friends by the extent and accuracy of his information, and by the original manner in which he brought his knowledge before them. Not only was he well versed in those subjects in which one might naturally suppose him proficient, but in departments of knowledge entirely distinct from that in which he gained his reputation as an original thinker. Although without a musical ear, he had a nice feeling for the movement of a poem, and was fond of drawing from

his retentive memory poetic quotations apt to the occasion. He was a diligent student of mental philosophy, and also took a lively interest in the progress of biological science, especially in following the recent generalizations of Darwin; while the astonishing development of modern research in tracking the history of prehistoric man had for him a peculiar fascination. Yet with all his learning, reputation, and influence, Henry was as modest as he was pure.

One day, on opening Henry's copy of "Young's Lectures on Natural Philosophy"—a book which he had studied more than any other work of science—I read on the fly-leaf, written by his own hand, these words:—

> "In Nature's infinite book of secrecy
> A little I can read.
>
> *Shakespeare.*"

And did he not read a little "in Nature's infinite book of secrecy"? And did he not read that little carefully and well? May we all read our little in that book as modestly and as reverently as did JOSEPH HENRY.

SECTION A.

MATHEMATICS, PHYSICS AND CHEMISTRY.

University of MICHIGAN

ADDRESS

BY

PROFESSOR ASAPH HALL.

VICE-PRESIDENT, SECTION A.

FELLOW-MEMBERS OF THE ASSOCIATION:—

ASTRONOMY, in some of its forms, reaches back to the most distant historical epochs, and the changes that it has undergone during this long lapse of time give to this science a peculiar interest. In no other branch of human knowledge have we such a long and continuous history of the search after truth, of the painful struggle through which men have passed in freeing themselves from theories approved by the wise of their own times, and in overthrowing beliefs which had become incorporated into the life and culture of those times. Perhaps the grand array of the heavens, and the vast phenomena which they display, naturally led men to the invention of complicated theories; but these passed away at last before the test of observation, and the criticism of sceptical men; and the Copernican theory of our solar system, Kepler's laws of elliptical motion, and the Newtonian law of gravitation, gave to astronomy a real scientific character.

The discovery of the laws that govern the motions of the heavenly bodies, and the construction of the theory of these motions, demanded from practical astronomy better observations and a more accurate determination of the orbits of the planets and the moon, or of the constants that enter into the problems of celestial mechanics;

and this demand led to an improvement in the instruments, and in the art of observing. The astronomers and instrument-makers of England and France led the way in these improvements. The great national observatories of those countries were established, and in England, Flamsteed and Sharp, Bird and Bradley, were foremost in raising practical astronomy to the condition of satisfying the demands of theory. But theoretical astronomy was soon to receive a wonderful advancement. Perhaps no one contributed more powerfully to this progress than Lagrange. The writings of this man are models of simplicity and elegance; and yet so complete and general are his investigations, that they contain the fundamental theorems of celestial mechanics. By the invention and perfection of the method of the variation of the arbitrary constants of a problem, and by the establishment of the differential equations of a planetary orbit depending on the partial differential coefficients of a single function, Lagrange reduced the question of perturbations to its simplest form, and gave the means of deducing easily the most interesting conclusions on the past and future condition of our solar system. To supplement this great theorist, there was needed another kind of genius. Combining the highest mathematical skill with unequalled sagacity and common sense in its application, Laplace gathered up and presented in a complete and practical form the whole theory of celestial mechanics. Besides his numerous and brilliant discoveries in theoretical astronomy, Laplace gave us some of the finest chapters ever written on the theory of attraction,* and a complete treatise on the calculus of probability.

By such labors as these the questions of astronomy were brought into order and classified, and the attention of astronomers was directed better than ever before to the determination of the quantities which must be found from observation. Moreover, the refinement of analysis and the completion of theory brought out new and more delicate questions, not less interesting, and requiring more complete investigation and more powerful instruments. The careful examination and study of the instruments and methods of observation became necessary, as well as complete and rigorous methods of reduction; and finally there was needed a critical and satisfactory method for the discussion of observations. For these last improvements in astronomy we are indebted chiefly to the astronomers and mechanics of Germany.

* "Ein schönes Document der feinsten analytischen Kunst."— GAUSS.

Among those who contributed by means of their optical and mechanical skill to furnish astronomy with the instruments necessary for its further advancement, no one holds a more honorable place than Joseph Frauenhofer. This man began his scientific work at the age of twenty-two, and died at thirty-nine, and yet in those seventeen years he gave to astronomy great improvements in the manufacture of optical glass, driving clocks for equatorials, and telescopes and micrometers, that in the hands of Bessel and Struve gave to observations a degree of accuracy hardly thought of before. To such men as Frauenhofer and his co-workers, who have carried on and improved the construction of instruments of precision, practical astronomy owes much; and yet, after all, the principal thing in a science is the man himself. No matter how excellent the instruments may be, the question whether they shall be used for the advancement of the science, and shall contribute the full value of their peculiarities to help towards increasing the accuracy of astronomical determinations, depends wholly on the astronomer. Again, astronomy is now so completely a science, and all its operations are so closely connected with theory, that no one is fit to have charge of an extended series of astronomical observations who has not a fair amount of theoretical knowledge. Without such knowledge his labor is apt to be thrown away, and is never so effective.

As a good example of what the modern astronomer should aim to be, we may take Bessel. To this man we owe a large part of our best methods for the examination and determination of the errors of our instruments, and the introduction of complete and rigorous methods for the reduction of observations. Bessel's reduction and discussion of Bradley's observations was a master-piece of its kind, bringing out the value of Bradley's work, which had lain unnoticed for more than half a century, and forming a starting-point for sidereal astronomy. This work was continued and perfected in his tables for the reduction of astronomical observations, published twelve years afterwards; a work that has done more than anything else to introduce order and system into practical astronomy. In the discussion of instruments and the determination of their errors, Bessel's conception of an instrument was that of a geometrical figure, and the positions of the lines and divisions of this instrument were considered with corresponding rigor. Although devoted almost entirely to astronomy, yet Bessel was an able mathematician, and of this he has left abundant proof. It seems to be

necessary that a man should die and be forgotten personally before his work can be fairly estimated; but time adjusts these matters at last, and I know of no astronomer whose work promises to endure the judgment of the future better than that of F. W. Bessel.

It has been said that for producing the most puzzling compound of metaphysics and mathematics, something which has neither height nor depth, nor length nor breadth, and which no one can understand, the German mathematician is unequalled. And at the same time it must be said that, for clearness of conception, and beauty and precision of expression, Germany has produced in Gauss a mathematician who is unsurpassed, and who is worthy a place by the side of Lagrange. Omitting all reference to the works of Gauss in theoretical astronomy and in geodesy, which are many and important, I refer here only to his method for the discussion of observations, and of deducing the most probable values of our constants. Almost the entire work of astronomy is a vast system of numerical approximation, in which the first steps are obvious and easy, but where the theory soon becomes complicated and the labor enormous. Thus the calculation of the approximate orbit of a planet or of a comet is the work of only a few hours; but the computation of the perturbations, and the correction of the elements from all the observations, may be the work of months and years. It is therefore of the highest importance that we should have a method for the discussion of observations that will give us the best result, and which will introduce order and system into this department of astronomy. Such a method is that of least squares. For the complete theory of this method, and for nearly all the arrangements and algorithms necessary for its practical application, we are indebted to Gauss. The invention and application of this method to the discussion of observations of all kinds seems to me one of the greatest improvements of modern times, and its proper use will lead to a steady progress in astronomy. We must remember, however, that this method does not undertake the improvement of the observations themselves, as some have seemed to think; but, when rightly used, it produces simply the best result we can hope for from a given series of observations. It does not, therefore, dispense with skill and judgment on the part of the astronomer, but one is tempted to say that, if he has not these prime qualities, then the next best thing for him to have is the method of least squares. The use of this method has become one of the chief characteristics

of modern astronomy, and if we compare the results of its application with those of the older methods, we shall see its superiority. Thus, for example, no astronomer of to-day, who is accustomed to the modern methods of discussion, would be satisfied with the manner in which Bouvard represents in his tables the observations of Jupiter and Saturn, but would suspect at once some error in his theory of the motions of these planets.

The present condition of astronomy is the result of the continued labors of our predecessors for many generations; and to this result the lapse of time itself has largely contributed. For the full development of the secular changes of our solar system, for an accurate knowledge of the proper motions of the stars of our sidereal universe, and of the great changes of light and heat that are going on among them, the astronomer must wait until future ages. It is his present duty to prepare for that future by making the observations and investigations of his own day in the best manner possible; and to do this needs a careful consideration of the present condition of the science. Although the objects for observation have become so numerous, and the range of investigation so wide, that there is room for the most varied talent and skill, yet there is danger that there may be a waste of labor, either in duplicating work, or in doing it in an improper manner. Especially may this happen in observations of the principal planets of our system, and of the fixed stars. In the case of the planets the observations are abundant, and the orbits are already well determined, except that of Neptune, for which, on account of its slow motion, we must of necessity wait for time to develop its small peculiarities, if such there be. For all these planets the observations at one or two observatories are amply sufficient, and even then the observations ought to be confined to a short time near the opposition, or at quadrature, and so made that they may be easily combined into a single normal position, which will suffice for the theoretical astronomer. To scatter such observations over a period of several months is to throw away one's labor, and to leave to the computer the disagreeable duty of rejecting a part of the observations as useless. It seems to me, therefore, unwise for several observatories to continue heaping up observations of the four outer planets of our system, when ten observations a year of each planet will give all the data that are needed. Again, for all the principal planets, observation is now in advance of theory, except, perhaps, in the case of one or two of them.

Thus, for Saturn, all the tables are decidedly in error, and, although an attempt has been made to accuse the observations of this planet, it is quite certain that the trouble lies in the theory; for in the case of Jupiter and Saturn we have the most complicated planetary theory of our system, and one that has not yet been completely developed. It seems to me, also, that observations of our moon might well be confined to one or two observatories. Here again observation is far in advance of theory, if indeed there be now in use anywhere a pure lunar theory. All the lunar ephemerides that we have are affected with empirical terms, and the lunar theory itself remains an unsolved mystery. In this case there is no attempt to impeach the observations. The trouble seems to be with the perturbations of long period, and this does not call for numerous observations during each lunation. By a proper consideration of these matters astronomers may, I think, save themselves much useless labor.

Observations of the fixed stars are of the utmost importance in astronomy, since the positions of the stars are the fundamental points on which depends our knowledge of the motions of the planets, the moon, and of the stars themselves; and it is on account of this fact that Bessel's tables, published in 1830, were of such great service, since they introduced correct and elegant methods of reduction, and clearly defined all the constants and epochs. We now have the positions of several hundred stars so well known that they may be safely used in the reduction of observations; and for these accurate positions we are largely indebted to the astronomers of the Pulkowa Observatory, who have made such absolute determinations a special work. There is still an opportunity for the improvement of these positions, and every well-executed determination will be of value; but it is doubtful if crude and irregular observations can add anything to our knowledge of the positions of these stars. Neither can the routine, mechanical style of observing, that is apt to prevail in large observatories, be of much use here. It would be better in most cases for such observatories to assume the positions of the fundamental stars, and to leave the further improvement of their places to skilful astronomers who understand the theory of such work, and who carefully study and become masters of their instruments. In these refined observations the refraction of light by our atmosphere also plays an important part, and this question will need to be examined at every observatory that undertakes to do independent work. It is true that every new and good

meridian instrument may, and perhaps ought, to contribute something towards removing constant errors, and giving us a more accurate knowledge of a star's position; but when this position is very well known, the only way for further improvement is through complete and careful observations, and their thorough reduction and discussion.

In the observations of double stars but little had been done before the present century, and the labors of W. Struve form the real starting-point in this branch of astronomy. These labors have been ably continued by his son, the present Director of the Pulkowa Observatory, and the observations of these two astronomers, extending over a period of nearly sixty years, are of the greatest value for our knowledge of the motions of the double stars. This is a branch of the science into which irregular workers are apt to enter, and where some of them have done good service; but if any amateur astronomer will compare his own work with that of the Struves, and will study the methods followed by them in determining their personal and instrumental errors, and will emulate the steadiness with which they have followed out their purpose, he can do much to enhance the value of his labor. Here the observations are simple, and easily reduced, and the chief requisites are skill and patience on the part of the observer. He should not be discouraged because he obtains no immediate or great reward for his work, or public notice, or because some one who rants about the nebular hypothesis and kindred subjects of which he knows nothing is for a time the great astronomer of the day. The observer will learn finally that a good observation of the smallest double-star, or of the faintest comet or asteroid, is worth more than all such vague talk. The observation has a positive value, however small, but the physical theories of the universe, of which modern popular science is so productive, are generally worse than useless.

The first step towards a rational and trustworthy knowledge of our sidereal universe must come from a determination of the distances of the stars. The solution of this problem was attempted soon after the Copernican theory of our solar system was established, when it was seen that we have a long base line for our measures, or the diameter of the earth's orbit, and it was supposed that the solution would be easy. These early trials were all failures, but they led to some very interesting and important discoveries, such as Bradley's discovery of the aberration of light; to the knowl-

edge of the fact that the determination of the parallaxes, or the distances of the stars, although simple in theory, is practically a difficult question; and then to an improvement in the instrumental means of observation, to a careful study of the methods of observation and the instruments, and to a recognition of the necessity of a complete and rigorous reduction of the observations. An examination of these early attempts is an instructive study. It is only about forty years ago that the solution of this problem was at last attained, and then only by the application of the most powerful instruments, and the best observing skill. An interesting result of the determinations of stellar parallax is obtained at once in the check it puts on speculations concerning the structure of the sidereal universe. The first astronomers who considered the parallaxes of the stars very naturally assumed that the bright stars are nearer to us than the faint ones, and therefore they observed the bright stars for parallax. Now, while this assumption may be true as a general statement, the actual determinations of parallax show that some of the faint stars which are not visible to the naked eye are much nearer to us than the brightest stars of our northern sky. Again it was assumed that a large proper motion is a certain index of a star's nearness to us; but observation shows that this also may be an erroneous assumption. This is a problem whose solution is only just begun, but already we know enough of its difficulties to see that we need the most powerful micrometrical apparatus that can be brought into use. The invention of some micrometer that, while as accurate as the present filar micrometer, would give the observer a much greater range of observation, and enable him to select suitable stars of comparison, is something much to be desired. At present the heliometer seems to be the best instrument for observations of this kind. Formerly it was thought that photography would furnish a good method for such delicate determinations; but so far the photographic methods have not given the necessary degree of accuracy in the measurements, and the astronomical use of photography is confined mostly to descriptive astronomy, where, especially in solar eclipses, it has rendered excellent service. Closely connected with the parallaxes of the stars and their proper motions is the interesting question of determining their motions to or from our sun according to the theory of Doppler. Here likewise the numerical determinations are so discordant, that we cannot have much confidence in the results. In both these cases we need more pow-

erful apparatus, and a complete and thorough investigation of the methods of observation. Perhaps some of the large instruments now constructing may be employed in these methods, and we may soon have better results.

A great advance has been made in cataloguing the fainter stars. This work was begun by the French astronomers nearly a century ago, and was continued by Bessel, Argelander, and others. An important step towards the completion of this work was taken by Argelander and his assistants in their great catalogue of the approximate positions of 324,198 stars, which was finished in 1861. This census of the stars will soon be extended, we hope, over the whole heavens; and it already forms the groundwork for the great zone observations of stars now going on in Europe and in this country, and which must be nearly finished. These observations will doubtless reveal many interesting cases of the proper motion of the stars, and will certainly form the basis for a knowledge of the motion of our solar system in space, and for sidereal astronomy generally, such as we have never had before. Our American observatories can render a good service by observing stars of southern declination, since our observatories are ten or twelve degrees farther south than those of Europe, and thus have an advantage of position which ought to be made use of; and which may serve to unite into a harmonious system the observations made in the northern and southern hemispheres. The work of mapping the very faint stars near the ecliptic has also been greatly extended, and it is to this extension that we owe the rapid increase in the number of the small planets between Mars and Jupiter. But besides aiding in the discovery of the asteroids, accurate charts of the small stars have a permanent value in giving us a knowledge of the heavens at their epoch, and also some idea of the distribution of the stars in space.

It is an interesting question whether, among the thousands of nebulæ that are scattered over the heavens, any of them show changes of form or of brightness. These objects seem to be at least as distant as the stars, and as they have sometimes an area of several degrees, they must be bodies of an enormous extent. That changes are going on in these bodies seems probable, but to be visible at such distances the changes must be very great. In this case there is need of much caution in the discussion of the drawings made at different epochs, and by different astronomers

with telescopes of different power; since the nebulæ change their appearance with the telescope used, with different conditions of the air, and with a variation of their altitude above the horizon. Here the excellent photometers that have been recently invented, and which are being so well applied to the determination of the brightness of the stars, may give us assistance. Perhaps also new drawings of the nebulæ, and their criticism and discussion, and a full recognition of the difficulties of making such drawings, will soon lead to a decision of the question of their change of form. Since the study of the light of the stars with new and improved photometers has now become a specialty, we may look for more exact and continued observations of the variable stars. This is a matter of which we know but little, and it is one where a persevering observer may do good service. Although he may not find any immediate encouragement in the discovery of remarkable relations among these stars, or the probable cause of their variability, he will be collecting observations that must form the test of every theory. As examples of the result of intelligent and persevering observation, we have the case of the sun spots, which led directly to the discovery of their period, and its singular variability; and that of the shooting stars, which has shown us a very curious relation between these meteors and the comets, and one which may open to us the most extensive views of the relations between our own solar system and other systems in space.

The present condition of astronomy, with its vast and rapidly increasing store of accurate observations, offers many interesting subjects to the theoretical astronomer. The observations of the stars are now so numerous, and have been so fully reduced and criticised, and the time during which the observations have been made is so extended, that we shall soon have excellent data for a new and very exact determination of the constant of precession. The orbits of the planets and the moon, and their masses, are now so well known that little uncertainty can arise from this source; and by taking into the calculation a great number of stars in different parts of the heavens, we may be able to determine the motion of the solar system in space, as well as the constant of precession. The constant of aberration also needs a new determination, and since this constant is so closely connected with the theory of light and its velocity, and the methods of its determination are still under discussion, it would be well if several astronomers could determine this constant inde-

pendently. The value we now use was found by W. Struve from prime-vertical observations, and is apparently very accurate; but no astronomical constant should depend on the work of a single astronomer with a single instrument, when it can be determined so easily and by other methods. The old method of finding the value of this constant from the eclipses of Jupiter's satellites may yet give us a trustworthy value. The value of the other constant necessary for the reduction of observations, that of nutation, must be nearly that found by Peters in his well-known investigation of this question. This value may be verified by a new series of observations of Polaris, or of the declinations of stars situated so that this constant has its full influence on the reductions.

There are many subjects in astronomy that need investigation, but in most cases the labor required is very great, and the completion of the work would occupy a long time. This follows of course from the fact that, with the refinement of observations and their exact reduction, many small terms must be considered which formerly could be neglected. The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come. This will no doubt be the case until some able astronomer, with the will and perseverance of Delaunay, shall undertake its complete revision. This question should now be looked on as a purely scientific one, and its definite solution should be undertaken. The theory should not be patched up by guesswork to fit the observations, but should be carried out with the utmost rigor. This is a problem to which a young and able mathematician may well devote his life, and we must expect its solution from some such clear-headed devotee of science. Several of the planetary theories need a new investigation, and some of them are already in the hands of able astronomers. That of Mercury is especially interesting in connection with the intra-Mercurial planets, and it is to be hoped that Leverrier's theory of this planet may soon have a careful revision.

Again, among the secondary systems, the satellites of Jupiter and Saturn offer many interesting questions to the astronomer. At present the satellites of Jupiter demand a more complete theory, and new tables of their motions. Corrected elements of these satellites may be required for reducing observations of their eclipses, and for deriving a new value of the constant of aberration. These satellites form a peculiar and interesting system, and their theory is so com-

plicated that the labor of correcting their elements and forming new tables would be great, but still within the power of a persevering astronomer. The recent discovery of the connection of comets with streams of meteors has given additional interest to cometary astronomy, and there is plenty of hard work to be done in reducing observations, in computing perturbations, and in deducing the best orbits of the comets. The periodical comets have another interest, since they may give us information concerning the matter filling space. It seems to be probable from different reasons, such as the consideration of the light of the stars, that there must be matter spread throughout the celestial spaces; but the only heavenly body that has directly given us information on this subject is Encke's comet, which has a period of $3\frac{1}{3}$ years. For a long time the motion of this comet was very completely computed by Encke, whose calculations show very strong proof of a resisting medium. These calculations were continued by Von Asten, whose early death prevented him from finishing his work, and the theory of this comet is left in an unsatisfactory condition. It is very desirable that the motion of this comet should be completely investigated, and although the method of the special perturbations of the elements followed by Encke is probably the best that can be used, still in such a case it would be well to apply various methods. Here again, on account of the frequent returns of the comet, the labor of computation is very great, and probably would be enough fully to occupy the time of one astronomer. The interesting questions connected with the motion of this comet ought to induce some one to undertake this laborious work, and these questions are so important that two or three astronomers might well be employed on its theory.

The methods of astronomy have now become so well established, that the future advancement of the science is assured, especially since long intervals of time give an increased value to observations. Yet we may hope for improvement in instruments, for the introduction of new methods of observing, for better trained and more efficient astronomers; and perhaps also the rapid advancement of the physical sciences may furnish us with new and more powerful methods of investigation. There is an intimate relation between the instrument-maker and the astronomer, and they should understand each other better than is generally the case. It may seem a small matter that the divisions of a circle, or of a scale, should not be too finely or too coarsely cut; that the reading scale should not be placed in an

inconvenient position, and that the illumination of the instrument should be carefully studied, and brought under the control of the astronomer; but these are really essential points, and, if not rightly arranged, are certain to weary the observer and to impair the quality of his work. Such mistakes will not be remedied until the makers better understand the uses of an astronomical instrument, and have correct ideas of the ends to be attained. Since our American opticians have placed themselves at the head of their craft, we may hope that our instrument-makers will do likewise, and that they will soon be able to furnish us with the best instruments of precision.

There is one point to which astronomers should give more attention, and from which we may reasonably hope that great advantages to astronomy may come; and that is to the selection of sites for new observatories. It is possible, perhaps probable, that our instruments may be greatly enlarged and improved, and that important discoveries and improvements in the manufacture of optical glass may be made; but it seems certain that we have within easy reach very decided advantages for astronomical work by the choice of better positions for our instruments. Very few American observatories have been established for the purpose of doing scientific work, or with much thought or care for their future condition; but generally they are built in connection with some college or academy, and are the product of local and temporary enthusiasm, which builds an observatory, equips it with instruments, and then leaves it helpless. The atmosphere that surrounds us, and its sudden changes of temperature, are the great obstacles to the good performance of a telescope; and the larger the instrument, and the higher the magnifying power, the more serious are these hindrances. Now, with our present means of travel, we can easily place our instruments at an altitude of eight or ten thousand feet, and above a large part of the atmosphere. In this way we may be able to do with small instruments what at common altitudes can be done only with large ones; and when possible it is always better to use small instruments, since they are more easily handled, and are relatively stronger and better than large ones. Uniformity of temperature may be secured by seeking locations in the tropical islands, or on coasts like that of California, where the ocean winds keep the temperature nearly uniform throughout the year. At great altitudes we may secure a clearness of vision that would be of the greatest value in the examination of faint objects, and by this means, and by persevering and

continuous observation, interesting discoveries may be made. It is a matter of course that, except in the case of comets, the future discoveries in astronomy will belong to faint and delicate objects; but these are interesting, and should not be neglected. A uniform temperature, which secures good definition, and steady images of the stars, is necessary for accurate determinations of position, and for all measurements of precision. This condition is especially important in such work as that of stellar parallax, the determination of the constant of aberration, and wherever the yearly change of temperature may act injuriously. In the selection of better sites for observatories, I think we have an easy means of advancing astronomy.

As this science grows and expands, it will become more and more necessary to study the economy of its work, in order that astronomers may bestow their labors in the most advantageous methods, and may rid themselves of all cumbersome and time-consuming processes. The manner of publishing observations has already been much abbreviated, and improved I think, by some of the European astronomers, and this change seems destined to become universal. As the positions of many objects are now well known, the need of printing all the details of the observation, such as the transits of the wires, the readings of the micrometers, etc., is very slight; and this printing may be safely abandoned. Even this change will lead to a great saving in the time and cost of printing. But this will necessitate a more complete discussion of the work, and a more careful examination of the instruments; things to be desired, since they tend to lift the observer out of his routine, and make him a master of his business. There are objections to this change, and some of them are real, such as the importance of publishing a complete record; but this is overestimated, I think, since the original records ought always to be referred to in case of doubt; and other objections are factitious, such as the need of publishing a large and showy book in order to impose on the public.

We may hope also for improvements in theoretical astronomy, and for the better training and preparation of students of this science. I know that it is sometimes said that theoretical astronomy is finished, and that nothing more can be done. Such assertions come from professors who are old and weary, or from those young men who tire out early in life; but they are wrong. The improvements that Hansen has made in the theory of perturbations,

and Poinsot's study of the theory of rotation, show what careful investigation may do, and assure us of further progress. It must be confessed that some of the astronomical work done in our country bears evidence that the astronomers did not understand the correct methods of reduction, and much of it shows evidence of hasty and ill-considered plans. This is perhaps a natural condition for beginners, but we trust that it has been outgrown. An actual need for the astronomical students of our country is a good book on theoretical astronomy, similar to Pontécoulant's work, in which the whole subject shall be presented in a complete form, such as we find in the *Mécanique Céleste*, together with an account of the improvements made by Gauss, Poisson, Hansen, and others. There is no American book of this kind, and the English works are too partial, designed apparently to fit the student for college examinations, and not to give him a complete knowledge of the science. Such a book has hardly been attempted in our language, unless that of Woodhouse may be an exception, and it may be a long time in coming, since it requires a man qualified to do the work, and will involve an expense of labor in the preparation, and of cost in publishing, such as few are willing to incur. In the mean time it is far better for the student to go directly to the writings of Lagrange and Laplace, of Gauss and Poisson and other masters, rather than to spend time in reading second-rate authors who endeavor to explain them. And generally this will be found the easier way also, since the student avoids the confused notions and symbols, and the grotesque expressions and egotism of small men, and is lifted into the region of ideas and invention.

In presenting his exposition of the nebular hypothesis, which has since become so celebrated, Laplace says: "I present this hypothesis with the distrust which everything ought to inspire that is not a result of observation or of calculation." It is a singular fact that, among all the writings on the nebular hypothesis, I have never seen a reference to this presentation of it by its most distinguished advocate; and yet this is the true spirit of scientific astronomy. Laplace did not wish to exempt his own theories from criticism, and neither should any one. In astronomy there is no final human authority, no synod or council, but simply an appeal to reason and observation. If a theory or a discovery be true, it will stand the test of observation and of calculation; if false, it must pass away to that Miltonian limbo where so many things have gone and are

going. The question is sometimes asked, Of what use is astronomy? and the reply generally made is that it has conferred great benefits on navigation and on commerce, since it is by means of his astronomical knowledge that the sailor determines the position of his ship on the ocean. There is a truth in this reply, but it is only partial. The great value of astronomy is that it is really a science, and that it has broken the path and led the way through which all branches of science must pass if they ever become scientific. It is the spirit of honest, unrelenting criticism, and of impartial examination, that finally eliminates error and awards to every one his just due, that makes astronomy honorable and attractive; and it is by cultivating this spirit that astronomy confers its chief benefit, for it is this that shall break in pieces and destroy all false assumptions in science and in philosophy.

PAPERS READ.

On the Production and Reproduction of Sound by Light.

By Alexander Graham Bell, of Washington, D. C.

In bringing before you some discoveries made by Mr. Sumner Tainter and myself, which have resulted in the construction of apparatus for the production and reproduction of sound by means of light, it is necessary to explain the state of knowledge which formed the starting point of our experiments.

I shall first describe that remarkable substance "selenium," and the manipulations devised by previous experimenters; but the final result of our researches has widened the class of substances sensitive to light vibrations, until we can propound the fact of such sensitiveness being a general property of all matter.

We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, german-silver, Jenkin's metal, Babbitt's metal, ivory, celluloid, gutta-percha, hard rubber, soft vulcanized rubber, paper, parchment, wood, mica, and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscope glass.[1]

We find that when a vibratory beam of light falls upon these substances *they emit sounds*, the pitch of which depends upon the frequency of the vibratory change in the light. We find further, that when we control the form or character of the light-vibrations on selenium (and probably on the other substances), we control the quality of the sound, and obtain all varieties of articulate speech. We can thus, without a conducting wire as in electric telephony, speak from station to station wherever we can project a beam of light. We have not had the opportunity of testing the

[1] Later experiments have shown that these are not exceptions.

limit to which this photophonic effect may be extended, but we have spoken to and from points 213 metres apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available.

I shall now speak of selenium.

Selenium.—In the year 1817, Berzelius and Gottlieb Gahn made an examination of the method of preparing sulphuric acid in use at Gripsholm. During the course of this examination they observed in the acid a sediment of a partly reddish, partly clear brown color, which, under the action of the blowpipe, gave out a peculiar odor, like that attributed by Klaproth to tellurium.

As tellurium was a substance of extreme rarity, Berzelius attempted its production from this deposit, but he was unable after many experiments to obtain further indications of its presence. He found plentiful signs of sulphur mixed with mercury, copper, tin, zinc, iron, arsenic and lead, but no trace of tellurium.

It was not in the nature of Berzelius to be disheartened by this result. In science every failure advances the boundary of knowledge as well as every success; and Berzelius felt that if the characteristic odor, that had been observed, did not proceed from tellurium, it might possibly indicate the presence of some substance then unknown to the chemist. Urged on by this hope he returned with renewed ardor to his work.

He collected a great quantity of the material and submitted the whole mass to various chemical processes. He succeeded in separating successively the sulphur, the mercury, the copper, the tin, and the other known substances, whose presence had been indicated by his tests; and after all these had been eliminated, there still remained a residue, which proved upon examination to be what he had been in search of—*a new elementary substance.*

The chemical properties of this new element were found to resemble those of tellurium in such a remarkable degree that Berzelius gave to the substance the name of "selenium," from the Greek word σελήνη, the moon ("tellurium," as is well known, being derived from *tellus*, the earth). Although tellurium and selenium are alike in many respects, they differ in their electrical

properties; tellurium being a good conductor of electricity, and selenium, as Berzelius showed, a non-conductor.

Knox[2] discovered in 1837, that selenium became a conductor when fused; and Hittorff,[3] in 1851, showed that it conducted at ordinary temperatures when in one of its allotropic forms.

When selenium is rapidly cooled from a fused condition it is a non-conductor. In this, its "vitreous" form, it is of a dark brown color, almost black by reflected light, having an exceedingly brilliant surface. In thin films it is transparent, and appears of a beautiful ruby red by transmitted light.

When selenium is cooled from a fused condition with *extreme slowness*, it presents an entirely different appearance, being of a dull lead color, and having throughout a granular or crystalline structure and looking like a metal. In this form it is opaque to light even in very thin films. This variety of selenium has long been known as "granular" or "crystalline" selenium; or, as Regnault called it, "metallic" selenium. It was selenium of this kind that Hittorff found to be a conductor of electricity at ordinary temperatures.

He also found that its resistance to the passage of an electrical current diminished continuously by heating up to the point of fusion; and that the resistance suddenly increased in passing from the solid to the liquid condition.[4]

It was early discovered that exposure to sunlight[5] hastens the change of selenium from one allotropic form to another; and this observation is significant in the light of recent discoveries.

Although selenium has been known for the last sixty years, it has not yet been utilized to any extent in the arts, and it is still considered simply as a chemical curiosity. It is usually supplied in the form of cylindrical bars. These bars are sometimes found to be in the metallic condition, but more usually they are in the vitreous or non-conducting form.

It occurred to Willoughby Smith that on account of the high resistance of crystalline selenium, it might be usefully employed at the shore-end of a submarine cable, in his system of testing

[2] Trans. Roy. Irish Acad. (1839), Vol. XIX, p. 147; also Phil. Mag. (3d ser.), Vol. XVI, p. 185.

[3] Pogg. Ann., LXXXIV, 214; also Phil. Mag. (4th ser.), Vol. III, p. 546.

[4] See Draper and Moss in Proc. Roy. Irish Acad., Nov., 1873 (2nd ser.), Vol. I, p. 529.

[5] Gmelin's Handbook of Chemistry (1849), Vol. II, p. 235; see also Hittorff in the Phil. Mag. for 1852 (4th ser.), Vol. III, p. 547.

and signaling during the process of submersion. Upon experiment, the selenium was found to have all the resistance required; some of the bars employed, measuring as much as 1400 megohms —a resistance equivalent to that which would be offered by a telegraph wire long enough to reach from the earth to the sun! But the resistance was found to be extremely variable. Efforts were made to ascertain the cause of this variability, and it was discovered that *the resistance was less when the selenium was exposed to light than when it was in the dark!*

This observation first made by Mr. May[6] (Mr. Willoughby Smith's assistant, stationed at Valentia) was soon verified by a careful series of experiments, the result of which was communicated by Mr. Willoughby Smith[7] to the Society of Telegraph Engineers, on the 17th of February, 1873. Platinum wires were inserted into each end of a bar of crystalline selenium, which was then hermetically sealed in a glass tube, through the ends of which the platinum wires projected for the purpose of connection. One of these bars was placed in a box, the lid of which was closed so as to shade the selenium, and the resistance of the substance was measured.

Upon opening the lid of the box, the resistance instantaneously diminished. When the light of an ordinary gas burner (which was placed at a distance of several feet from the bar) was intercepted by shading the selenium with the hand, the resistance again increased; and upon passing the light through rock salt, and through glasses of various colors, the resistance was found to vary according to the amount of light transmitted. In order to be certain that temperature had nothing to do with the effect, the selenium was placed in a vessel of water so that the light had to pass through a considerable depth of water in order to reach the selenium. The effects, however, were the same as before. When a strong light from the ignition of a narrow band of magnesium was held about nine inches above the water, the resistance of the selenium immediately fell more than two-thirds, returning to the normal condition upon the removal of the light.

The announcement of these results naturally created an intense interest among scientific men, and letters of enquiry regarding

[6] See lecture by Siemens, in Proc. Roy. Inst. of Great Britain, Vol. VIII, p. 68.

[7] Jour. of Soc. of Teleg. Engin., Vol. II, p. 31 (1873); Nature, Vol. VII, p. 303; Amer. Jour. of Science and Arts (3d ser.), Vol. V, p. 301.

the details of the experiment soon appeared in the columns of Nature, from Harry Napier Draper[8] and Lieut. M. L. Sale,[9] which were answered in the next number by Willoughby Smith.[10]

Sale and Draper were soon able to corroborate the statements that had been made by Willoughby Smith.

Sale[11] presented his researches to the Royal Society on the 8th of May, 1873, and in the following November, Draper[12] presented his results to the Royal Irish Academy in the shape of a joint paper by himself and Richard J. Moss.

Draper and Moss gave in their paper an admirable summary of the condition of our knowledge regarding selenium at that time. They confirmed Hittorff's observation that the temperature of minimum resistance of granular selenium was somewhere about 210° C., and that at 217° C. (the fusing point), the resistance suddenly increased. They carried the temperature to a still higher point than Hittorff had done, and found that the resistance again diminished, reaching a second minimum at 250° C.

During the course of their experiments they produced a variety of granular selenium not different in appearance from other specimens but having different electrical properties. In this form the resistance became greater instead of less when the temperature was raised.

They also used thin plates of selenium instead of the cylindrical bars formerly employed, and found great advantage from the increased sensitiveness of the former to light.

Sale found upon exposing selenium to the action of the solar spectrum that the maximum effect was produced just at or outside the extreme edge of the red end of the spectrum at a point nearly coincident with the maximum of the heat rays, thus rendering it uncertain whether the effect was due to light or to radiant heat.

In the winter of 1873–4 the Earl of Rosse[13] attempted to de-

[8] Nature, Vol. VII, p. 340, Mch. 6, 1873. [9] *Ibid.* [10] Nature, Vol. VII, p. 361, Mch. 13, 1873.

[11] Proc. Roy. Soc., Vol. XXI, p. 283; see also Pogg. Ann., Vol. CL, p. 333; Phil. Mag. (4th ser.), Vol. XLVII, p. 216; Nature, Vol. VIII, p. 134.

[12] Proc. Roy. Irish Acad. (2nd ser.), Nov. 10, 1873, Vol. I, p. 529; see also a communication from Richard J. Moss to Nature, Aug. 12, 1875, Vol. XII, p. 291; being an answer to a letter from J. E. H. Gordon, upon the "Anomalous behavior of Selenium," published in that journal on the 8th of July, 1875; see Vol. XII, p. 187.

[13] Phil. Mag. (4th ser.), March, 1874, Vol. XLVII, p. 161; see, also, Am. Journ. of Sci. and Arts (3rd ser.), Vol. VII, p. 512.

cide this question by comparing the selenium effects with the indications of the thermopile. He exposed selenium to the action of non-luminous radiations from hot bodies, but could produce no effect; whereas, a thermopile under similar conditions gave abundant indications of a current.

He also cut off the heat rays of low refrangibility from luminous bodies by the interposition of glass and alum between the selenium and the source of light without materially affecting the result; but when the thermopile was employed the greater portion of the heat-effect was cut off.

Later, Prof. W. G. Adams,[14] of Kings College, took up the question, and his experiments seemed to prove conclusively that the action was due principally, if not entirely, to those rays of the spectrum which were luminous, and that the ultra red or the ultra-violet rays had little or no effect.

This conclusion was supported by the marked effect produced by the light of the moon, and by the apparent insensitiveness of selenium to rays passed through a solution of iodine in bisulphide of carbon. He found that the maximum effect was produced by the greenish-yellow rays, and showed that *the intensity of the action depended upon the illuminating power of the light*, being *directly as the square root of that illuminating power*.

Professor Adams and Mr. R. E. Day[15] continued these researches; and, among other interesting and suggestive results, discovered that light produces in selenium an electromotive force without the aid of a battery.

The most sensitive variety of selenium that has yet been produced was obtained in Germany, by Dr. Werner Siemens, by continued heating for some hours at a temperature of 210° C., followed by extremely slow cooling.

Dr. C. W. Siemens,[16] in a lecture delivered before the Royal Institution of Great Britain, on the 18th of February, 1876, stated that his brother's modification of selenium was so sensitive to

[14] Proc. Roy. Soc., June 17, 1875, Vol. XXIII, p. 535; see, also, Proc. Roy. Soc., Jan. 6, 1876, Vol. XXIV, p. 163. Nature, Jan. 20, 1876, Vol. XIII, p. 238. Nature, Mar. 23, 1876, Vol. XIII, p. 419. Scient. Amer. Supplement, June 3, 1876, Vol. I, p. 354.

[15] Proc. Roy. Soc., June 15, 1876, Vol. XXV, p. 113.

[16] Proc. Roy. Inst. Gt. Brit., Feb. 18, 1876, Vol. VIII, p. 68; see also, Nature, Vol. XIII, p. 407; Scient. Amer. Supplement, Apr. 1, 1876, Vol. I, p. 222; Scient. Amer. Supplement, June 10, 1876, Vol. I, p. 375.

light that its conductivity was *fifteen times as great in sunlight as it was in the dark.*

In Werner Siemens'[17] experiments special arrangements were made for reducing the resistance of the selenium.

For this purpose two fine platinum wires were coiled into a double flat spiral and were laid upon a plate of mica, so that they did not come into contact with one another. A drop of melted selenium was then placed upon the platinum wire arrangement, and a second sheet of mica was pressed upon the selenium so as to cause it to spread out and fill the spaces between the wires. Each cell was about the size of a silver dime. The cells were then placed in a paraffine bath and annealed.

Siemens devised other arrangements of apparatus for reducing the resistance. In the form known as "Siemens' Grating," the two wires, instead of being coiled together, were arranged in zig-zag shape, forming a sort of platinum gridiron.

This was treated in the same way as the spiral arrangement. Another form of cell consisted of a sort of lattice-work or basket-work of platinum wires arranged upon a perforated mica plate, the wires interlacing with one another, and with the mica plate so as to make metallic contact only with alternate wires. He also found that iron and copper might be employed, instead of platinum.

Without dwelling further upon the researches of others I may say that all observations concerning the effect of light upon the conductivity of selenium had been made by means of the galvanometer, but it occurred to me that the telephone, from its extreme sensitiveness to electrical influences, might be substituted with advantage. Upon consideration of the subject, however, I saw that the experiments could not be conducted in the ordinary way, for the following reason: The law of audibility of the telephone is precisely analogous to the law of electric induction. No effect is produced during the passage of a continuous and steady current. It is only at the moment of change from a stronger to a weaker state, or, *vice versa*, that any audible effect is produced; and the amount of effect is exactly proportional to the amount of *variation* in the current.

[17] Monatsbericht der Kön. preuss. Akad. der Wissenschaften zu Berlin for 1875, p. 280; Phil. Mag., Nov. 1875 (4th ser.), Vol. L, p. 416; Nature, Dec. 9, 1875, Vol. XIII, p. 113; Monatsber. Berl. Akad., Feb. 17, 1876; Pogg. Ann., Vol. CLIX, p. 117; Monatsb. Berl. Akad., June 7, 1877; Pogg. Ann., 1877, Vol. II, p. 521.

It was, therefore, evident that the telephone could only respond to the effect produced in selenium at the moment of change from light towards darkness, or *vice versa*, and that it would be advisable to intermit the light with great rapidity so as to produce a succession of changes in the conductivity of the selenium corresponding in frequency to musical vibrations within the limits of the sense of hearing. For I had often noticed that currents of electricity, so feeble as hardly to produce any audible effects from a telephone when the circuit was simply opened and closed, caused very perceptible musical sounds when the circuit was rapidly interrupted; and that the higher the pitch of the sound the more audible was the effect. I was much struck by the idea of producing sound in this way by the action of light.

I proposed to pass a bright light through one of the orifices in a perforated screen consisting of a circular disk or wheel with holes near the circumference. Upon rapidly rotating the disk an intermittent beam of light would fall upon the selenium, and a musical tone should be produced from the telephone, the pitch of which would depend upon the rapidity of the rotation of the disk.

Upon further consideration, it appeared to me that all the audible effects obtained from variations of electricity could also be produced by variations of light, acting upon selenium. I saw that the effect could not only be produced at the extreme distance at which selenium would normally respond to the action of a luminous body, but that this distance could be indefinitely increased by the use of a parallel beam of light, so that we might telephone from one place to another without the necessity of a conducting wire between the transmitter and receiver.

It was evidently necessary, in order to reduce this idea to practice, to devise an apparatus to be operated by the voice of a speaker, by which variations could be produced in a parallel beam of light, corresponding to the variations in the air produced by the voice.

I proposed to pass light through a perforated plate containing an immense number of small orifices.

Two similarly perforated plates were to be employed. One was to be fixed and the other to be attached to the centre of a diaphragm actuated by the voice; so that the vibration of the diaphragm would cause the movable plate to slide to and fro over

the surface of the fixed plate, thus alternately enlarging and contracting the free orifices for the passage of light. In this way the voice of a speaker could control the amount of light passed through the perforated plates without completely obstructing its passage. This apparatus was to be placed in the path of a parallel beam of light, and the undulatory beam emerging from the apparatus could be received at some distant place upon a lens, or other apparatus by means of which it could be condensed upon a sensitive piece of selenium placed in a local circuit, with a telephone and galvanic battery.

The variations in the light produced by the voice of the speaker should cause corresponding variations in the electrical resistance of the selenium at the distant place, and the telephone in circuit with the selenium should reproduce audibly the tones and articulations of the speaker's voice.

I obtained some selenium for the purpose of trying the apparatus described, but found upon experiment that its resistance was almost infinitely greater than that of any telephone that had been constructed; and I was therefore unable at that time to obtain audible effects in the way desired. I believed, however, that this obstacle could be overcome by devising mechanical arrangements for reducing the resistance of the selenium, and by constructing special telephones for the purpose.

I felt so much confidence in this that in a lecture delivered before the Royal Institution of Great Britain, on the 17th of May, 1878, I announced the possibility of hearing a shadow by means of interrupting the action of light upon selenium. A few days afterwards my ideas upon this subject received a fresh impetus by the announcement made by Mr. Willoughby Smith,[18] before the Society of Telegraph Engineers, that he had heard the action of a ray of light falling upon a bar of crystalline selenium by listening to a telephone in circuit with it.

It is not unlikely that the publicity given to the speaking telephone during the last few years may have suggested to many minds, in different parts of the world, somewhat similar ideas to my own; indeed, it has recently come to my knowledge that a writer (J. F. W.,[19] of Kew), on the 13th of June, 1878, asked

[18] See Journ. of Soc. of Teleg. Engin., May 23, 1878, VII, 284.
[19] Nature, Vol. XVIII, p. 169.

the readers of "Nature," through the columns of that periodical, whether any experiments had been made with a telephone in circuit with a selenium galvanic element arranged as in Sabine's selenium battery;[20] and suggested that it was not unlikely that sounds would be produced in a telephone by the action of light of variable intensity upon a selenium element in circuit with it.

In September, or October, 1878, Mr. A. C. Brown, of London, submitted to me, confidentially, the details of a most ingenious invention of his, of which we may yet hear more. This invention, although entirely different from my own, involved the use of selenium in circuit with a battery and telephone, and the production of articulate speech by the action of a variable light. I am also aware that Mr. W. D. Sargent, of Philadelphia, has had some ideas of a similar nature, the details of which I do not know. I understood from Mr. Sargent that he proposed submitting selenium to the influence of an oscillating beam of light, which should be sent on and off the selenium by the action of the voice. If this is so, the effect produced would be only of an intermittent character, and a musical tone, not speech, would be heard from the telephone in circuit with the selenium.

Although the idea of producing and reproducing sound by the action of light, as described above, was an entirely original and independent conception of my own, I recognize the fact that the knowledge necessary for its conception has been disseminated throughout the civilized world, and that the idea may therefore have occurred, independently, to many other minds.

I have stated above the few facts bearing upon the subject that have come under my observation.

The fundamental idea on which rests the possibility of producing speech, by the action of light, is the conception of what may be termed an undulatory beam of light in contradistinction to a merely intermittent one.

By an undulatory beam of light I mean a beam that shines continuously upon the receiver, but the intensity of which upon that receiver is subject to rapid changes corresponding to the changes in the vibratory movement of a particle of air during the transmission of a sound of definite quality through the atmosphere. The curve that would graphically represent these changes of light

[20] Nature, Vol. XVII, p. 512, Apr. 25, 1878.

would be similar in *shape* to that representing the movements of the air. I do not know whether this conception had been clearly realized by J. F. W., of Kew, or by Mr. Sargent, of Philadelphia; but to Mr. A. C. Brown, of London, is undoubtedly due the honor of having distinctly and independently formulated the conception and of having devised apparatus, though of a crude nature, for carrying it into execution.

It is greatly due to the genius and perseverance of my friend, Mr. Sumner Tainter, of Watertown, Mass., that the problem of producing and reproducing sound by the agency of light has at last been successfully solved. For many months past we have been devoting ourselves to the solution of this problem and I have great pleasure in presenting to you to-night the results of our labors.

Researches of Sumner Tainter and Alexander Graham Bell.

The first point to which we devoted our attention was the reduction of the resistance of crystalline selenium within manageable limits. The resistance of selenium cells, employed by former experimenters, was measured in millions of ohms, and we do not know of any record of a selenium cell measuring less than 250,000 ohms in the dark.

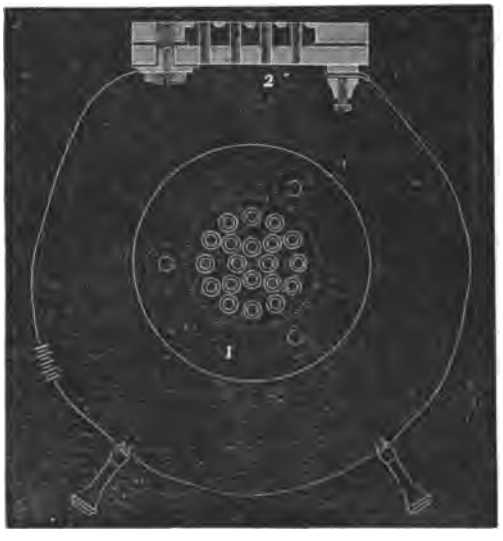
We have succeeded in producing sensitive selenium cells measuring only 300 ohms in the dark and 150 ohms in the light. All former experimenters seemed to have used platinum for the conducting part of their selenium cells, excepting Werner Siemens, who found that iron and copper might be employed. We have discovered that brass, although chemically acted upon by selenium, forms an excellent and convenient material; indeed, we are inclined to believe that the chemical action between the brass and selenium has contributed to the low resistance of our cells by forming an intimate bond of union between the selenium and brass.

We have observed that melted selenium behaves to other substances as water to a greasy surface, and we are inclined to think that when selenium is used in connection with metals not chemically acted upon by it, the points of contact between the selenium and the metal offer a considerable amount of resistance to the passage of a galvanic current, and thus serve to increase the apparent resistance of the selenium.

By using brass we have been enabled to construct a large num-

ber of cells of different forms. Time will only admit of my

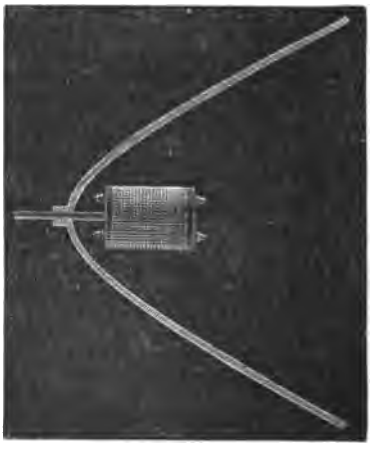
FIGS. 1 and 2.



showing you two typical forms. One of these is shown in plan in fig. 1, and in section in fig. 2.

This cell consists of two brass plates insulated from one another by a sheet of mica. The upper plate has numerous perforations, and brass pins attached to the lower plate pass through these orifices, so that their ends without touching the upper plate are flush with its surface.

FIG. 3.



The annular spaces between the pins and the plate are filled with selenium. The whole arrangement forms part of a galvanic circuit, and it will be observed that the current can only pass from the plate to the pins through the selenium rings.

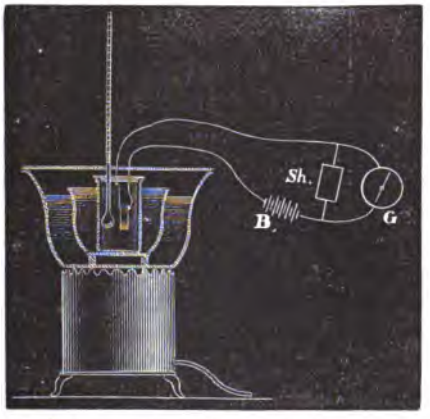
It will also be seen that owing to the conical shape of the perforations, the points of closest

approximation between the pins and the plate are on the upper surface. As the effect produced by light upon selenium is chiefly a surface action, this arrangement is found to be of great advantage.

The second typical cell is cylindrical in form for the purpose of being used with a concave reflector instead of with a lens (see fig. 3).

This cell is composed of a large number of metallic disks separated by disks of mica slightly smaller in diameter. The spaces between the brass disks over the mica are filled with selenium, and the alternate brass disks are metallically connected. The arrangement practically consists of a large number of annular selenium cells united in multiple arc.

FIG. 4.



B = battery; Sh = shunt; G = galvanometer.

The mode of applying the selenium is as follows:

The cell is heated, and when hot enough a stick of selenium is rubbed over the surface.

In order to acquire conductivity and sensitiveness the selenium must next undergo a process of annealing.

The method we first adopted was the following:

The selenium cell was placed with a thermometer in the interior of the cylindrical annealing chamber shown in fig. 4.

This was inserted in a pot of linseed oil, and the latter stood upon glass supports within another similar pot containing linseed oil. The whole arrangement was then placed over a gas stove and heated to a temperature of about 214° C., which was found to be the temperature of maximum conductivity for the selenium used.

This temperature was retained for about twenty-four hours, and the pots, with their contents, were then packed in a box so arranged as to retard radiation of heat.

The selenium took from forty to sixty hours to cool down to the temperature of the air.

A powerful battery current was passed through the selenium during the whole process of heating and cooling, in accordance with our theory that the current exerted a powerful influence in causing a set of the selenium molecules, and in retaining them in position until fixed by crystallization.

A shunted galvanometer was introduced into the circuit for the purpose of observing the changes of conductivity. We subsequently found this tedious process to be unnecessary, as during the course of our experiments we discovered a method of preparing sensitive selenium in a very few minutes.

We now simply heat the selenium over a gas stove and observe its appearance. When the selenium attains a certain temperature, the beautiful reflecting surface becomes dimmed. A cloudiness extends over it, somewhat like the film of moisture produced by breathing upon a mirror.

This appearance gradually increases and the whole surface is soon seen to be in the metallic, granular, or crystalline condition. The cell may then be taken off the stove and cooled in any suitable way. When the heating process is carried too far, the crystalline selenium is seen to melt.

Our best results have been obtained by heating the selenium until it crystallizes as stated above, and by continuing the heating until signs of melting appear, when the gas is immediately put out.

The portions that had melted instantly re-crystallize, and the selenium is found upon cooling to be a conductor, and to be sensitive to light. The whole operation occupies only a few minutes. This method has not only the advantage of being expeditious, but it proves that many of the accepted theories on this subject are fallacious.

Early experimenters considered that the selenium must be "cooled from a fused condition with extreme slowness." Later authors agree in believing that the retention of a high temperature —short of the fusing point—and slow cooling are essential, and the belief is also prevalent that crystallization takes place only during the cooling process.

Our new method shows that fusion is unnecessary, that conductivity and sensitiveness can be produced without long heating and slow cooling; and that crystallization takes place during the heating process. We have found that on removing the source of

heat immediately on the appearance of the cloudiness above referred to, distinct and separate crystals can be observed under the microscope, which appear like leaden snowflakes on a ground of ruby red.

Upon removing the heat when crystallization is further advanced, we perceive under the microscope, masses of these crystals arranged like basaltic columns, standing detached from one another —and at a still higher temperature the distinct columns are no longer traceable, but the whole mass resembles metallic puddingstone with here and there a separate snowflake, like a fossil on the surface. Selenium crystals, formed during slow cooling after fusion, present an entirely different appearance, showing distinct facets.

I will now endeavor to explain the means by which a beam of light can be controlled by the voice of a speaker.

Photophonic Transmitters.

We have devised upwards of fifty forms of apparatus for varying a beam of light in the manner required, but only a few typical varieties need be described.

(1) The source of light may be controlled, or (2) a steady beam may be modified at any point in its path.

In illustration of the first method we have devised several forms of apparatus founded upon Kœnig's manometric capsule, operating to cause variations in the pressure of gas supplied to a burner, so that the light can be vibrated by the voice.

In illustration of the second method, I have already shown one form of apparatus by which the light is obstructed in a greater or less degree, in its passage through perforated plates. But the beam may be controlled in many other ways. For instance, it may be polarized, and then affected by electrical or magnetical influences in the manner discovered by Faraday and Dr. Ker.

Let a polarized beam of light be passed through a solution of bisulphide of carbon contained in a vessel inside a helix of insulated wire, through which is passed an undulatory current of electricity from a microphone or telephonic transmitter operated by the voice of a speaker.

The passage of the polarized beam should be normally partially obstructed by a Nicols prism, and the varying rotation of the plane of polarization would allow more or less of the light to pass

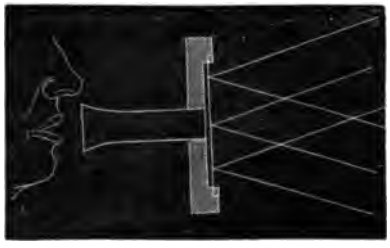
through the prism, thus causing an undulatory beam of light capable of producing speech.

The beam of polarized light, instead of being passed through a liquid, could be reflected from the polished pole of an electro-magnet in circuit with a telephonic transmitter.

Another method of affecting a beam of light is to pass it through a lens of variable focus[21] formed of two sheets of thin glass or mica containing between them a transparent liquid or gas. The vibrations of the voice are communicated to the gas or liquid, thus causing a vibratory change in the convexity of the glass surfaces and a corresponding change in the intensity of the light received upon the sensitive selenium. We have found that the simplest form of apparatus for producing the effect consists of *a plane mirror of flexible material*, such as silvered *mica or microscope-glass, against the back of which the speaker's voice is directed*, as shown in the diagram (fig. 5).

Light reflected from such a mirror is thrown into vibrations corresponding to those of the diaphragm itself. In its normal condition, a parallel beam of light falling upon the diaphragm mirror would be reflected parallel. Under the action of the voice the mirror becomes alternately convex and concave, and thus alternately scatters and condenses the light.

FIG. 5.



When crystalline selenium is exposed to the undulatory beam, reflected from such an apparatus, the telephone connected with the selenium audibly reproduces the articulation of the person speaking to the mirror.

In arranging the apparatus for the purpose of reproducing sound at a distance, any powerful source of light may be used, but we have experimented chiefly with sunlight.

For this purpose, a large beam is concentrated by means of a lens upon the diaphragm mirror and after reflection is again

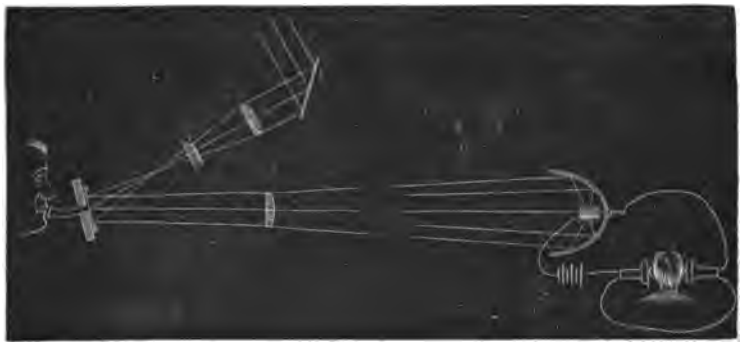
[21] I observe that a lens of similar construction has been invented in France by Dr. Cusco, and is described in a recent paper in "La Nature," June 19, 1880. See, also, Scien. Amer., Aug. 28, 1880. Vol. XLIII, p. 131. Mr. Tainter and I have used such a lens in our experiments for months past.

rendered parallel by means of another lens. The beam is received at a distant station upon a parabolic reflector, in the focus of which is placed a sensitive selenium cell, connected in a local circuit with a battery and telephone. We have found it advisable to protect the mirror by placing it out of the focal point, and by passing the beam through an alum cell, as shown in Fig. 6.

A large number of trials of this apparatus have been made with the transmitting and receiving instruments so far apart that sounds could not be heard directly through the air. In illustration, I shall describe one of the most recent of these experiments.

Mr. Tainter operated the transmitting instrument, which was placed on the top of the Franklin School House in Washington, D. C., and the sensitive receiver was arranged in one of the

FIG. 6.



windows of my laboratory, 1325 L Street, at a distance of 213 metres.

Upon placing the telephone to my ear, I heard distinctly from the illuminated receiver the words:—"Mr. Bell, if you hear what I say, come to the window and wave your hat."

In laboratory experiments the transmitting and receiving instruments are necessarily within earshot of one another, and we have therefore been accustomed to prolong the electric circuit connected with the selenium receiver, so as to place the telephones in another room.

By such experiments we have found that articulate speech can be reproduced by the oxyhydrogen light, and even by the light of a kerosene lamp. The loudest effect obtained from light is produced by rapidly interrupting the beam.

A suitable apparatus for doing this is a perforated disk which can be rapidly rotated. The great advantage of this form of apparatus for experimental work is the noiselessness of its operation, admitting of the close approach of the receiver without interfering with the audibility of the effect heard from the latter—for it will be understood that musical tones are emitted from the receiver when no sound has been made at the transmitter. A

FIG. 7.

silent motion thus produces a sound. In this way musical tones have been heard even from the light of a candle.

When distant effects are sought the apparatus can be arranged as shown in fig. 7.

By placing an opaque screen near the rotating disk, the beam can be entirely cut off by a slight motion of the hand, and musical signals, like the dots and dashes of the Morse telegraph code, can thus be produced at the distant receiving station. Such a screen operated by a key like a Morse telegraph key is shown in fig. 8, and has been operated very successfully.

FIG. 8.

Experiments to ascertain the nature of the rays that affect selenium.

We have made experiments with the object of ascertaining the nature of the rays that affect selenium. For this purpose we have

placed in the path of an intermittent beam various absorbing substances.

Prof. Cross has been kind enough to give his assistance in conducting these experiments.

When a solution of alum, or bisulphide of carbon, is employed, the loudness of the sound produced by the intermittent beam is very slightly diminished, but a solution of iodine in bisulphide of carbon cuts off most, but not all, of the audible effect. Even an apparently opaque sheet of hard rubber does not entirely do this.

This observation, which was first made in Washington, D. C., by Mr. Tainter and myself, is so curious and suggestive that I give in full the arrangement for studying the effect.

FIG. 9.

When a sheet of hard rubber, A, was held as shown in the diagram (fig. 9) the rotation of the disk or wheel B interrupted what was then an invisible beam, which passed over a space of several metres before it reached the lens C, which finally concentrated it upon the selenium cell, D.

A faint but perfectly perceptible musical tone was heard from the telephone connected with the selenium that could be interrupted at will by placing the hand in the path of the invisible beam.

It would be premature without further experiments to speculate too much concerning the nature of these invisible rays; but it is difficult to believe that they can be heat rays, as the effect is produced through two sheets of hard rubber having between them a saturated solution of alum.

Although effects are produced as above shown by forms of radiant energy which are invisible, we have named the apparatus for the production and reproduction of sounds in this way "the

Photophone," because an ordinary beam of light contains the rays which are operative.

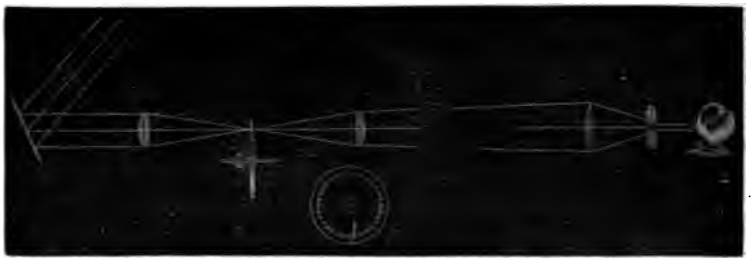
Non-Electric Photophonic Receivers.

It is a well-known fact that the molecular disturbance produced in a mass of iron by the magnetizing influence of an intermittent electrical current can be observed as sound by placing the ear in close contact with the iron, and it occurred to us that the molecular disturbance produced in crystalline selenium by the action of an intermittent beam of light should be audible in a similar manner without the aid of a telephone or battery.

Many experiments were made to verify this theory, but at first without definite results.

The anomalous behavior of the hard rubber screen alluded to above suggested the thought of listening to it also.

FIG. 10.



This experiment was tried with extraordinary success. I held the sheet in close contact with my ear while a beam of intermittent light was focussed upon it by means of a lens. A distinct musical note was immediately heard. We found the effect intensified by arranging the sheet of hard rubber as a diaphragm, and listening through a hearing tube, as shown in fig. 10.

We then tried crystalline selenium in the form of a thin disk, and obtained a similar but less intense effect.

The other substances, which I enumerated at the commencement of my address, were now successively tried in the form of thin disks, and sounds were obtained from all but carbon and thin glass.[22]

In our experiments, one interesting and suggestive feature was the different intensities of the sounds produced from different

[22] We have since obtained perfectly distinct tones from carbon and thin glass.

substances under similar conditions. We found hard rubber to produce a louder sound than any other substance we tried, excepting antimony, and zinc; and paper and mica to produce the weakest sounds.

On the whole, we feel warranted in announcing as our conclusion that *sounds can be produced by the action of a variable light from substances of all kinds when in the form of thin diaphragms.* The reason why thin diaphragms of the various materials are more effective than masses of the same substances appears to be that the molecular disturbance produced by light is chiefly a surface action, and that the vibration has to be transmitted through the mass of the substance in order to affect the ear.

On this account we have endeavored to lead to the ear, air that is directly in contact with the illuminated surface, by throwing the beam of light upon the interior of a tube; and very promising results have been obtained. Fig. 11 shows the arrangement we have

FIG. 11.

tried. We have heard from interrupted sunlight very perceptible musical tones through tubes of ordinary vulcanized rubber, of brass and of wood. These were all the materials at hand in tubular form, and we have had no opportunity since of extending the observations to other substances.[23]

I am extremely glad that I have the opportunity of making the first publication of these researches before a scientific society, for it is from scientific men that my work of the last six years has received its earliest and kindest recognition. I gratefully remember the encouragement which I received from the late Professor Henry, at a time when the speaking telephone existed only in

[23] A musical tone can be heard by throwing the intermittent beam of light into the ear itself. This experiment was at first unsuccessful on account of the position in which the ear was held.

theory. Indeed, it is greatly due to the stimulus of his appreciation that the telephone became an accomplished fact.

I cannot state too highly also the advantage I derived in preliminary experiments on sound vibrations in this building from Professor Cross, and near here from my valued friend Dr. Clarence J. Blake. When the public were incredulous of the possibility of electrical speech, the American Academy of Arts and Sciences, the Philosophical Society of Washington, and the Essex Institute of Salem, recognized the reality of the results and honored me by their congratulations. The public interest, I think, was first awakened by the judgment of the very eminent scientific men before whom the telephone was exhibited in Philadelphia, and by the address of Sir William Thomson before the British Association for the Advancement of Science. At a later period, when even practical telegraphers considered the telephone as a mere toy, several scientific gentlemen, Professor John Pierce, Professor Eli W. Blake, Dr. Channing, Mr. Clark, and Mr. Jones of Providence, R. I., devoted themselves to a series of experiments for the purpose of assisting me in making the telephone of practical utility; and they communicated to me, from time to time, the results of their experiments with a kindness and generosity I can never forget. It is not only pleasant to remember these things and to speak of them, but it is a duty to repeat them, as they give a practical refutation to the often repeated stories of the blindness of scientific men to unaccredited novelties, and of their jealousy of unknown inventors who dare to enter the charmed circle of science.

I trust that the scientific favor which was so readily accorded to the Telephone may be extended by you to this new claimant,—"*The Photophone.*"

On some of the consequences of the hypothesis, recently proposed, that the intrinsic brilliancy of the Fixed Stars is the same for each star.[1] By Edward S. Holden, of Washington, D. C.

I.

In all statistical researches upon the arrangement of the fixed stars in space, it has been found to be necessary to make some fundamental assumption, more or less probable.

Some assumption is forced upon us by our complete ignorance of the nature of the stars themselves and of the real laws according to which they are distributed.

The fundamental assumptions have usually been, either that the stars are all of equal brightness; or else that they are equally scattered, so that within equal portions of space, equal numbers of stars exist. Various modifications of these two hypotheses have been made and their consequences worked out; but some form of one of them has usually been the starting point.

It may be of interest to see the consequences which follow from an assumption less violent than either of the preceding. This is that the brightness of the unit-area of all stars is the same. The simple formulæ which relate to this subject were put in a form which allows the consequences of each of the three fundamental assumptions to be seen, in Newcomb and Holden's *Astronomy*,[2] page 489. The hypothesis that the brightness of the unit-area of all stars is the same has been recently made the basis of computation in a paper by Prof. E. C. Pickering, "Dimensions of the Fixed Stars, etc., reprinted from the Proceedings of the American Academy, Cambridge, 1880," in which (page 35) it is used to determine the dimensions of a dark satellite to *Algol*. As we can have no *à priori* proof of the truth of this hypothesis, it will perhaps be useful to trace its consequences in various directions. Although the data at our command are not sufficient to enable us to come to certain conclusions, yet they may be sufficient to test the value of the fundamental assumption, and it is only for this reason that I bring them together. It may not be improper to say

[1] Dimensions of the fixed stars, etc., by Prof. E. C. Pickering, Cambridge, 1880, p. 3.
[2] American Science Series, New York, 1879.

that they were deduced in 1877, and would have remained unpublished except for their bearing on the present question.

While the first two assumptions in regard to the distribution of stars have been shown to be roughly and in a general way approximations to the truth[3] we know that they are in fact untrue. For the hypothesis that all stars are of equal brightness, and thus that stellar magnitude depends on stellar distance alone, is contradicted directly by the determinations of parallax, and more glaringly and in a more general manner by the existence of clusters, in which stars of different brightness are associated at the same distance from the earth.

The hypothesis of equable distribution is negatived by the existence of clusters at all, so that while this supposition is also in a general way true, it needs serious modifications to make it fit special cases. The third hypothesis of equal brightness of the unit-area of the surfaces of all stars is certainly not true in every case, but in any case it is less violent than either of the others *à priori*. The objections to it we may consider later. It may be mentioned here, that of the 324,000 stars from first to ninth magnitude, we know considerably less than 1,000 highly colored stars; the vast majority of stars being white. This is in no sense a proof of the assumption. It does not militate against it, however, and is what might be expected if the assumption were indeed true.

We may express our conditions in an algebraic form as follows:

If S be the surface and R the radius of a star at distance D; i the amount of light emitted per unit of surface, B_m, its brightness in arbitrary units as seen from the earth, m, its stellar magnitude on any scale whose light-ratio is δ, then the most general expression for B_m is

$$(1)\quad \begin{cases} B_m = \frac{S \times i}{D^2} \text{ if light is not extinguished in space, or} \\ B_m = \frac{S \times i}{f(D)} \text{ if light is extinguished in space.} \end{cases}$$

The unit of D is arbitrary.

If the brightness of an average first magnitude star is unity, $B_1 = 1$, then

(2) $B_m = \delta^{m-1}$; so that for a star of the m^{th} magnitude

[3] Not only by the results of W. HERSCHEL, but by the later researches of PETERS, GYLDÉN, C. S. PEIRCE, GOULD, and others.

(3) $B_m = \delta^{m-1} = \frac{S \times i}{D^2}$; for a star of the n^{th} magnitude

(4) $B_n = \delta^{n-1} = \frac{S' \times i'}{D'^2}$: also

(5) $\frac{B_m}{B_n} = \delta^{m-n} = \left(\frac{1}{\delta}\right)^{n-m} = \frac{S \times i \times D'^2}{S' \times i' \times D^2}$.

If m and n are known on some scale whose δ is known, or if B_m B_n are measured photometrically, then for each star one of the quantities S, i, D, can be expressed in terms of the other two, and in general this is all that can be done. If we assume throughout $i = i'$ then these equations become

(6) $B' = \frac{4\pi R'^2 \times i}{D'^2}$; (7) $B'' = \frac{4\pi R''^2 \times i}{D''^2}$; and

(8) $\frac{B'}{B''} = \delta^{m-n} = \frac{R'^2 \times D''^2}{R'^2 \times D'^2}$.

There are three special cases which we may examine: I. Stars of known distances D′, D″, etc., or of known parallaxes π', π'', etc. II. Binary stars where $D' = D''$, although both D′ and D″ are unknown in general. III. Clusters, where $D' = D''$, etc., and D′, D″, etc., are unknown.

I.

In the case of stars of known parallaxes, π', π'', etc., the equation (8) becomes

$$\frac{B'}{B''} = \frac{R'^2 \times \pi''^2}{R''^2 \times \pi'^2}.$$

Table A (page 140), contains the result of recent measures of well determined parallaxes (excluding double stars).

We may assume α *Lyræ* as the unit star, so that $B'' = 1$ and $R'' = 1$ and deduce $\frac{R'}{R''}$ for each star; *i. e.*, the diameter of each star relative to the diameter of α *Lyræ*. The table shows the largest diameter to be 271 times the smallest, or not far from the ratio of the Sun's diameter to Mercury's (291 to 1). This difference corresponds to an immense difference of mass, but perhaps not sufficient to show that the fundamental hypothesis $i = i' = i''$, etc., is erroneous. We may go farther and apply the formulæ to double stars and clusters.

II.

In a note in the American Journal of Science for 1880, June (page 467), I gave certain tables of binary stars which I had prepared in 1877 with reference to this subject.

Table I, there given, contained 122 stars certainly binary and with component stars of like color. The magnitudes and colors are from the best authorities. The mean difference of magnitudes (B–A) is 0.53^m.

TABLE A.

STAR'S NAME.	MAG.	π	COLOR.	B' BRIGHTNESS. α Lyræ = 1	R' DIAMETER. α Lyræ = 1 R'' = 1
21185 Lalande	7	0''.50		[0.0041]	0.02
34 Groombridge	8	0 .31		[0.0016]	0.02
21258 Lalande	8.5	0 .26		[0.0010]	0.02
O. A. 17415	9.5	0 .25		[0.0002]	0.01
σ Draconis	5	0 .22		[0.0256]	0.11
α Canis Maj.	1	0 .19		4.285[4]	1.63
α Lyræ	1	0 .15		1.000[4]	1.00
1830 Groombridge	7	0 .15	reddish	[0.0041]	0.06
ι Ursæ Maj.	3	0 .13		0.0476[4]	0.25
α Boötis	1	0 .13		0.7943[4]	1.03
γ Draconis	2	0 .09		0.0817[4]	0.48
α Aurigæ	1	0 .05		0.8185[4]	2.71
α Urs. Min.	2	0 .05		0.1202[4]	1.04
					0.64 mean

Table II contained forty stars certainly binary, the components being of different colors: the mean difference of magnitudes (B–A) is 2.44^m.

These tables showed that *considering every known case of binary stars of known color:* I. *The components of the* 122 *binary stars of the same color differ in magnitude on the average only* 0.5^m.

II. *The components of the* 40 *binary stars of different colors differ in magnitude on the average* 2.4^m."

Our equation (8) is $\delta^{m-n} = \frac{R'^2 \times D''^2}{R''^2 \times D'^2}$.

For any pair of stars, certainly binary, $D' = D''$ and the ratio $\frac{R'^2}{R''^2}$ can be taken out of the following table, which is computed

[4] SEIDEL: Ueber d. gegenseitigen Helligkeiten der Fixsterne (1852). The other numbers in this column are interpolated with $\delta = 0.40$.

with various values of δ (the light-ratio), by entering it with $n - m$, *i. e.*, the difference in magnitude of the component stars.

TABLE B.

GIVING THE VALUES OF $\left(\frac{1}{\delta}\right)^{n-m}$ FOR THE LIGHT-RATIOS OF ARGELANDER, STRUVE AND J. HERSCHEL.[5]

$n-m$	ARGELANDER. $\delta = 0.400$	STRUVE. $\delta = 0.350$	HERSCHEL. $\delta = 0.480$	$n-m$	ARGELANDER. $\delta = 0.400$	STRUVE. $\delta = 0.350$	HERSCHEL. $\delta = 0.480$
0.0	1.000	1.000	1·000	5.0	98.	190.	39.
.1	1.10	1.11	1.08				
.2	1.20	1.23	1.16	5.5	155.	321.	65.
.3	1.32	1.37	1.25				
.4	1.44	1.52	1.34	6.0	245.	526.	109.
.5	1.582	1.695	1.445				
.6	1.73	1.88	1.56	6.5	386.	806.	183.
.7	1.90	2.09	1.68				
.8	2.07	2.33	1.80	7.0	613.	1242.	306.
.9	2.27	2.58	1.94				
1.0	2.500	2.857	2.083	7.5	970.	1908.	512.
.1	2.74	3.17	2.25				
.2	3.00	3.52	2.40	8.0	1531.	2933.	862.
.3	3.29	3.91	2.50				
.4	3.61	4.35	2.79	8.5	2410.	4525.	1439.
.5	3.953	4.831	3.003				
.6	4.33	5.38	3.24	9.0	3831.	6897.	2415.
.7	4.74	5.99	3.48				
.8	5.21	6.67	3.75	9.5	6024.	10582.	4065.
.9	5.71	7.41	4.03				
2.0	6.250	8.130	4.348	10.0	9615.	16234.	6757.
.1	6.85	9.01	4.67				
.2	7.52	10.00	5.03	10.5	15314.	24938.	11377.
.3	8.26	11.11	5.41				
.4	9.09	12.35	5.81	11.0	24038.	38461.	18975.
.5	10.000	13.717	6.250				
.6	10.75	15.24	6.71	11.5	38314.	59172.	31746.
.7	11.79	16.95	7.25				
.8	12.94	18.83	7.81	12.0	60241.	91743.	53191.
.9	14.16	20.92	8.40				
3.0	15.625	23.310	9.091	12.5	94340.	140845.	89286.
.1	17.1	25.8	9.7				
.2	18.7	28.6	10.5	13.0	150376.	216450.	149701.
.3	20.5	31.7	11.3				
.4	22.4	35.2	12.1	13.5	236967.	332226.	250000.
.5	24.57	39.06	13.06				
.6	27.0	43.5	14.0	14.0	375940.	510204.	418410.
.7	29.6	48.3	15.1				
.8	32.5	53.8	16.3				
.9	35.6	59.9	17.5				
4.0	39.06	66.67	18.80				
.1	42.9	74.1	20.2				
.2	46.7	82.0	21.8				
.3	51.3	90.9	23.5				
.4	56.2	101.0	25.3				
.5	61.73	112.23	27.17				
.6	67.6	124.7	29.2				
.7	74.1	138.5	31.5				
.8	81.3	153.9	33.9				
.9	89.3	171.2	36.5				
5.0	97.66	190.48	39.06				

[5] This table is the converse of that given by JOHNSON in the Radcliffe Observations, Vol. XII, p. 31, Appendix, which is computed with three significant figures.

If the underlying hypothesis $i = i'$, etc., is true, it would follow that of the stars of Table I the larger is on the average 1.6 times the surface of the smaller, while Table II would show that on the average the surface of the larger star of each pair was 9.1 times that of the smaller for pairs of different colors.

It may be said in passing that the colors of the small stars in Table II are not due to their proximity to the large stars, but are intrinsic. The doubt in the conclusion as to relative size is a doubt whether the fundamental hypothesis of equal intrinsic brilliancy is true.

Still keeping to this assumption we can go farther, as Prof. PICKERING has shown (*op. cit.*, pp. 6, 7), for we can find the mass of each component of a binary star of known parallax. Thus if M and M′ be these masses, a the mean distance of M and M′ and T the periodic time in years

$$M + M' = \frac{a^3}{T^2}$$

D and p being the distance and parallax of the binary $D = 1 \div \sin p$.

If a'' is its semi-major axis in seconds

$a = D \times \sin a''$ and $a = \frac{\sin a''}{\sin p} = \frac{a''}{p}$.

Thus $M + M' = \frac{a''^3}{p^3 T^2}$, the unit of M and M′ being the sun's mass.

Knowing p, a'' and T for any binary, we may deduce the dimensions of each component. Thus for α *Centauri*, $p = 0''.9$ (GYLDÉN), $a'' = 15''.5$, $T = 80.0$ years (RUSSELL in Monthly Notices, R. A. S., 1877), whence $M + M' = 0.80$ of the sun's mass.

Sir JOHN HERSCHEL in the Cape of Good Hope observations (p. 300), says the components of α *Centauri* are of the same color and differ 0.73 magnitudes ($A = 1.00$, $B = 1.73$), δ for HERSCHEL is 0.480 whence $\frac{R'^2}{R''^2} = (2.083)^{0.73} = 1.71$,

also $\frac{R'}{R''} = 1.31$ and $\frac{R'^3}{R''^3} = \frac{M}{M'} = 2.23$.

Hence $M = 0.55$ of the sun's mass.
$M' = 0.25$ " " " "
$M + M' = 0.80$ " " " "

These results differ from those given by Prof. PICKERING (page 6), who makes $\frac{M}{M'} = 63.0$ on account of a different assumption as to the relative magnitude of the two components.

The recent research of Dr. ELKIN, Ueber die parallaxe von α Centauri leads to the conclusion (p. 25), that the masses M and M′ are equal.

III.

Let us apply this hypothesis to the case of a cluster of stars. The cluster contains say N_1 stars all at a distance D_1 from us. Suppose the cluster to be 10′ in diameter. Beside the N_1 stars which really make up the cluster, others are seen in the same field of view: that is the cluster, as we see it, consists of two classes of stars. *First*, the N_1 stars really associated with each other in a physical connection and at the same distance D_1 from the sun, and *second*, those scattered on the background of the heavens which (accidentally) are seen among the component stars of the cluster.

The last may be practically eliminated from the cluster by examining many fields of view 10′ in extent situated about the cluster in question. The average number and magnitude of the stars in such 10′ fields can be determined and subtracted from the stars of the real cluster. Then there remain N_1 stars of a magnitude from a to b for example. At the unknown distance D_1 of this particular cluster we learn that stars of the apparent magnitudes from a to b are intermixed. From other clusters we may also determine like data.

But all the stars of the real cluster are at (essentially) the same distance D_1 and assuming them to have the same power to radiate light per unit of surface the range of magnitudes $(a \ldots\ldots b)$ will afford us a means of determining the probable range of size $R_1 \ldots\ldots R_2$ of the component stars.

For a star of the brightest magnitude a, gives out an amount of light expressed by δ^{a-1}, and similarly the faintest star of the cluster gives out the light δ^{b-1}. As these stars are at the same distance their relative brightness is proportional to $\delta^{a-b} = \frac{R'^2 \times i'}{R''^2 \times i''}$. Thus for this particular cluster the assumption $i' = i''$ gives us the relation $R' = R'' \times \delta^{\frac{a-b}{2}}$. Other clusters will give us $R''' = R^{iv} \times \delta^{\frac{c-d}{2}}$ etc., and by collecting all the cases where

clusters have been examined with respect to the magnitudes of the contained stars, a mean value of the ratio $\frac{R_1}{R_2}$ can be had. Of course the best value is only to be had from those clusters which have been most carefully studied, but these are now too few for our purpose. I have collected, in Table C (pp. 146–151), the results from the principal monographs and from the clusters of Sir JOHN HERSCHEL's southern sweeps,—in all 167 cases. The mean value of $\frac{R_1}{R_2}$ is 4.1. That is, in these 167 clusters, supposing the stars in each cluster to be equally bright per unit-area, the diameter of the largest star is on the average 4.1 times the diameter of the smallest star of each cluster. This depends upon the fact, which is well known, that clusters usually are composed of stars of about the same magnitudes.

Under the fundamental assumption the mean difference of magnitudes is about 3.8^m on HERSCHEL's scale.

SUMMARY.

The fundamental assumption that the brightness of the unit-area of all stars is the same has now been applied to the cases of

1st, stars of known distance,

2nd, binary stars,

3rd, clusters.

The results reached are

1st, that, admitting this assumption, some of the isolated stars are as much as 271 times greater in diameter than others,

2nd, that the binary stars of like color are composed of two equal stars, while those of different color are composed of two stars one about three times the diameter of the other on the average.

3rd, that, on the average, the brightest star of any cluster is about 4.1 times as great in diameter as the smallest of the same cluster.

There is nothing impossible, or it may even be said highly improbable, in these conclusions. They stand or fall with the hypothesis on which they are founded.

We can push our conclusions a little farther.

If we have two stars A and B whose brightness is B′ and B″ we have seen that

$$\frac{B'}{B''} = \frac{R'^2 \times D''^2}{R''^2 \times D'^2} \text{ or}$$

$$\frac{D''}{D'} = \frac{R''}{R'} \sqrt{\frac{B'}{B''}}.$$

$\frac{B'}{B''}$ is necessarily finite and for our present telescopes its greatest value is about 400,000. $\frac{R''}{R'}$ we have seen for isolated stars has a maximum value (Table A) 271, whence $\frac{D''}{D'} = 271 \times 633 = 171{,}543$.

From this it would appear that on the fundamental assumption and using the best data now available, it is not likely that any visible star is more than 200,000 times farther from us than the nearest fixed star.

There are strong reasons to believe, as I have elsewhere noted, that an indefinite increase of the aperture of our telescopes would not bring to view an indefinite number of stars of the fainter magnitudes, or that $\frac{B'}{B''}$ has a limiting value not far perhaps from 500,000. The co-efficient 271 (Table A) will be changed with further determinations of parallax but must always remain finite.

Thus it would follow that our sidereal system is comparatively narrowly limited in its boundaries, since $\frac{D''}{D'}$ is always finite and small.

The above conclusions follow when once the fundamental hypothesis is admitted as a basis for computation. I have thought that they should be presented in connection with that hypothesis to aid in forming an adequate idea of its justness and probability.

U. S. Naval Observatory, 1880, August.

TABLE C.

CLUSTER.	NO. OF STARS.	MAGNITUDES BETWEEN *a*.....*b*.	REMARKS.	APERTURE OF TELESCOPE.	$\frac{E_1}{E_2\sqrt{\beta-1}}$	OBSERVER.	MEMOIR.
G. C. 1712	37	[8–11]	57 stars large enough to measure; 8m–9m (7); 9–10 (20) 10–11 (10) many fainter.	12 in.		KOCH	monograph.
1712	150–200	[10–13]	8m–13m adopted.	12 in.	10	VOGEL	Leipzig Obs. Vol. I (1876).
5h 58m ; +12·9°	30–40	9–11		12	2	"	"
1361	20	[9–10]	7m (1) 8m–9m (2) 12m (1) 7m–12m adopted.	12	10	"	"
6h 3m ; +12.7°	30	9–10		12	2	"	"
1383	many	10–11		12	2	"	"
1408	40–50	10–11		12	2	"	"
1435	30	9.5–12		12	3	"	"
1474	many	10–11		12	2	"	"
4440	65	9–13		12	6	"	"
4519	30	11		12	1	"	"
4670	many	11–12		12	2	"	"
584	117	7.2–10.7		3.25	5	PIHL	monograph.
20 Vulpec.	104	[6.0–13.2]	6m (1) and then none till 8.8m from which the mags. run regularly to 13·2.	9	7	SCHULTZ	"
		8.8–13.2					
h. Persei.	43	6.5–9.6	many stars omitted.	6	4	KRUEGER	"
4366	21	6–12		12	16	VOGEL	(1867).
18h 9m ; –18·8°	?	8–10.5	Atti del Accad. N. L.	9.6	3	SECCHI	(1853).
—	?	9–12	" " " "	"	4	"	"
18h 10m ; –18·9°	?	9–12	" " " "	"	4	"	"
4366	?	8–11	" " " "	"	4	"	"
512 ?	?	9–12	" " " "	"	4	"	"
1360	?	7–11	" " " "	"	6	"	"
4437	?	[10–11]	" " " "	"	3	"	"
4437	?	9.0–11.4	Hamburg Obs., Vol. I.	10		HELMERT	(1874)
4410	71	8.2–11.1	Mannheim Obs., Abt. III.	6	4	VALENTINER	(1879)

G. C. 1166	36	8.8–10.7	Mannheim Obs., Abt. III.	6	2	VALENTINER.	(1879)
31	?	12–15	Nebulosity intermixed.	11	4	D'ARREST.	Sid. Neb.
h. 3435	110	7–16		18½ reflector.	49	HERSCHEL.	C. G. H.
h. 3504	innumerable	?	The total light is not greater than a star 5.4m.	"		"	"
h. 2322	very many	12–14	nearly all equal.	"	2	"	"
h. 2375		13–15		"	2	"	"
h. 2376		9–15	a few 10m; one 9m; 10–15 adopted.	"	6	"	"
h. 2517		14–15	stars equal.	"	1	"	"
h. 2715		11–18		"	18	"	"
h. 2811		10–12		"	2	"	"
h. 2824		7–12		"	6	"	"
h. 2893		12–14		"	2	"	"
h. 2901		11–16		"	6	"	"
h. 2931		10–15		"	6	"	"
h. 2975		16	stars equal.	"	1	"	"
h. 3065		13–16	only 1 star 13m.	"	3	"	"
h. 3073		10–14		"	4	"	"
h. 3074	30	10–11		"	1	"	"
h. 3076		9–12		"	3	"	"
h. 3089		11–13		"	2	"	"
h. 3094	150	[12–14]	8m (1); 9m (1); 8′ in diameter. 9–14 adopted.	"	6	"	"
h. 3102		12	stars equal.	"	1	"	"
h. 3103		12–14	none 11m–few 14m.	"	2	"	"
h. 3106		10–12		"	2	"	"
h. 3107		11–13		"	2	"	"
h. 3111		[8–16] [7–12] [9–13]	many of the larger magnitudes. 7m–16m adopted.	"	40	"	"
h. 3114		11–13		"	2	"	"
h. 3120	60	[12–13] 12–15	few very minute.	"	3	"	"
h. 3123	—	12	stars nearly equal.	"	1	"	"
h. 3125		15		"	1	"	"
h. 3132		12–13	stars nearly equal.	"	1	"	"
h. 3137		11–14		"	3	"	"
h. 3152	innumerable.	15–16	a few 15m.	"	1	"	"
h. 3154		12–14		"	2	"	"
h. 3171		11–15	one star 10m.	"	4	"	"
h. 3191	50	12–15		"	3	"	"
h. 3194	50–60	11–12		"	1	"	"

TABLE C. (Continued).

CLUSTER.	NO. OF STARS.	MAGNITUDES BETWEEN *a*.....*b*.	REMARKS.	APERTURE OF TELESCOPE.	$\frac{R_1}{R_2}\sqrt{\delta-\lambda}$	OBSERVER.	MEMOIR.
h. 3219		[13–16] } [12–13] }	[the two collections appear to be separate].	18½ reflector.	3 1	HERSCHEL.	C. G. H.
h. 3224	many hundreds	8–13		"	6	"	"
h. 3238		13–16 } [13–15] }		"	3	"	"
h. 3310		13		"	1	"	"
h. 3315	200+	8, 9, 10, 11	chiefly 10m.	"	4	"	"
h. 3334		15–18	one red star 10m.	"	3	"	"
h. 3342	60–70	11–13 } 10–14 }	10–13 adopted.	"	3	"	"
h. 3352	150–200	8–12		"	4	"	"
h. 3364	50–60	9–12 } [13–14] }	a few 13–14. stars 9, 10, 11; 9–12 adopted.	"	3	"	"
h. 3371		16		"		"	"
h. 3377		10–13 } [10–14] } [10–12] }		"	3	"	"
h. 3386		13–15	1 star 12m; 12m–15m adopted.	"	3	"	"
h. 3388		12–14		"		"	"
h. 3389		[12–14] [12–13]	1 star 8.9; 8.5m–13m adopted.	"	5	"	"
h. 3390		12–16		"	4	"	"
h. 3392		11–12		"	1	"	"
h. 3404		12		"	1	"	"
h. 3407	30–40	11–13		"	2	"	"
h. 3448		[13–18] } [10–15] }	2 stars 8.9 and 9.10 mag. 10–18 adopted.	"	29	"	"
h. 3444		12–16		"	4	"	"
h. 3449	80–100	11–15 } [10, 11, 12] }	some smaller.	"	4	"	"
h. 3476		11		"	1	"	"

h. 3496		12–16		18½ reflector.	4	HERSCHEL.	C. G. H.
h. 3512		13–16		"	3	"	"
h. 3531		10–13		"	2	"	"
h. 3532		7.8–16	*i. e.*, 7.5 to 16.	"	38	"	"
h. 3533		12–15	one star 12m.	"	3	"	"
h. 3534		14		"	1	"	"
h. 3545		11–14		"	3	"	"
h. 3549?				"		"	"
h. 3566		10		"	1	"	"
h. 3570	100	8–14	(3) 10m; (5 or 6) 11m; the rest below 11m.	"	6	"	"
h. 3573	50	7–12		"	6	"	"
h. 3577		11–13		"	2	"	"
h. 3580		10–11		"	1	"	"
h. 3588		9–12		"	3	"	"
h. 3589		13–14		"	1	"	"
h. 3596		12–16		"	4	"	"
h. 3603		11–14		"	3	"	"
h. 3604		15–17	[but it is likely the stars are equal.]	"	1	"	"
h. 3611		13–15		"	2	"	"
h. 3612		12–14		"	2	"	"
h. 3615		11–15		"	3	"	"
h. 3616		7–12?		"	6	"	"
h. 3618		11–14		"	3	"	"
h. 3619		12–14		"	2	"	"
h. 3622		7–10		"	3	"	"
h. 3623		[15–18] / [13–16]	14–17 adopted.	"	3	"	"
h. 3624		15 or 14	equal?	"	1	"	"
h. 3626		8–11		"	3	"	"
h. 3627		13–15		"	2	"	"
h. 3641		12–14		"	2	"	"
h. 3642		10–11		"	1	"	"
h. 3644		11–12		"	1	"	"
h. 3644		8–9		"	1	"	"
h. 3646		[9–12] / [11–14]	10–13 adopted: see text.	"	3	"	"
h. 3648		12–15		"	3	"	"
h. 3650		12–13		"	1	"	"
h. 3652		5–13		"	29	"	"
h. 3654		[9–11] / [9–12] / [10–12]	9–12 adopted.	"	3	"	"

TABLE C. (Continued).

CLUSTER.	NO. OF STARS.	MAGNITUDES BETWEEN *a*.....*b*.	REMARKS.	APERTURE OF TELESCOPE.	$\frac{R_1}{R_2\sqrt{\frac{a-b}{}}}$	OBSERVER.	MEMOIR.
h. 3655		[9-12] } [9-15] }	9-14 adopted.	18½ reflector.	6	HERSCHEL.	C. G. H.
h. 3656		8-12		"	4	"	"
h. 3657		13		"	1	"	"
h. 3658		20		"	1	"	"
h. 3659		11-15		"	4	"	"
h. 3660		11-12	nearly equal.	"	1	"	"
h. 3661		15	very equal.	"	1	"	"
h. 3662		10-12		"	2	"	"
h. 3663		14-16		"	2	"	"
h. 3664		9-11		"	2	"	"
h. 3665		16		"	1	"	"
h. 3666		16-18		"	2	"	"
h. 3667		15-16		"	1	"	"
h. 3670		16-17		"	1	"	"
h. 3671		16-17		"	1	"	"
h. 3672		12-14		"	2	"	"
h. 3677		14		"	1	"	"
h. 3683		equal?		"	1	"	"
h. 3684		[12-16] } [14-17] }	13-17 adopted.	"	4	"	"
h. 3689		6.5-13		"	14	"	"
h. 3690		17-20		"	3	"	"
h. 3698		all equal		"	1	"	"
h. 3699		7-10		"	3	"	"
h. 3702		8-12		"	4	"	"
h. 3703	20-30	10-12		"	2	"	"
h. 3705		equal?		"	1	"	"
h. 3706		12-13		"	1	"	"
h. 3707		12-15		"	3	"	"
h. 3710		7.5-12		"	5	"	"
h. 3720		equal?		"	1	"	"

h. 3721		9–13		18½ reflector.	4	HERSCHEL.	C. G. H.
h. 3723		equal		"	1	"	"
h. 3726		15–16		"	1	"	"
h. 3730		14–20		"	10	"	"
h. 3731		16		"	1	"	"
h. 3736		15		"	1	"	"
h. 3737		16	equal.	"	1	"	"
h. 3739		13		"	1	"	"
h. 3742		equal		"	1	"	"
h. 3743		14–16		"	2	"	"
h. 3746		12–15		"	3	"	"
h. 3747		14–15		"	1	"	"
h. 3749		15–16		"	1	"	"
h. 3751		9–13		"	4	"	"
h. 3753		[11 and 15]	stars of these mags. only.	"		"	"
h. 3756		15–17		"	2	"	"
h. 3758		9–15		"	10	"	"
h. 3763		14–15		"	1	"	"
h. 3770		14–16		"	2	"	"
h. 3778		11–16	stars of two sizes only.	"		"	"
h. 3798		[13–16] } [11–13] }	12–15 adopted.	"	3	"	"
h. 3978		12–16		"	4	"	"

167 cases; mean value $\frac{R_1}{R_2}=4.1$

PROBLEMS IN WATSON'S CO-ORDINATES. By THOMAS HILL, of Portland, Me.

[ABSTRACT.]

THE equation $p = A\ (a - \sin m\nu)^n$, in which, as in Vol. XXII, pp. 27–30, and Vol. XXIV, pp. 41–47, p is the length of a perpendicular from the origin on the tangent, and ν is the angle it makes with the axis, was discussed for the case in which $n = -1$. The curves represented by it are peculiar; and touch the conic sections in the parabola and circle, but not in any other forms; a new illustration of the fact that the capability of assuming certain forms in common is not proof of community of origin.

ON TWO NEW INSTRUMENTS FOR THE DETERMINATION OF TIME AND LATITUDE. By S. C. CHANDLER, jr., of Boston, Mass.

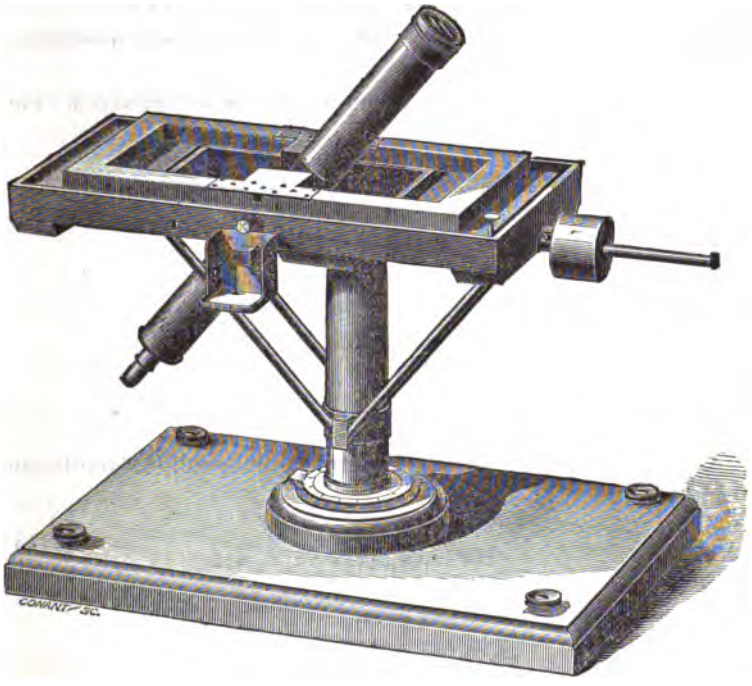
I DESIRE to call the attention of practical astronomers to two new instruments for the determination of time, one of them also for latitude, the principles of construction of which I believe to be novel. They seem to possess advantages which entitle them to consideration.

The first of the instruments I propose to call the *Almacantar*, from an Arabic term, now obsolete, but appropriate for this purpose. From the results attained with an experimental instrument, I feel justified in claiming that in accuracy, efficiency, and convenience, this construction, for instruments of moderate size, is superior to the transit instrument, while it is very much cheaper. The principle involved is the same as that of Kater's floating collimator for determining the zenith point of a graduated circle. Beyond this, however, there is no further resemblance.

Briefly described, the instrument consists of a heavy base, with approximate levelling screws at the corners. From the centre arises an upright cylindrical pillar surmounted by a cap of hard brass, and surrounded at the middle and the base of the pillar by

brass collars. These serve as bearings for a hollow brass sleeve, closely fitting and turning smoothly on the pillar. This sleeve is provided with a cross-head and lateral diagonal braces which support a shallow trough, in the form of a long, hollow rectangle. In this trough is contained the mercury, to the depth of about an

FIG. 1.



eighth of an inch, upon which swims a float of wood or iron, also a hollow rectangle, a little smaller than the trough. By means of two pins projecting from the sides of the trough and playing in vertical slots in the sides of the float, the latter is kept in place, while it is free to seek its equilibrium. From the middle of the inside edges of the float project two bent arms of brass, the lower ends of which support the horizontal axis of the telescope. The axis is provided at one end with a clamp, and at the other with an illuminating contrivance; and the telescope has a reticule of five horizontal spider-lines. (FIG. 1.)

If the telescope is turned on its axis and clamped at any desired altitude, and the whole instrument revolved around the upright axis, the sight line will describe a small circle parallel to the horizon. A few seconds only are required for the mercury and float to come to perfect rest. It is evident that the transit of stars, as they rise or fall over this horizontal circle, may be observed, and will furnish the means of finding the clock error, and the latitude, by a proper selection of the stars in different azimuths.

The mathematical theory of the instrument is succinctly indicated as follows:—

Let Φ = latitude of place of observation.
ζ = zenith distance of star.
t = hour angle " "
A = azimuth " "
q = parallactic angle " "
α = right ascension " "
δ = declination " "
θ = sidereal time

From the spherical triangle between the pole, the zenith, and the star

$$\left.\begin{aligned} &\text{Cos } t = \frac{\cos\zeta - \sin\phi \sin\delta}{\cos\phi \cos\delta} \\ &\text{We have also } \theta = \alpha + t. \end{aligned}\right\} \quad (1)$$

The differentiation of (1) gives

$$d\theta = d\alpha + dt$$

$$\left. dt = \cos q \frac{1}{\cos\phi \sin A} d\delta + \frac{1}{\cos\phi \sin A} d\zeta - \cos A \frac{1}{\cos\phi \sin A} d\varphi \right\} \quad (2)$$

where q and A are found by

$$\text{Sin A} = \frac{\cos\delta \sin t}{\sin\zeta} \qquad \sin q = \frac{\cos\phi \sin A}{\cos\delta} = \frac{\cos\phi \sin t}{\sin\zeta} \qquad (3)$$

Let ζ_o, φ_o, α_o, and δ_o, be assumed approximate values, which employed in eq. (1) give θ_o, and t_o.

Also let α', δ', be the apparent place of the star,

ζ = the *actual* zenith distance at which the instrument is pointed,

r = the refraction.

$$\left.\begin{array}{ll} \text{Put } d = \delta' - \delta_0 \text{ (arc)} & \mathrm{D} = \frac{1}{15}\cos q \frac{1}{\cos\phi \sin \mathrm{A}} \\ l = \varphi - \varphi_0 \text{ "} & \mathrm{L} = -\frac{1}{15}\cos \mathrm{A} \frac{1}{\cos\phi \sin \mathrm{A}} \\ z = \zeta - \zeta_0 \text{ (time)} & \mathrm{Z} = \frac{1}{\cos\phi \sin \mathrm{A}} \\ a = \alpha' - \alpha_0 \text{ "} & \end{array}\right\} \quad (4)$$

Then we have the final equation for the clock correction ΔT, T being the observed clock time of the star's passing the middle thread,

$$\Delta \mathrm{T} = \mathrm{Z}\,(z + r) + \mathrm{L}l + \theta_0 - \mathrm{T} + a + \mathrm{D}d \qquad (5)$$

where $(z + r)$, treated as a single unknown quantity, is the deviation of the collimation line of the telescope from the assumed horizontal small circle. The term L Z disappears if the correct latitude has been used in the computation of t_0 and θ_0. The rest of the second member contains only known quantities.

For reducing the observed time T′ of the star's passage over a side thread, whose interval (in time) from the middle or mean thread is f, we have

$$\mathrm{T} = \mathrm{T}' + \mathrm{Z}f - \mathrm{Z}^2 \frac{\cos \mathrm{A} \cos q}{\sin t} \frac{15}{\pi} \sin 1'' f^2 \qquad (6)$$

where the last term is insensible for observations made near the prime vertical, and is in general small except near the meridian.

For the special case where $\zeta = 90 - \varphi$, that is, for the small horizontal circle passing through the pole, the above formulas are very much simplified, as follows:

$$\left.\begin{array}{ll} \mathrm{Cos}\ t = \tan\varphi \tan (45° - \tfrac{1}{2}\delta) & \\ \mathrm{Sin\ A} = \frac{\cos\delta \sin t}{\cos\phi} & q = t \\ \mathrm{D} = \frac{1}{15} \frac{1}{\cos\delta \tan t} & \\ \mathrm{L} = -\frac{1}{15} \frac{\cos \mathrm{A}}{\cos\delta \sin t} & \\ \mathrm{Z} = \frac{1}{\cos\delta \sin t} & \\ \mathrm{Z}^2 \frac{\cos \mathrm{A} \cos q}{\sin t} = \mathrm{Z}^2 \frac{\cos \mathrm{A}}{\tan t} & \end{array}\right\} \quad (6)$$

In the practical use of the instrument at any given station, it will be convenient to compute once for all, for some one appropriate horizontal circle, or value of ζ, the values of θ_0 and also the co-efficients D and Z for all Nautical Almanac stars, with

assumed approximate values of α_0 and δ_0. These values will last for a series of years. I would suggest for these latitudes the small circle passing through the pole, being one which possesses many practical advantages, as the plane in which the instrument should be used. Whatever circle is chosen, the instrument can be quickly and approximately set to the altitude in question by methods which I need not stop to describe. As many stars as are desired may then be observed alternately east and west, preferably near the prime vertical. A single pair of stars will of course suffice to determine the deviation of the instrument from the assumed plane and the clock correction, or $(z+r)$ and Δ T. A set of several stars can be treated by the method of least squares. If the latitude is only approximately known, the pairs of stars can be selected so as to eliminate the unknown error in latitude. Also stars can be chosen which have large latitude co-efficients, and the correction to assumed latitude found with great accuracy. I will only mention that by the application of a weight which, by a screw, may be moved longitudinally along the float, a micrometer of great delicacy would be furnished, operating on the principle of gradual and minute alterations of the centre of gravity, and thus of the altitude of the sight line. This would permit of the employment of the methods used with the zenith telescope for latitude.

The advantages which are claimed for this instrument, over the transit instrument, are principally:

1st. Simplicity in use, there being only one adjustment to make, and only one instrumental error to determine. There are no levellings or reversals to make, operations which take fully one-half the time and labor of using a transit. The observation of slow moving polar stars may be dispensed with, which is another economy of time.

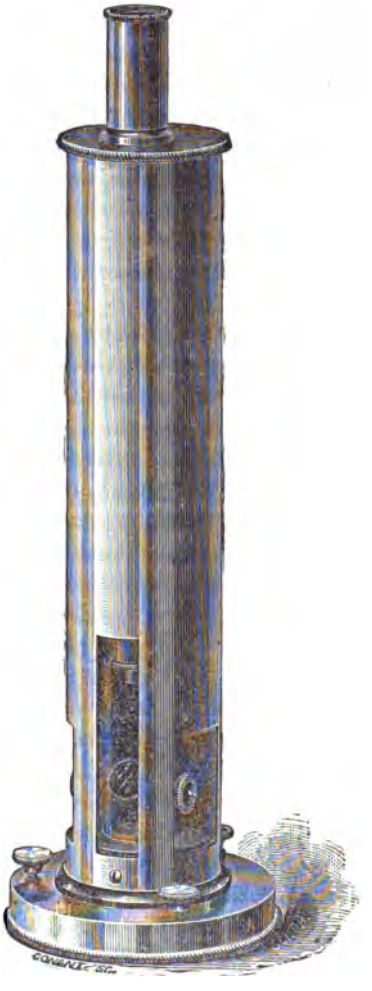
2d. The greatly lessened danger of disturbing the adjustment of the instrument by blows or jars, or want of steadiness in the mounting. A jar, which would infallibly throw a transit out of level or azimuth, produces in this instrument only a slight oscillation, which ceases in a few seconds, the instrument returning to absolutely the same plane as before disturbance.

3d. Greatly diminished cost; there being no parts requiring great nicety of construction, such as the pivots of the transit.

4th. Combination of a time and latitude instrument in one.

As to accuracy, I find that, even with this experimental instrument, in many respects provisional in its construction, with an object glass of very poor definition (of $1\frac{3}{4}$ inches aperture and 25 inches focus), employed in the open air with no protection whatever from the wind, and placed merely upon an ordinary table, or box of sand, I obtain results of at least equal precision with those given by transit instruments of about the same size, under the best circumstances. The probable error of the clock correction found by this instrument, from a set of stars such as would ordinarily be taken with a transit, for clock and instrumental corrections, is not over $\pm 0{\cdot}05$ sec. From my practical experience with transits in actual work, I do not hesitate to assert the belief that for instruments of moderate size, this new construction is more serviceable than the meridian transit.

FIG. 2.



The second instrument to be described is also an equal altitude instrument, but is constructed on a totally distinct principle, and is of a very different order of accuracy. It is intended to supply the place of a sextant for land use, and of small, portable transits in the determination of time by surveyors, exploring parties, watchmakers, and all who desire a simple means of finding the time within a second.

It consists of a swinging bar, suspended at the upper end on a pivot, in such a way as to permit the bar to assume freely a

vertical position, without any torsional revolution. To the bar is affixed a small telescope, the object glass near the bottom, the eye-lens at the top of the bar, with a horizontal wire in the common focus. Below the object glass a frame is fixed to the bar, carrying a plane mirror swinging on a horizontal axis, and provided with a clamp for fixing the mirror at any desired inclination. Below this is a metal bob. The whole construction thus forms a pendulum, which is suspended inside a large tube, at the base of which are approximate levelling screws. (FIG. 2.)

It is evident that if the instrument be turned towards the sun, and the mirror revolved until rays pass after reflection directly up the telescope tube, an eye looking into the telescope will see an image of the sun in the field which, as the sun rises or falls in altitude, will appear to cross the horizontal wire, which, in fact, represents a section of a small circle in the heavens, parallel to the horizon.

This brief explanation will indicate clearly the use of this instrument in getting the time by equal altitudes of the sun. For this purpose it possesses the most surprising facility and accuracy. The instrument can be adjusted in a few seconds so as to bring the sun to the proper place in the field. The mirror being clamped, the transit of both limbs of the sun over the horizontal wire may be observed to within a half a second by an ordinary watch. The instrument is then set aside until a corresponding time, afternoon, or next morning, when the transit is again observed. The watch error from noon, or midnight, of course follows by applying to the mean of the observed times, the "*equation of equal altitudes*, and the "*equation of time*," as explained in the text book.

For cheapness and simplicity, this instrument has, of course, very greatly the advantage over the sextant with the artificial horizon, and over the small portable transits used for this purpose by watchmakers and others; while in point of accuracy it is equal or superior to them. By an extended series of tests made on twelve instruments of this construction, I find the probable error of a single determination of time to be 0.8 sec., which could be somewhat reduced by employing three wires instead of one. The degree of accuracy already attained is, however, sufficient for the purposes of those for whose use the instrument is intended.

Tidal Theory of the Forms of Comets. By Geo. W. Coakley,[1] of New York, N. Y.

1. It is proposed in this paper to inquire, whether the forms and transformations of comets may not be explained by the mutual gravitation of their parts, together with a tidal disturbing force, due to the *difference* of the sun's attraction on the several parts of a comet. For this purpose a comet will be taken to be a mass of gaseous matter, large enough to give it a certain controlling power over its figure of equilibrium, especially when far away from the sun. But, on the other hand, the mass will be taken to be so small, that the comet's figure of equilibrium is readily and largely disturbed, as it approaches or recedes from the sun.

2. The success of La Place's Dynamical Theory of the Tides produced in the waters of the Earth, by the disturbing forces of the sun and moon, depends mainly upon the smallness of the ocean depths. Hence it will be found not very suitable to the discussion of those great changes of figure produced throughout the whole dimensions of a comet, which a similar disturbing force of the sun may produce. On this account I have preferred to apply to the case of the comets what has been called the Equilibrium Theory of the Tides.

3. There exist three model treatises on the equilibrium theory of the tides, all included in the Glasgow edition of Newton's Principia, published in 1833. They are by Daniel Bernoulli, MacLaurin, and Euler; the first in French, the other two in Latin. Bernoulli's treatise is too closely restricted to the special case of the Earth's tides by the condition that the changes of figure, produced by the disturbing forces, shall always be small. This condition, with more or less stringency, is employed in all these treatises. But the mode of treating the subject, and of dealing with the disturbing forces, adopted by Euler, seems less restricted to this condition; and therefore more suitable to the case of the comets, than the methods employed in the other two treatises. I have not, therefore, hesitated to make Euler's plan, in a measure at least, my model in treating the disturbing force of the sun, or

[1] Professor of Mathematics and Astronomy, University of the City of New York.

its tidal action in producing and changing a comet's figure of equilibrium.

I have, however, varied somewhat the mode of resolving the sun's disturbing force, and perhaps the whole subject will be found to be treated in a more strictly analytical manner than that adopted by Euler.

I have not, however, been able to carry the analysis to its last degree of completeness, partly on account of the law of a comet's density being unknown; and also from the fact that the equations of equilibrium depend upon the comet's form, while the varying form depends again upon the forces expressed by these equations.

La Place notices this difficulty in the problem of the Earth's figure of equilibrium, and is only able to deal with it successfully by the condition, that the Earth's figure never departs widely from that of a sphere. But in the case of the comets, there is generally a wide departure of their figure of equilibrium from that of a sphere, or from any given figure at a given time, in consequence of their great change of distance from the sun, and hence of a great change of his disturbing force. This fact, of the large and ever-varying tidal disturbing force of the sun on a comet's figure of equilibrium, makes the complete analysis perhaps the most difficult problem of celestial mechanics. I have, therefore, only attempted the solution so far as to give what might be called *qualitative*, in contradistinction to *quantitative* results, except in a few special cases where the formulæ obtained seemed applicable to at least an approximate determination of the *limits* of the masses of certain comets, for which suitable data could be obtained from observation.

4. In the accompanying figure on page 161, let SK represent the transverse axis of a comet's orbit, S the position of the sun's centre; let C be the comet's centre of gravity, at such a distance, $r = SC$, from the sun, in the first instance, that the comet has nearly or quite a spherical figure, the section of which by the plane of the orbit is the circle B_0BB_1. Let the radius of this sphere be $CB = \rho$, and let the distance of a particle of the comet at B from the sun be $BS = r_1$; and let the radius CB make the angle $BCS = \theta$ with the straight line joining the centres of the sun and comet, while BS makes the angle $BSC = \varphi$ with the same line.

If C be taken as the origin of rectangular co-ordinates in the plane of the orbit, CS the positive direction of the axis of x, and

CB_2 that of the axis of y, then CA, and AB, the co-ordinates of B are

$x = \rho \cos \theta$, $y = \rho \sin \theta$, and we also evidently have

$r - x = r_1 \cos \varphi$, $y = r_1 \sin \varphi$

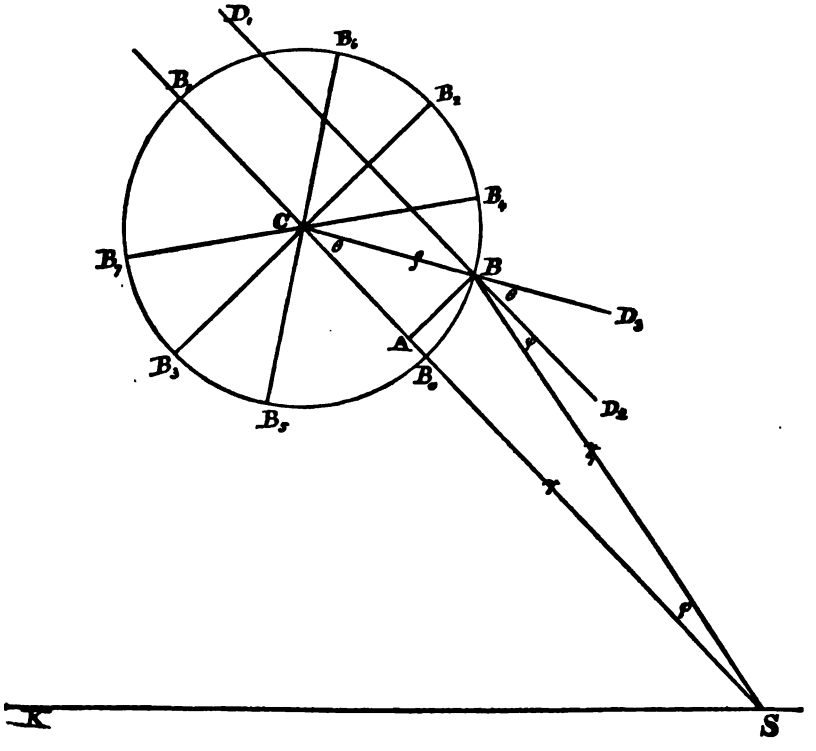
Hence $r_1^2 = (r-x)^2 + y^2 = r^2 + x^2 + y^2 - 2rx = r^2 - 2r\rho \cos \theta + \rho^2$

$\therefore r_1^2 = r^2 \left(1 - 2\frac{\rho}{r} \cos \theta + \frac{\rho^2}{r^2}\right)$. Let $\frac{\rho}{r} = \beta$, then

$r_1 = r (1 - 2\beta \cos \theta + \beta^2)^{\frac{1}{2}}$.

Also $r_1 \sin \varphi = \rho \sin \theta$, $\therefore \sin \varphi = \beta \sin \theta (1 - 2\beta \cos \theta + \beta^2)^{-\frac{1}{2}}$, and

$\cos \varphi = \frac{r-x}{r_1} = \frac{r - \rho \cos \theta}{r(1 - 2\beta \cos \theta + \beta^2)^{\frac{1}{2}}} = (1 - \beta \cos \theta)(1 - 2\beta \cos \theta + \beta^2)^{-\frac{1}{2}}$.



5. Let the unit of mass be that of the sun, and let the comet's mass be denoted by μ. Then the accelerative attraction of the comet on one of its particles B is $f_1 = \frac{\mu}{\rho^2}$. The accelerative attraction of the sun on every particle of the comet, considered as

all condensed into its centre of gravity at C, or the force which produces the motion in the orbit, is $f_2 = \frac{1}{r^2}$. But the sun's acceleration of the particle at B, in the direction of BS, is $f_3 = \frac{1}{r_1^2}$, or $f_3 = \frac{1}{r^2}(1-2\beta\cos\theta+\beta^2)^{-1} = f_2(1-2\beta\cos\theta+\beta^2)^{-1}$.

6. Let us resolve the force f_3, which acts in the direction of BS, into two rectangular components, one of them, f_4, in the direction BD_2, parallel to CS, and the other, f_5, in the direction BA, perpendicular to CS and to BD_2. They are, $f_4 = f_3\cos\varphi$, and $f_5 = f_3\sin\varphi$.

7. If now we suppose a force equal to f_2, or to the force which, acting at the comet's centre of gravity, maintains its orbital motion, to be applied to every particle of the comet, in directions parallel to CS, but opposite thereto, or in directions parallel to CB_1, away from the sun, then the comet may be supposed to be at rest, or to move only by virtue of its inertia.

If then the component, f_4, in the direction of BD_2, were equal to the force f_2 applied in the direction BD_1, the particle of the comet at B would be undisturbed, by this component, in its relation to the centre of gravity at C. But the force f_4, at B, exceeds the force f_2 by f_4-f_2; this excess therefore tends to draw the particle at B away from C in the direction BD_2. Moreover, the component f_5 tends to move the particle at B in the direction BA, or towards the line, CS, joining the centres of the sun and comet.

8. Let us again resolve these two disturbing forces, namely, f_4-f_2, and f_5, each into two new components, in the two directions BD_3, or the prolongation of CB, and a perpendicular to BD_3 at B, on the side towards CS.

The components of

f_4-f_2 are, $f_6=(f_4-f_2)\cos\theta$, in the direction of BD_3,
and $f_7=(f_4-f_2)\sin\theta$, perpendicular to BD_3.

The components of

f_5 are $f_8=f_5\cos(90°+\theta)$, in the direction of BD_3,
and $f_9=f_5\sin(90°+\theta)$, perpendicular to BD_3.

Hence

$f_6=(f_3\cos\varphi-f_2)\cos\theta=f_2\cos\theta\cos\varphi(1-2\beta\cos\theta+\beta^2)^{-1}-f_2\cos\theta$

$f_8=-f_2\sin\theta\sin\varphi(1-2\beta\cos\theta+\beta^2)^{-1}$

$f_7=f_2\sin\theta\cos\varphi(1-2\beta\cos\theta+\beta^2)^{-1}-f_2\sin\theta$

$f_9=f_2\sin\varphi\cos\theta(1-2\beta\cos\theta+\beta^2)^{-1}$.

9. If therefore F_1 is the whole disturbing force in the direction BD_3, and F_2 the whole disturbing force at B perpendicular to BD_3, we have

$$\left.\begin{aligned} F_1 &= f_2 \cos(\theta+\varphi)\,(1-2\beta\cos\theta+\beta^2)^{-1} - f_2\cos\theta \\ F_2 &= f_2 \sin(\theta+\varphi)\,(1-2\beta\cos\theta+\beta^2)^{-1} - f_2\sin\theta \end{aligned}\right\}$$

But we readily find $\cos(\theta+\varphi) = (\cos\theta-\beta)\,(1-2\beta\cos\theta+\beta^2)^{-\frac{1}{2}}$.

and $\sin(\theta+\varphi) = \sin\theta(1-2\beta\cos\theta+\beta^2)^{-\frac{1}{2}}$.

Hence

$$\left.\begin{aligned} F_1 &= f_2(\cos\theta-\beta)\,(1-2\beta\cos\theta+\beta^2)^{-\frac{3}{2}} - f_2\cos\theta \\ F_2 &= f_2\sin\theta\,(1-2\beta\cos\theta+\beta^2)^{-\frac{3}{2}} - f_2\sin\theta \end{aligned}\right\}$$

or, replacing f_2 by its value, $\frac{1}{r^2}$, we have

$$\left.\begin{aligned} F_1 &= \tfrac{1}{r^2}\left\{(\cos\theta-\beta)\,(1-2\beta\cos\theta+\beta^2)^{-\frac{3}{2}} - \cos\theta\right\} \\ F_2 &= \tfrac{1}{r^2}\sin\theta\left\{(1-2\beta\cos\theta+\beta^2)^{-\frac{3}{2}} - 1\right\} \end{aligned}\right\}$$

The particle at B is therefore urged towards the comet's centre of gravity at C by the force $f_1 - F_1$ or $\frac{\mu}{\rho^2} - F_1$; and it is also urged tangentially towards CS by the force F_2.

10. If θ be made to take all values from 0° to 360°, or from 0° to $\pm$ 180°, and the plane of the circle at the same time be made to revolve completely around B_0B_1, the particle at B may have any position whatever on the surface of the comet's spherical figure. Moreover, if ρ be made to vary from 0 to CB, the particle at B may be situated anywhere within the comet's figure.

11. The acceleration towards the comet's centre is

$$f_1 - F_1 = \frac{\mu}{\rho^2} - \frac{1}{r^2}\left\{(\cos\theta-\beta)\,(1-2\beta\cos\theta+\beta^2)^{-\frac{3}{2}} - \cos\theta\right\}$$

Let F'_1 equal the value of this acceleration when applied to the particle of the comet at B_0, *nearest the sun*, which I shall call the comet's *front;* and let F_1'' be its value at B_1, the part *most remote from the sun*, which I shall call the *rear of the comet.* Then, since $\cos\theta = 1$ in the first case, while $\cos\theta = -1$ in the second case, we have

$$\left.\begin{aligned} F_1' &= \frac{\mu}{\rho^2} - \frac{1}{r^2}\left\{(1-\beta)^{-2} - 1\right\} = \frac{\mu}{\rho^2} - \frac{2\beta}{r^2}\cdot\frac{1-\frac{1}{2}\beta}{(1-\beta)^2} \\ F_1'' &= \frac{\mu}{\rho^2} - \frac{1}{r^2}\left\{1-(1+\beta)^{-2}\right\} = \frac{\mu}{\rho^2} - \frac{2\beta}{r^2}\cdot\frac{1+\frac{1}{2}\beta}{(1+\beta)^2} \end{aligned}\right\}$$

$$\left.\begin{aligned} \text{or } F_1' &= \frac{\mu}{\rho^2} - \frac{2\rho}{r^3}\left(1+\frac{3}{2}\frac{\rho}{r}+\frac{4}{2}\frac{\rho^2}{r^2}+\frac{5}{2}\frac{\rho^3}{r^3}+\frac{6}{2}\frac{\rho^4}{r^4}+\frac{7}{2}\frac{\rho^5}{r^5}+\text{etc.}\right) \\ F_1'' &= \frac{\mu}{\rho^2} - \frac{2\rho}{r^3}\cdot\left(1-\frac{3}{2}\frac{\rho}{r}+\frac{4}{2}\frac{\rho^2}{r^2}-\frac{5}{2}\frac{\rho^3}{r^3}+\frac{6}{2}\frac{\rho^4}{r^4}-\frac{7}{2}\frac{\rho^5}{r^5}+\text{etc.}\right) \end{aligned}\right\}$$

12. In both these positions, at B_0 and at B_1, the force F_2 will be zero, because $\sin\theta = 0$. But the gravitation towards the centre at C, both at the *front* and at the *rear* of the comet, will be diminished by the value of F_1 at those points.

13. Let us consider next the particles at B_2 and B_3, at the extremities of a diameter perpendicular to CS. In both these positions $\cos\theta = 0$; at B_2, $\sin\theta = 1$, at B_3, $\sin\theta = -1$. Let F_1''' and F_1^{IV} be the accelerations towards the centre C, arising from the combination of F_1 with f_1 at B_2 and B_3 respectively. Then

$$F_1''' = \frac{\mu}{\rho^2} + \frac{\beta}{r^2}(1+\beta^2)^{-\frac{3}{2}} = \frac{\mu}{\rho^2} + \frac{\rho}{r^3}\left(1+\frac{\rho^2}{r^2}\right)^{-\frac{3}{2}} = F_1^{IV}.$$

At B_2 the tangential component F_2 becomes

$$F_2''' = \frac{1}{r^2}\left\{(1+\beta^2)^{-\frac{3}{2}} - 1\right\} = -\frac{1}{r^2}\cdot\left\{1-(1+\beta^2)^{-\frac{3}{2}}\right\}$$

But at B_3, the tangential disturbing force is

$$F_2^{IV} = +\frac{1}{r^2}\cdot\left\{1-(1+\beta^2)^{-\frac{3}{2}}\right\}$$

Hence it is evident that at B_2 and B_3, the effect of the disturbing force F_1 is to *increase* the comet's attractive force for a particle in the direction towards the centre C.

With regard to the tangential component, F_2, of the sun's disturbing force, it should be observed that it is positive when it produces rotation from B towards B_0, or in the direction in which the hands of a watch move over the dial; and negative when it produces motion in the opposite direction. Hence, as F_2''' is negative at B_2, its effect is to produce motion from B_2 towards B_1. But as F_2^{IV} is positive at B_3, its effect is to produce motion from B_3 towards B_1, this last motion being positive, while the former is negative.

14. Since the gravity of the particles at B_0 and B_1 is diminished by the sun's disturbing force, F_1, and since the gravity is increased at B_2 and B_3, there must be intervening points at which this disturbing force is zero, tending neither to increase nor to decrease the natural gravity of the comet. For these points we must have

$$(\cos\theta - \beta)\,(1 - 2\beta\cos\theta + \beta^2)^{-\frac{3}{2}} - \cos\theta = 0$$

$$\text{or } \left(1-\frac{\beta}{\cos\theta}\right)^2 = (1-2\beta\cos\theta+\beta^2)^3.$$

If we neglect small quantities of the order β^2, β^3, etc., we shall find $\cos^2\theta = \frac{1}{3}$, $\sin^2\theta = \frac{2}{3}$, $\tan^2\theta = 2$, or $\tan\theta = \pm\sqrt{2}$. Hence $\theta = \pm(54°\,44')$ nearly, or $\theta = \pm(125°\,16')$ nearly. If, therefore,

the lines B_4B_7, and B_5B_6 be drawn making with CS these angles of $\pm$ (54° 44′) and their supplements, the comet's attraction on its superficial particles will be diminished by the disturbing force of the sun within the space $B_4B_0B_5$, and also within the space $B_6B_1B_7$; but the natural gravity of the comet will be increased by this disturbing force within the spaces $B_4B_2B_6$ and $B_5B_3B_7$.

15. It follows, therefore, that the sun's disturbing action on the comet's figure of equilibrium, exactly like that of his action in producing the tides in our oceans, is to elongate the comet at first into an ellipsoidal figure with its longest axis pointed towards the sun. The amount of this disturbance of the comet's figure, as compared with the tidal disturbance produced by the sun in our oceans, it is evident, will be inversely as the comet's mass compared with that of the earth, if the comet have the same spherical radius at the earth's distance from the sun. Or rather, this *would be* the ratio, if the comet's materials were thus disturbed only to the depth of five or six miles, like the waters of the earth. But since the tidal disturbance of the comet goes to its very centre, for thousands of miles, it is evident that the effect on its figure of equilibrium is on this account largely increased.

16. It is evident from the expressions for F'_1 and F''_1 in Art. 11, which are the total accelerations towards the comet's centre, at its *front* and *rear* respectively, that the sun's disturbing forces at these points are nearly equal, while the comet is very far from the sun, or while the radius-vector, r, is great. In this case the comet's figure will depart very little from that of a sphere. But on the comet's nearer approach to the sun, it is also evident that, when it becomes necessary to take account of the small terms in the parentheses, $\frac{\rho}{r}$, $\frac{\rho^2}{r^2}$, etc., the tidal action of the sun on the comet's *front* is appreciably more than that on the *rear*. The tendency of this must be to draw *more of the comet's mass* into the *front hemisphere*, and to *leave less of it* in the *rear hemisphere*. In other words, the *centre of gravity* must be *thrown forwards* towards the sun. Thus would be explained the formation of a *nucleus* towards the comet's front.

17. Since the effect of the tangential force, F_2, was to cause a rotation of the particles, both at B_2 and at B_3, backwards, or towards B_1, and we found that this force vanished at B_0 and at B_1, it will be well to determine at what other points it vanishes, and

is therefore likely to change its sign. For this purpose we shall put

$$F_2 = \frac{1}{r}\sin\theta \cdot \left\{ (1 - 2\beta\cos\theta + \beta^2)^{-\frac{3}{2}} - 1 \right\} = 0.$$

Hence, either $\sin\theta = 0$, as previously found, or

$$(1 - 2\beta\cos\theta + \beta^2)^{-\frac{3}{2}} - 1 = 0.$$

From which we have

$$\cos\theta = \tfrac{1}{2}\beta = \tfrac{1}{2}\frac{\rho}{r} \therefore \sin\theta = \pm\left(1 - \tfrac{1}{4}\frac{\rho^2}{r^2}\right)^{\frac{1}{2}} = \pm\left(1 - \frac{1}{8}\frac{\rho^2}{r^2}\right).$$

Hence it is evident that F_2 will be zero, not only at B_0 and B_1, but at two points, one very near B_2, but towards B_0, the other very near B_3, but also towards B_0. From those two points the force F_2 produces rotation towards the front, B_0, in the front hemisphere, and it produces rotation towards B_1 in the rear hemisphere from the same two points.

It is easy to determine, moreover, that the *maximum effect* of the tangential force, F_2, is nearly at the points about 45° from B_0 and B_1, on each side of the diameter joining these points.

18. Soon after the establishment of a nucleus, or centre of gravity near the comet's front, it becomes necessary to change somewhat the expressions for F'_1 and F''_1 in Art. 11. For now the ρ is no longer the same for the front and rear, if we count its value, as we should, from the *centre of gravity*, or *nucleus*. Hence I shall in future denote the ρ for the front, at B_0, by ρ_0, and that for the rear, at B_1 by ρ_1. Hence the two new values of F_1' and F_1'' are, putting them in parentheses for distinction,

$$\left.\begin{aligned}(F'_1) &= \frac{\mu}{\rho_0^2} - \frac{2\rho_0}{r^3}\cdot\left\{1 + \tfrac{3}{2}\cdot\frac{\rho_0}{r} + \tfrac{4}{2}\left(\frac{\rho_0}{r}\right)^2 + \tfrac{5}{2}\left(\frac{\rho_0}{r}\right)^3 + \tfrac{6}{2}\left(\frac{\rho_0}{r}\right)^4 + \tfrac{7}{2}\left(\frac{\rho_0}{r}\right)^5 + \text{etc.}\right\}\\ (F''_1) &= \frac{\mu}{\rho_1^2} - \frac{2\rho_1}{r^3}\cdot\left\{1 - \tfrac{3}{2}\cdot\frac{\rho_1}{r} + \tfrac{4}{2}\left(\frac{\rho_1}{r}\right)^2 - \tfrac{5}{2}\left(\frac{\rho_1}{r}\right)^3 + \tfrac{6}{2}\left(\frac{\rho_1}{r}\right)^4 - \tfrac{7}{2}\left(\frac{\rho_1}{r}\right)^5 + \text{etc.}\right\}\end{aligned}\right\}$$

As ρ_0 is less than ρ, while ρ_1 is greater than the same, it follows that the controlling power of the comet's mass on the front, which is expressed by $\frac{\mu}{\rho_0^2}$, is increased, while the effect of the sun's disturbing power, which is expressed by the remainder of the value of F'_1, its principal term being $\frac{2\rho_0}{r^3}$, is diminished. The comet therefore *gains control* over *its particles at the front*, by the change of the centre of gravity. The exact reverse is the fact with regard to *the particles at the rear*. For in (F_1'') the increase of ρ_1 has diminished $\frac{\mu}{\rho_1^2}$, and it has increased the principal term of the sun's disturbing power, $\frac{2\rho_1}{r^3}$. Hence, if the

comet's mass is small, and ρ_1 becomes large enough, it may happen, not long after the change of the centre of gravity, or the formation of the nucleus, that $\frac{2\rho_1}{r^3}$ may exceed $\frac{\mu}{\rho_1^2}$. In that case, the comet will cease to control its particles at the rear, and they will break away from its figure of equilibrium, or be left behind to revolve about the sun as independent particles. This appears to be the explanation of the formation of a comet's tail or train, the particles of which seem to follow the comet in its approach to perihelion, without forming a closed, well-defined figure of equilibrium.

19. Here let us note an important conclusion.

If we could observe the moment when this rupture of the comet's figure of equilibrium takes plaee, or the first formation of a tail or train, and measure at that moment the value of ρ_1, we should thereby have a good approximation to the comet's *mass*. For just before the rupture we ought to have

$$\frac{\mu}{\rho_1^2} > \frac{2\rho_1}{r^3} \cdot \left\{1 - \frac{3}{2} \cdot \frac{\rho_1}{r} + \frac{4}{2}\left(\frac{\rho_1}{r}\right) - \frac{5}{2}\left(\frac{\rho_1}{r}\right)^3 + \text{etc.}\right\}$$

and at the moment of rupture we ought to have very nearly

$$\frac{\mu}{\rho_1'^2} = \frac{2\rho_1}{r^3} \cdot \left\{1 - \frac{3}{2} \cdot \frac{\rho_1}{r} + \frac{4}{2}\left(\frac{\rho_1}{r}\right)^2 - \frac{5}{2}\left(\frac{\rho_1}{r}\right)^3 + \text{etc.}\right\}$$

Hence just before the rupture we ought to have

$$\mu > 2\left(\frac{\rho'}{r}\right)^3 \cdot \left\{1 - \frac{3}{2} \cdot \frac{\rho'}{r} + \frac{4}{2}\left(\frac{\rho'}{r}\right)^2 - \frac{5}{2}\left(\frac{\rho'}{r}\right)^3 + \text{etc.}\right\}$$

and at the moment of rupture very nearly

$$\mu = 2\left(\frac{\rho_1}{r}\right)^3 \cdot \left\{1 - \frac{3}{2} \cdot \frac{\rho_1}{r} + \text{etc.}\right\}$$

We thus have, in the one case, a close limit, and in the other a near value of the comet's mass. It is therefore of the greatest importance to measure, when possible, the extreme distances from a comet's nucleus to the limits of its well-defined figure of equilibrium, and to obtain that measure as nearly as possible at the time when that equilibrium is broken up.

20. With the rapid decrease of r, as the comet approaches perihelion, having a small mass, limited in amount, if not actually decreasing, by the waste of the detached matter forming the train, it follows that the term $\frac{2\rho_0}{r^3}$ in (F_1') must be rapidly growing towards equality with the term $\frac{\mu}{\rho_0^2}$; and the former term may at last surpass the latter. This effect, taking place gradually, not only at the immediate front, B_0, but at some distance on either

side of it, and in like manner at considerable depths towards the nucleus, would go far towards explaining the formation of the successive envelopes observed by Mr. George P. Bond, and other observers, in Donati's comet.

Before the separation of such an envelope, and at all times while the figure of equilibrium is maintained at the front, the value of ρ_0 should be such that

$$\frac{\mu}{\rho_0^2} > 2\,\frac{\rho_0}{r^3}\cdot\left\{1+\frac{3}{2}\cdot\frac{\rho_0}{r}+\frac{4}{3}\left(\frac{\rho_0}{r}\right)^2+\text{etc.}\right\}$$

and hence $\mu > 2\left(\frac{\rho_0}{r}\right)^3\cdot\left\{1+\frac{3}{2}\cdot\frac{\rho_0}{r}+\frac{4}{3}\left(\frac{\rho_0}{r}\right)^2+\text{etc.}\right\}$

But when the envelope is about to separate, we should have very nearly

$$\mu = 2\left(\frac{\rho_0}{r}\right)^3\cdot\left\{1+\frac{3}{2}\cdot\frac{\rho_0}{r}+\frac{4}{3}\left(\frac{\rho_0}{r}\right)^2+\text{etc.}\right\}$$

21. To apply these formulæ to the determination of the mass of Donati's comet, I take from Mr. George P. Bond's account of this comet, in the Annals of the Observatory of Harvard College for 1858, the following observations of the Normal distances of the Apex of Envelope A from the Nucleus, reduced to the unit of distance from the Earth, at 7^h mean solar time, Observatory of Harvard College.

ENVELOPE A, NORMAL DISTANCE OF APEX FROM NUCLEUS.

Sept. 20, 16.2″
" 21, 19.1″
" 22, 22.2″
" 23, 25.7″
" 24, 29.1″
" 25, 32.7″
" 26, 35.8″
" 27, 38.0″
" 28, 39.7″
" 29, 40.1″
" 30, 40.9″

Mr. Bond also determined the *rate* at which this Envelope A receded from the nucleus to be 2.94″ per day. He also gives a series of measurements from the nucleus to the vertex of the comet's head nearest to the sun, or to what I have called the comet's *front*. These measurements are given in the following form:—

Taking the nucleus as an origin of rectangular co-ordinates, the axis of the comet's figure being that of abscissas, positive towards the tail, negative towards the head or front, Mr. Bond locates any point on the curve of the comet's figure by the co-ordinates (a, b). For the vertex of the head, $b = 0$, and on Sept. 2nd $a = -1.5'$; on Oct. 5th, $a = -1.0'$, $b = 0$, these values being reduced to distance unity. One of these series of measurements, that on Oct 2nd, is printed $a = +1.7'$, which I take to be an evident mistake as to the algebraic sign. Rejecting it, however, and using the other eleven values which I have taken from Mr. Bond's data, I find the average value of $a = -1.164'$, or in seconds of arc, and changing the algebraic sign for my purpose, $a = 69.84''$. But on Sept. 30th, Envelope A was $40.9''$ from the nucleus, according to Mr. Bond. Hence it had $28.94''$ yet to travel in order to reach the vertex of the comet's head, or its *front*. At the rate of $2.94''$ per day, determined by Mr. Bond, Envelope A would therefore reach the point where it would begin to separate from the comet's front in about ten days, or ten days after the perihelion passage.

I have computed the value of the comet's radius-vector at this date, Oct. 10th, from Mr. George Searle's Elements of the Orbit, given in Dr. Gould's Astronomical Journal. These elements are :—

$T = 1858$ Sept. $29^d.75230$, Washington Mean Time.
$\pi = 36° 12' 21.4''$
$\Omega = 165° 18' 46.2''$
$i = 116° 57' 46.1''$
$\varphi = 85° 21' 21.2''$
log. $q = 9.7622362$
log. $a = 2.245944$
Period = 2338 years.

I find, from Mr. Searle's Elements, the logarithm of the comet's radius-vector, ten days after perihelion passage, to be log. $r = \overline{1}.792684$, and from Mr. Bond's value of $a = 69.84''$, I find log. $\rho_0 = \log. \sin a = \overline{4}.529679$.

Hence log. $2\left(\frac{\rho_0}{r}\right)^3 = \overline{10}.512015 = \log. \mu$ nearly.

Hence $\mu = 0.0000000003251$

22. In order to have a better standard with which to compare such small masses as those of the comets, I have computed very carefully the value of the mass of the Earth's atmosphere in

terms of the sun's mass as the unit. Denoting the mass of the Earth's atmosphere by A, in terms of the sun's mass, I find

$$\log. A = \bar{1}\bar{2}.3974508.$$

Hence $\log. \frac{\mu}{A} = 2.114564$, or $\frac{\mu}{A} = 130.186$.

Hence the mass of Donati's comet, the great comet of 1858, was about *one hundred and thirty* times as great as the mass of the Earth's atmosphere.

23. A limit to this mass may also be obtained simply from the measurement of ρ_0 at any time, by means of the approximate formula $\mu > 2\left(\frac{\rho_0}{r}\right)^3$.

Thus on Sept. 30th, very nearly the date of the perihelion passage, Mr. Bond gives the two measurements $a = -1.4'$, $b = 0$, and $a = -0.8'$, $b = 0$, the former made at Liverpool, the latter at Poulkova. The mean of these measurements, changing the algebraic sign, is $1.1'$ or $66''$ for the angular value of ρ_0. Taking Mr. Searle's q for r we have

$$\begin{aligned}
\log. \rho_0 = \log. \sin 66'' &= \bar{4}.5051188 \\
\log. r &= \bar{1}.7622362 \\
&\overline{\bar{4}.7428826} \\
&\qquad 3 \\
&\overline{\overline{\bar{1}\bar{0}.2286478}} \\
\log. 2 &= 0.3010300 \\
&\overline{\overline{\bar{1}\bar{0}.5296778}} \\
\log. A &= \bar{1}\bar{2}.3974508 \\
&\overline{2.1322270}
\end{aligned}$$

Hence $\frac{\mu}{A} > 135.6$

That is, the mass of Donati's comet, by this computation, was more than one hundred and thirty-five times as great as the mass of the Earth's atmosphere. Although this limit is somewhat greater than the former result, yet it is perhaps sufficiently near to give confidence in the general method.

24. I shall next apply the formula for the limit to the first comet of 1770, or the famous comet of Lexell, which passed very close to the planet Jupiter, and for which La Place assigned the limit $\mu < \frac{1}{5000}$ of the Earth's mass. This limit is equivalent to $\frac{\mu}{A} < 225.6$.

I shall employ the elements of Lexell's comet computed by Leverrier, as given in Cooper's "Cometic Orbits." They are as follows: —

Elements of Lexell's comet by Leverrier.

$T = 1770$, Aug. $13^d.54085$, Greenwich Mean Time of Per. Pass.
$\pi = 356° 16' 51''$ Longitude of Perihelion.
$\Omega = 131° 58' 56''$ " " Ascending Node.
$i = 1° 34' 28''$ Inclination to the Ecliptic.
$\log. q = 9.8289491$, log. of perihelion distance.
$e = 0.786119$, Eccentricity.
Motion Direct.

The comet was discovered by Messier, June 14, 1770. "He stated," says Mr. Cooper, "that on the night of his discovery, the comet showed a very feeble nebulosity, occupying little space; the centre was brilliant, but it was difficult to decide whether the object was a nebula or a comet. He could observe no relative change of its place in comparing it with fixed stars during two hours. On the night of 17th—18th June, the diameter of the nucleus $= 0' 22''$; that of the nebulosity $= 5' 23''$." "On the 29th—30th, the former $1' 22''$, the latter $54'$, without any sign of tail."

From the rapid increase of these angles, as the comet approached the Earth, I judge that they have not been reduced to the unit of distance. Hence it is necessary to compute the Earth's distance from the comet at the date of the observation on June 17th—18th, in order to find ρ_0 or ρ_1 or ρ.

From Leverrier's elements of Lexell's comet, I have found for June $17^d.54085$, Gr. M. Time, $\log. r = 0.0731141$, and for D, the comet's distance from the Earth at that date, $\log. D = \bar{1}.2380469$. Hence $\rho = D \sin (2' 41{\cdot}5'')$.

$$
\begin{aligned}
\therefore \log. \rho &= \bar{4}.1317933 \\
\log. r &= 0.0731141 \\
\hline
\log. \tfrac{\rho}{r} &= \bar{4}.0586792 \\
&\quad 3 \\
\hline
&\bar{12}.1760376 \\
\log. 2 &= 0.3010300 \\
\hline
&\bar{12}.4770676 \\
\log. A &= \bar{12}.3974508 \\
\hline
&0.0796168
\end{aligned}
$$

Hence $\frac{\mu}{\Delta} > 1.2012$.

The mass of Lexell's comet is therefore greater than that of the Earth's atmosphere, but appears to be far within the superior limit assigned by La Place.

25. I shall next apply the formula for the limit to Encke's comet.

The comet of 1795, discovered by Miss Caroline Herschel, was subsequently recognized as Encke's comet. It was discovered on Nov. 7, 1795, and the discoverer observed that the diameter on that evening was "about 5′." "It had no nucleus, and had the appearance of an ill-defined haziness, which was rather strongest about the middle." I quote from the Notes of Cooper's "Cometic Orbits." On Nov. 21st, Olbers observed the comet, and found its diameter to be "about 3′." I shall therefore take the mean of these determinations, or 4′, for the diameter on Nov. 14, 1795.

Olbers states that the comet was "round, badly-defined, and without a distinct nucleus."

The Elements of this comet, for 1795, were subsequently computed by Encke, and are given by Cooper, as follows:—

Perihelion Passage 1795, December $21^d.44098$, Gr. M. T.

$\pi = 156° 41' 24''$

$\Omega = 334° 39' 26''$

$i = 13° 42' 30''$

$\log. q = \overline{1}.5243046$

$e = 0.8488828$

Motion direct.

From these elements and the Earth's place in her orbit, I have found for Nov. 14, 1795,

$$\log. 2 \left(\frac{\rho}{r}\right)^3 = \overline{12}.9643042$$
$$\log. A = \overline{12}.3974508$$
$$0.5668534$$

Hence $\frac{\mu}{A} > 3.6886$

Encke's comet has therefore more than three times the mass of the Earth's atmosphere.

26. I shall next apply the formula for the mass to Halley's comet.

Arago says that on January 25, 1836, Sir J. Herschel made two measurements of the head of Halley's comet, as follows:—

Diameter of the head in the direction of right-ascension, 229.4″

" " " " " " " " declination, 237.3″

Two hours after, the measurements were respectively 196.7″ in R. A.
and 252.0″ " Dec.

Whether these measurements were reduced to the unit of distance, or not, is not stated. I shall therefore compute the limit of the mass, first on the supposition that they have not been so reduced, and again by supposing that they have been reduced to the unit.

I shall take the mean of the first two measurements, or 233.4″ for the apparent diameter, which differs only about 4½″ from the mean of the four.

From Cooper's "Cometic Orbits," the elements of Halley's comet are taken, at its perihelion passage in 1835, being the second set of elements computed by H. Westphalen. The inclination, however, has been changed to its supplement, because the motion is retrograde, in order to use *Gauss's* general formulæ.

These elements are as follows:—

Halley's comet, perihelion passage Nov. $15^d.93927$, 1835, Gr. M. T.

$\pi = 304° 31' 34''$
$\Omega = 55° 9' 59''$
$i = 162° 14' 55''$
$\log. q = 9.7683261$
$e = 0.96736929$

From these elements, and the Earth's position at the date, 1836, January $25^d.5$, I have found $\log. 2 \left(\frac{\rho}{r}\right)^3 = \overline{1}1.9331398$

$\log. A = \overline{1}2.3974508$

1.5356890

Hence $\frac{\mu}{A} > 34.331$

If, therefore, Herschel's observations were not reduced to the unit of distance, Halley's comet had a mass more than thirty-four times as great as that of the Earth's atmosphere.

But if, as is probable, Herschel reduced his measurements to the unit of distance, then I find that Halley's comet had a mass somewhat more than forty-five times that of the Earth's atmosphere; or $\frac{\mu}{A} > 45.098$.

Regarding a comet as comparatively a small mass of gaseous matter, which largely changes its distance from the sun, the preceding analysis shows that the sun exercises a great and constantly varying influence over the comet's figure of equilibrium. As long as any completely closed figure of the comet is maintained,

the attraction due to its mass must be superior to the sun's disturbing power. But at the moment when a rupture of the figure takes place in any direction, the sun's disturbing force must then and there be equal or superior to the attraction towards the comet's centre of gravity.

On these principles it seems easy to account for the forms of comets, their transformations, and even for their disruptions, as in the case of Biela's comet. Their divisions and subdivisions into swarms of small comets, or meteoric gaseous matter, scattered along the track of the original orbit, are also simple consequences of the same analysis.

THE RED SPOT ON JUPITER: OBSERVATIONS UPON ITS PHYSICAL CHARACTER; ITS MAGNITUDE AND THE DETERMINATION OF THE ROTATION PERIOD OF THE PLANET. By H. S. PRITCHETT, of Morrison Observatory, Glasgow, Mo.

THE great red spot on Jupiter which has attracted such widespread attention from astronomers for two years past was first observed at the Morrison Observatory in July, 1878. During 1879, the spot was observed on about seventy different nights and many observations were made of transits across the central meridian as well as careful observations of its physical appearance and surroundings. The observations were continued until Feb. 7, 1880. Observations were resumed again early in May and are being continued at the present time.

The observations have been all made by Prof. C. W. Pritchett with the 12½ inch Equatorial of this Observatory and all with the same power, a positive eyepiece magnifying about 280 times. In what follows I shall attempt to give simply a résumé of what has been done, together with the approximate results arrived at in a preliminary discussion of the observations. The rough sketches which are given are intended to give a correct idea of certain phases of the spot and its environment without special regard to finish, or

the special detail of the planet's disk. They represent in each case the inverted image as seen in the telescope.

In 1878 the spot was seen only once, July 9, at which time a drawing was made and the spot was described briefly as being a "rose-tinted elliptical cloud." Although sought for the next night and on succeeding nights, when it should have been seen near the center of the disk, nothing more was seen of it during this opposition. For the failure to see the spot during the latter part of 1878 I am at a loss to account.

The general appearance of the spot and the physical phenomena of the surrounding surface may be best indicated perhaps by the following extracts from the observing books during 1879 and 1880.

1879, July 2.—Spot observed last year again seen for the first time. The major axis is about one-fourth longer than last year and the minor shorter. Color a bright pink and tinge deeper than that of the belts.

1879, Aug. 8.—At 11.5^h Gl.m.t., as for several days past, all the cloud masses called belts seem collected south of the equator except the broad and distinct northern equatorial belt. The surface south of the equator is dense with clouds except in the vicinity of the spot. Here the belt masses seem not to approach.

1879, Aug. 24.—Seeing very fine. For the first time fine red streaks were noticed extending out a short distance from the ends of the spot.

1879, Sept. 6.—A white spot, which was first observed on Sept. 4, is again seen, but much farther to the preceding side, showing a motion either of this or of the red spot.

1879, Sept. 13.—Spot and belts are nearly of the same color (bright pink). The spot is always pink while the belts take all hues from white to ashy, gray, brown and red.

1879, Sept. 20.—The appearance of Jupiter to-night was magnificent. The northern equatorial belt is much redder than the southern which is almost blue. Between the two main belts is an irregular mottled space interspersed with numerous white patches. One white spot just north of the preceding end of the red spot at time of transit. Two small dark cloudlets, one a little preceding, the other following, the west end of the red spot. Far north of the equatorial belt are two very sharp dark lines curving as parallel circles. It is worthy of note that the red spot does not seem to change its position relative to the dark cloudlets above it, while in

reference to the white spots situated between the equatorial belts it changes rapidly.

1879, Sept. 30 and Oct. 5.—The same dark spots are observed and keep the same relative position with respect to the red spot.

1879, Nov. 6.—A very fine needle-like streak extends from the following end of the spot. Definition fine. The environment of the spot is about the same as for many weeks.

1879, Nov. 25.—The dark gray masses before mentioned are still seen in the same positions.

1880, Jan. 14.—At $6^h\ 42^m$ Gl.m.t. the shadow of Sat. III was near the center of the spot as shown in the drawing. The shadow on the spot appears black as on the surface of the planet. Sometimes there appeared to be a slight penumbra around the black shadow, but there were thin clouds which gradually increased and at $7^h\ 14^m$ the observations were entirely interrupted by clouds.

At times, during breaks in the clouds, the spot and the black shadow in it and the belts were quite distinct; but it was impossible on account of clouds to observe the egress of the shadow from the spot.

1880, Feb. 1.—The spot was observed in full daylight. Both the belts and spot seem very distinct.

The spot was again observed for the last time on Feb. 7. Observations were commenced again on May 11, 1880.

On May 23, the seeing was good and a very good observation was obtained. In appearance the spot has changed but little from last year. It is decidedly farther north than when first seen in 1878. The preceding end slightly more acute than the following and the needle-like projection still exists.

1880, July 10.—There seems a remarkable absence of belts in both northern and southern hemispheres. The surface seems more luminous than usual. One large white spot exists in the equatorial belt directly north of the preceding end of the spot. There is a trough-like appearance between the belts reaching to the eastern limb.

1880, July 29.—Images very good. The belts are very brown, the northern much broader and darker, the interval between being much diversified by white patches almost continuous. A number of fine dark lines on the northern hemisphere.

These notes from the observing books seem to show that from July 1878 to July 1879 the spot became somewhat longer and nar-

rower and moved farther north and nearer the equatorial belts. Since 1879, it has changed its form and size but little, preserving the same outline and color and showing the same minute projections from the ends. It has kept uniformly throughout the observations a deep pink color bordering on red.

The luminous border spoken of so frequently by the English observers was noted only once or twice by one of the observers in 1879, and nothing whatever is seen of it in 1880. Even on the occasions when it was suspected to exist, it was described as being but little brighter than the ordinary bright disk of the planet, and I am strongly inclined to the belief that the appearance of the luminous border is due chiefly to the effect of contrast between the bright surface of the planet and the spot and adjacent belts.

Transits of the shadows of satellites across the spot have been observed under favorable conditions on four different occasions. The shadow in every case appeared black just as it is seen on the planet's disk with a slight suspicion at times of a penumbra.

The great relative motion of the white spots with respect to the red spot was often noted. In two of the sketches presented, those of July 10 and July 13, 1880, the motion of one of these spots in three days is very clearly shown. The fixity of the dark gray masses with respect to the red spot was also noted.

The persistence of this spot in size, color, position and general outline for so long a time, suggests very strongly that it is more than a displacement of the upper Jovian atmosphere and that its cause is analogous to the great solar upheavals which produce the larger and more permanent sunspots. Only a local cause operating from beneath the surface of Jupiter would seem to be able to produce a phenomenon so uniform and long continued. This phenomenon may give additional weight to the theory which regards Jupiter and the outer planets as secondary suns. The small change in the size of the spot is shown in the following measures (Table I, p. 178) made with a filar micrometer during 1879 and 1880. The measures are all reduced to the epoch of the first observations on July 10, 1879, and were made when the spot was at the center of the disk.

The small differences in the measures on different dates are probably accidental. The mean of the values of the two axes is 13.60″ and 3.98″. Using Newcomb's value of the Solar Parallax, these apparent chords correspond respectively to distances

of 26400 and 7700 miles. So that if Mr. Proctor should be correct in his hypothesis that this phenomenon is caused by the breaking away of the upper cloud masses of the Jovian atmosphere, exposing the heated surface beneath, the rent thus made is large enough to receive three globes of the size of our earth placed side by side.

TABLE I.

Date. 1879.	Major Axis.	Minor Axis.	Date. 1880.	Major Axis.	Minor Axis.
July 10	12.26"	3.96"	July 10	13.38"	"
27	13.55		13	14.19	
Aug. 18	12.73		24	13.24	
Sept. 4	14.72		29	13.35	
Nov. 23	14.40		Aug. 12	13.47	4.00
Dec. 12	14.35				
Mean.	13.68	3.96		13.53	4.00

About ninety observations of transits of this spot across the central meridian have been made at this Observatory. These observations were commenced July 10, 1879, and are now being continued.

The observations were made in the following manner. The movable thread of the micrometer was set perpendicular to and bisecting the apparent equatorial diameter using the Ephemeris of Mr. Marth. The transit of the preceding end was then noted. The transit of the center was obtained by placing one of the wires of the micrometer on each end of the spot and noting the time when the segments cut off on each side became equal. The transit of the following end was noted in the same manner as that of the preceding end. The mean of the times noted for the transits of the two ends rarely differed from that noted for the transit of the center by more than one or two minutes.

From twenty of the observations of highest weight, commencing July 10, 1879, and continuing until Feb. 5, 1880, including 507 rotations of the planet, I have computed the rotation period. The observations after being corrected for the aberration time and the relative motion of the earth and Jupiter furnished twenty equa-

tions of condition, from which there resulted for the Rotation Time $9^h\ 55^m\ 34.2^s \pm 0.7^s$, expressed in mean time.

This value needs yet a small correction dependent on the latitude of the spot and also one dependent on the phase of the planet; but since the observations used are almost equally distributed on each side of opposition, this will be but small. The observations used, as well as the other observations made between these dates are published in the Monthly Notices for May, 1880.

When the observations are completed we hope to make a complete discussion of them. The value of the Rotation Time here given has been arrived at by assuming that the spot had no proper motion, and is offered, of course, not with any idea of furnishing an absolute value of the Rotation Time, but for comparison with other values. The apparent motion of the spot with respect to the white spots and its fixity with respect to the dark gray masses have been spoken of before.

But the question of the real motion of the spot is yet to be settled by observations. The discrepancy between the rotation time here given and the commonly accepted value has been pointed out in many of the astronomical journals. In Table II (p. 180) are collected all the determinations of the Rotation Time which I have been able to find, together with dates, observer, length of duration of spot, color, approximate Jovicentric latitude and other notes concerning the character of the spot observed.

A glance at this table shows that the clear, bright spots situated within the equatorial belts and usually of short duration give a short Rotation Period from $9^h\ 50^m$ to $9^h\ 53^m$, while the dark isolated spots farther away from the equatorial region give a period varying from about $9^h\ 55^m$ to $9^h\ 56^m$; so that the white spots have an apparent motion with respect to the colored in the direction of the rotation. That the law of these motions may be determined by careful continued observation seems probable.

In this connection there is one curious mistake which occurs in many of the semi-popular works on Astronomy which may be noted. The Rotation Period of Jupiter derived by Sir George Airy which is perhaps more often quoted than any other is given by Klein, Flammarion and others as $9^h\ 55^m\ 24.2^s$. This value needs yet a correction on account of the change in the light equation during the period of observation; and the corrected definitive value $9^h\ 55^m\ 21.3^s$ is given by Airy himself later on in his Memoir in Vol. IX,

Mem. R. A. S. In some way the uncorrected value has been copied into many of the text-books.

All observers of Jupiter of late years are greatly indebted to Mr. Marth of England for his excellent ephemerides published in the Monthly Notices R. A. S.

TABLE II.

DATE.	OBSERVER.	APPROXIMATE POSITION.	COLOR, DURATION, ETC.	ROTATION TIME.	AUTHORITY.
				h m s	
1665	Cassini.	Touching Eq. belts.		9 56	Flammarion, Klein etc.
1672	"	"		9 55 51	" "
1677	"			9 55 50	" "
1690	"	Centre of Eq. belts.		9 51	" "
1692	"	" " " "	Bright spot.	9 50	Flammarion.
1773	Sylvabelle.		Black spot.	9 56	Flammarion, Klein etc.
1778	W. Herschel.	Equatorial zone.	Dark spot.	9 54 53 9 55 40	" "
1779	"	Equatorial.	Clear spot.	9 51 45	" "
1779.	"	"	" "	9 50 48	" "
1785–90	Schroeter.			6 56 56	Flammarion.
1786	"		Gray band—observed 90 days.	9 55 17	"
1786	"		Dark spot.	9 55 53	"
1786	"		Dark spot.	9 56 33	"
1834–35	Beer & Mädler.	5° N. Jovic. Lat.	Two dark spots, lasted several months.	9 55 26.5	Ast. Nach. Vol. 12.
1851	Schmidt.	In South streaks.	Large dark isolated spot followed for 121 rotations	9 56 45.4	" " No. 1542.
1862	"	30 N. Lat.	Dark spots.	9 55 28.7	Ast. Nach. No. 1542.
1862	Schmidt.	In South streaks.	Clear spot.	9 52 33	" " " "
1866	"	In North Belts.	Round white spot.	9 55 43.4	" " " 1627.
1866	"	" " "	" " "	9 55 49.3	" " " "
1834	Airy.	In North Belts.	Black well defined spot.	7 55 21.3	Mem. R.A.S. Vol. IX.
1873	Lord Ross.		Break in S. side Eq. Belts.	9 54 55	Flammarion.
1873–76	Flammarion.		Irregularities in Belts at Eq.	9 54 30	Les Terres de Ciel.
1873–76	"		Spots at 35° Lat.	9 55 45	" " " "
1872–73	Lohse.		Clear spots near Eq.	9 51 9 53	
1879	Pratt.	22° S. Lat.	Great Red Spot.	9 55 33.9	Mon. Not.
1879	Trouvelot.		" " "	9 55 38.6	Observatory.
1879–80	Pritchett.		" " "	9 55 34.2	

FIG. 1.

July 9, 1878, 12h. 18m.

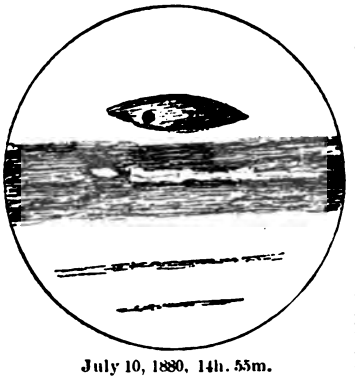
FIG. 2.

July 2, 1879, 16h. 10m.

FIG. 3.

Nov. 6, 1879, 6h. 21m.

FIG. 4.

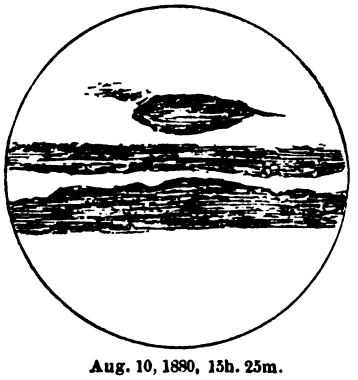


July 10, 1880, 14h. 55m.

FIG. 5.

July 13, 1880, 16h. 38m.

FIG. 6.



Aug. 10, 1880, 15h. 25m.

FRICTION OF LUBRICATING OILS. By C. J. H. WOODBURY, of Boston, Mass.

THE resistance existing between bodies of fixed matter, moving with different velocities or directions, presents itself in the form of a passive force which results in the diminution or the destruction of apparent motion. Modern science has demonstrated that this destruction is only apparent, being merely the conversion of the force of the moving body into the oscillation of the resisting obstacle or into that molecular vibration which is recognized as heat. Direct friction refers to the case where the two bodies are in actual contact, and mediate friction where a film of lubricant is interposed between the surfaces; and it is this which applies to nearly every motion in mechanics where bodies slide upon each other. The coëfficient of friction is the relation which the pressure upon moving surfaces bears to resistance. I have devoted some time to the examination of this subject, in the interests of the Mill Mutual Insurance Companies of New England. In this report of my work upon the measurement of friction of lubricating oils, I shall restrict myself to a description of the apparatus designed especially for the purpose, the method of its use, and the results obtained with a number of oils in our market which are used for lubricating spindles. Previous investigation of nine different oil testing machines used showed that none of them could yield consistent duplicate results in furnishing the coëfficient of friction. The operation of these machines, by their failure to obtain correct data, adduced certain negative evidence which established positive conditions as indispensable in the construction of a machine capable of measuring the friction of oils. The following circumstances must be known or preserved constant,—temperature, velocity, pressure, area of the frictional surfaces, thickness of the film of oil between the surfaces, and the mechanical effect of the friction. In addition to the foregoing conditions, the radiation of the heat generated by friction must be reduced to a minimum, and the arrangement of the frictional surfaces must be of such nature that no oil can escape until subjected to attrition. To measure the frictional resistance at the instant of a given temperature, and at a time when both temperature and friction are varying, requires a dynamometer which is instantaneous and automatic in its action.

The apparatus consists of an iron frame supporting an upright

shaft surmounted by an annular disk made of hardened tool steel. Upon the steel disk rests one of hard bronze (composed of the following alloy,—copper 32 parts, lead 2 parts, tin 2 parts, zinc 1 part) in the form of a cylindrical box. Water is fed in at one side and a diaphragm extending nearly across the interior produces a uniform circulation before discharge. Although this use of water is original with the writer in the method of its application, the first use of water to control the temperature of the bearing surfaces of oil testing machines is due to Monsieur G. Adolphus Hirn and described by him in a paper on the subject of friction, read before the Société Industrielle de Mulhouse, June 28, 1854. M. Hirn, however, confined his attention chiefly to the determination of the mechanical equivalent of heat, as measured by the amount of heat imparted to the circulating water, expressed in the work of friction. His investigations of lubrication with this apparatus were confined to the friction of lard and olive oils, at the light pressure of about one and four-tenths pounds to the square inch. Mr. Chas. N. Waite of Manchester, N. H., has independently, and I believe originally, made use of water in a friction machine, and has performed good work in the limit of his experiments.

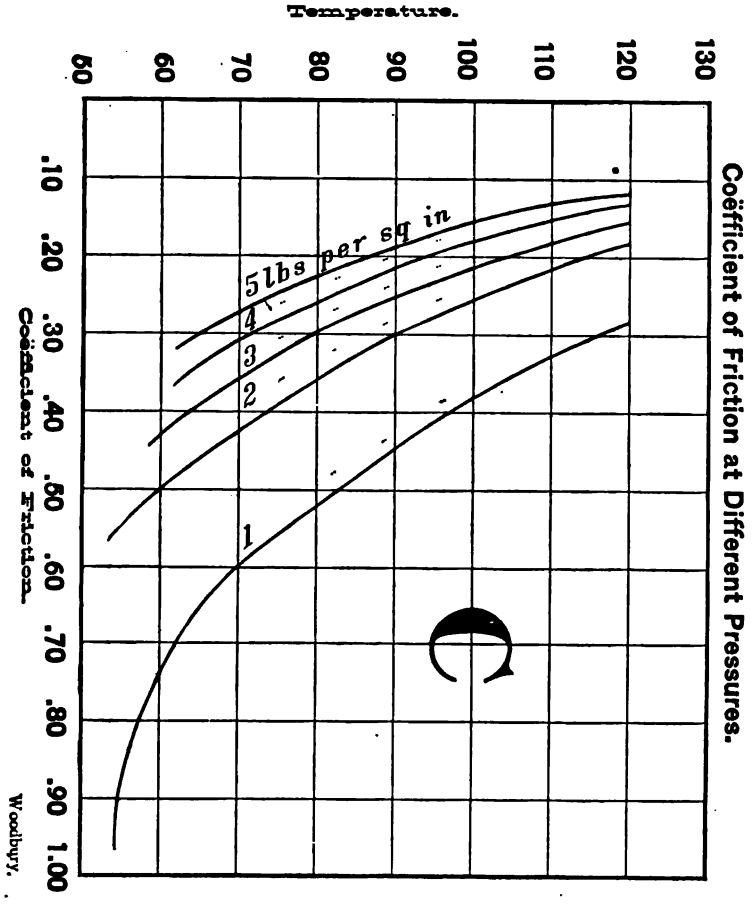
A protection of wool batting and flannel, to guard the disks against loss of heat by radiation, diminishes the escape of heat to about two degrees per hour, which loss is not appreciable when observations are taken within a few seconds' interval. A thin copper tube closed at the lower end, reaching through the cover, extends to the bottom of the disk; the bulb of a thermometer is inserted in this tube and measures the temperature of the disks, an oil tube runs to the centre of the disk, and a glass tube at the upper end indicates the supply and its rate of consumption and also serves to maintain a uniform head of oil fed to the bearing surfaces. The rubbing surfaces of both disks were made to coincide with the standard surface plates, in the physical laboratory of the Institute of Technology, and their contact with each other is considered perfect.

After this surface was finished, the bronze disk was treated with bichloride of platinum which deposited a thin film of platinum upon the surface. Upon the application of the disks to each other the steel disk rubbed off the platinum from all parts of the surface, showing the perfection of contact. This nicety of construction

enables a film of oil of uniform thickness to exist between the surfaces, and the resistances are not vitiated by the collision of projecting portions of the disk with each other. The rounded end of the upper shaft fits into a corresponding depression in the top of the upper disk. This method of connection retains the disk over the proper centre, yet it is allowed to sway enough to correct any irregularity of motion caused by imperfection of construction or wear of the lower disk. To obtain the desired condition of pressure, weights are placed directly upon the upper spindle. The axes of the upper and lower spindles do not lie in the same straight line, but are parallel, being about one-eighth of an inch out of line with each other; such construction, giving a discoid motion, prevents the disk from wearing in rings and assists in the uniform distribution of the oil. An arm is keyed through the lower part of the upper spindle and engages with projections upon the upper disk. Upon this arm which is turned to the arc of a circle, whose development is two and one-half feet, a thin brass wire is wrapped upon this arc and reaches to the dynamometer so that the tension of the dynamometer is tangential and the leverage is constant for all positions of the upper disk within its range of motion. The dynamometer consists of a simple bar of spring steel fastened at one end, and bent by the pull applied at the other. Its deflection is indicated by a pointer upon a circular dial, the motion of the spring being multiplied about eighty times by a segment and pinion. The whole is enclosed in a steam-gauge case.

When completed, the machine was subjected to a long series of tests with the same oil to determine the accuracy of the results, and the best method of procuring them. The operation of the machine under equal conditions with the same oil gives results which are as closely consistent with each other as could be expected from such physical measurements. As an example, four tests of the Downer Oil Co. Light Spindle (Sample No. 7) at 100° Fah. and on different days gave .1145, .1094, .1118, .1094; mean, .1113. Another example (Sample No. 14) Heavy Spindle Oil, made by the same firm, yielded for a coëfficient of friction as the result of five different trials .1246, .1195, .1297, .1201, .1221; mean, .1233. Much of the irregularity, slight as it is, is due to the variable speed of the engine. Concurrent results were obtained under equal circumstances, but the coëfficient of friction varied, not merely with the lubricants used, but also with the temperature, pressure,

and velocity. The results of my own experiments on mediate friction do not agree with the laws of friction as given in works on mechanics, but the coëfficient of friction varies in an inverse ratio with the pressure as shown graphically on diagram C.

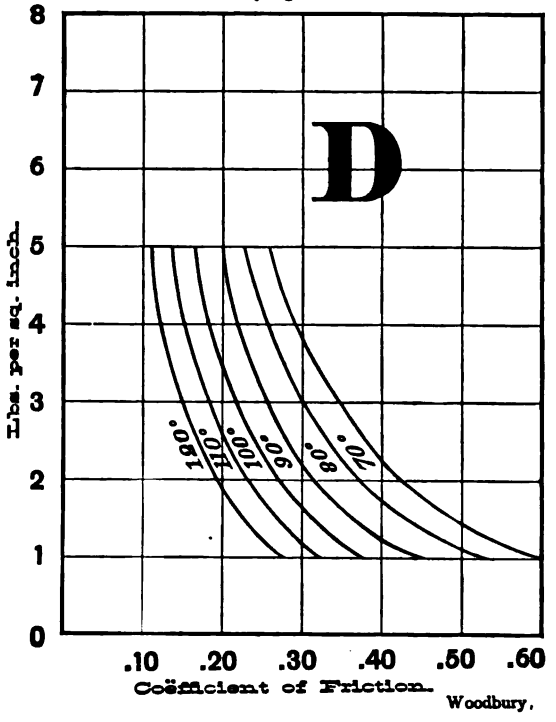


The variation of friction with the temperature is shown graphically on all sheets reporting the friction of each sample of oil. These curves belong to the hyperbolic class of a high degree; but I have not been able to deduce an equation which will answer to the conditions of more than one, because the law of the curves is modified by a constant dependent upon the individual sample of

oil used. A little difference in the sample would cause a difference in the line of curve.

Reference is made to diagram D showing the coëfficient of friction under equal ranges of temperature and velocity, but with a different series of pressures.

Curves showing Changes of Coëfficient of Friction under Varying Conditions.



Coëfficient of friction at 100° and 500 revolutions per minute.

Pressure per sq. inch.	Coëfficient of friction.
1 pound	.3818
2 "	.2686
3 "	.2171
4 "	.1849
5 "	.1743

The ratio of the changing coëfficient varies with the temperature at which the range of results is taken.

Friction varies with the area, because the adhesiveness of the lubricant is proportional to the area, and the resistance due to this cause is a larger fraction of the total mechanical effect with light, than it is with heavy pressures.

The limit of pressure permitting free lubrication varies with the conditions; for constant pressures and slow motion it is believed to be about five hundred pounds per square inch, while for intermittent pressures, like the wrist pin of a locomotive, the pressure amounts to 3000 pounds per square inch. It has been stated that about 4000 foot pounds of frictional resistance per square inch is the maximum limit of safe friction under ordinary circumstances.

As the results of this preliminary work indicated that the coëfficient of friction varied with all the circumstances, it was necessary to simulate the conditions of specific practical applications to determine the value of a lubricant for such purposes.

It was decided to begin these investigations with spindle oils, and therefore the machine was loaded with five pounds to the square inch and run at about five hundred revolutions per minute; as the oil is then submitted to conditions of attrition corresponding to those met with in extremes of velocity and pressure, in the case of a Sawyer spindle running at 7600 revolutions per minute with a band tension of four pounds, and the results subsequently given refer only to the friction under these conditions, except when definitely stated to the contrary.

This particular spindle was selected because, of the five million ring spindles in the United States, about one and a half million are of this manufacture, and in a large number of the remainder, the conditions of lubrication are quite similar.

The apparatus is used in the following manner to measure the coëfficient of friction of oil. After cleaning with gasoline and wiping carefully with wash leather, the disks are oiled and run for about five hours, being kept cool by a stream of water circulating through the upper disk. From time to time they are taken apart, cleaned, and oiled again. After using any oil, even if the disks are afterwards cleaned, the results with the oil subsequently used give the characteristics of the previous oil, and it is only after thirty-five to forty-five miles of attrition that these results become consistent with each other; each succeeding result, meantime, approaching the final series. This seems to indicate that the friction exists at the surface of the two disks between the film of

oil acting as a washer and the globules of oil partially embedded within the pores of the metal. If the dense bronze and steel retain the oil despite attempts to remove it, how much longer must it require to replace the oil in machinery with a new variety whose merits are to be tested. These experiments confirm the wisdom of the increasing use of cast iron for journals, as its porosity enables it to contain and distribute the lubricant.

When the disks are ready to test the oil, the apparatus is cooled by the circulation of water; the flow of which is stopped when the machine is started. At every degree of temperature, the corresponding resistance is read on the dynamometer. When the thermometer indicates a temperature of sixty degrees, the counter is thrown in gear, and the time noted. When one hundred and thirty degrees is reached, the counter is thrown out of gear, and the time noted. This not only gives the velocity of the rubbing surfaces, but the number of revolutions required to raise the temperature a stated number of degrees, and is a close criterion of the oil. The coëfficient of friction is the ratio of the pressure to the resistance, and is deduced in the following manner.

P = Weight on disks.
R = Outer radius of frictional contact.
V = Inner " " " "
N = Number of revolutions per minute.
W = Reading on dynamometer.
Φ = Coëfficient of friction.

In the friction of annular disks, the portions of the surfaces near the perimeter have a greater leverage than those near the centre. The mean sum of these moments is found by the calculus.

Let ρ = radius of any infinitesimal narrow ring or band.

Width of band $d\rho$ (1)
Length " " $2\pi\rho$ (2)
Area " " $2\pi\rho\, d\rho$ (3)
Moment " " $2\pi\rho^2 d\rho$ (4)
The expression for the area of an annular disk $= \pi(R^2 - r^2)$ (5)
To express the moment of a ring in terms of an annular surface we divide (4) by (5), which gives
Moment in terms of disk $= \frac{2\pi\rho^2 d\rho}{\pi(R^2 - r^2)} = \frac{2\rho^2 d\rho}{R^2 - r^2}$. (6)
Moment of whole disk $\frac{2}{R^2 - r^2}\int \rho^2 d\rho$ (7)
Integrating of whole disk $\frac{2}{R^2 - r^2}\left[\frac{\rho^3}{3}\right]_r^R$ (8)

Substituting the limits $\frac{2}{R^2-r^2}\frac{R^3-r^3}{3}$ (9)

The work of friction $= \varphi P$ (10)

Statical moment of friction of disk $\frac{2\phi P(R^3-r^3)}{3(R^2-r^2)}$ (11)

Mechanical effect $= \frac{4\pi\phi P(R^3-r^3)}{3(R^2-r^2)}$ (12)

Foot lbs. at any velocity $\frac{4\pi\phi P(R^3-r^3)N}{3(R^2-r^2)}$ (13)

As previously stated, the dynamometer exerts a pull at the end of a lever whose development is $2\frac{1}{2}$ ft.

Resistance of dynamometer $= \frac{5W}{2}$ (14)

" " " in ft. lbs. at any velocity $= \frac{5WN}{2}$ (15)

As the total friction = the resistance of the dynamometer

Eq (13) = Eq (15)

$$\frac{4\pi\phi PN(R^3-r^3)}{3(R^2-r^2)} = \frac{5WN}{2} \quad (16)$$

$$8\pi\varphi PN(R^3-r^3) = 15WN(R^2-r^2) \quad (17)$$

$$8\pi\varphi P(R^3-r^3) = 15W(R^2-r^2) \quad (18)$$

$$\varphi = \frac{15W(R^2-r^2)}{R^3-r^3)} \quad (19)$$

Separating the constants

$$\varphi = \frac{15(R^2-r^2)}{8\pi(R^3-r^3)}\frac{W}{P} \quad (20)$$

$R = .2213$ feet.

$r = .1211$ feet.

$R^3 = .01083$			$R^2 = .0489$		
$r^3 = .00177$			$r^2 = .0146$		
$R^3 - r^3 = .00906$	log	7.9571282	$R^2 - r^2 = .0343$	log	8.5352491
$\pi = 3.1416$	"	0.4971499	15	log	1.1760913
8	"	0.9030900			9.7113404
		9.3573681	. . . . .		9.3573681
					$2\ 259 = 0.3539723$

$$\varphi = \frac{2.259\,W}{P} \quad (21)$$

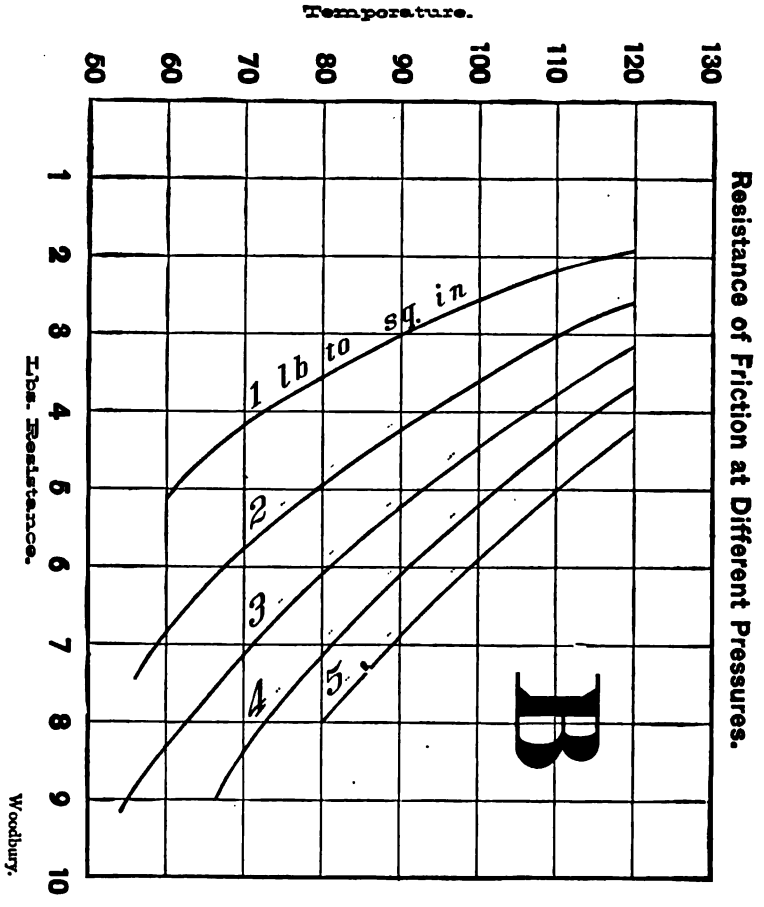
This equation was solved for each reading of the dynamometer with five pounds to the square inch pressure and the results tabulated in a convenient form for computing the coefficient of friction from the observed results.

The following table shows the resistance of friction at 100° 500 revolutions, for various pressures.

RESISTANCE OF FRICTION AT 100°

Pressure.	Resistance on Dynamometer.	Equivalent band tension.
1 lb.	2.62 lb.	0.8
2 "	3.68 "	1.6
3 "	4.48 "	2.4
4 "	5.28 "	3.2
5 "	5.98 "	4.0

For further detailed results, reference is made to diagram B.



These results seem to be intimately relevant to the most desirable limit of tension to the spindle band and methods of operating cotton spinning machinery. By weighing the band tension in various mills it was found that the practice of tying bands lacked uniformity. As an example of this variation: in one mill the bands of a single coarse frame are reported to vary from 1 to 16 lbs. In another mill on finer work, a number of spindles had a range of from $\frac{1}{2}$ to $2\frac{1}{2}$ lbs. and in a third mill the band tension was between the limits of $\frac{1}{4}$ to 5 lbs. The effect of atmospheric changes upon the fibre of textile bands renders it impossible, with the

present method of constructing frames, to keep them at a uniform tension, but this variation can be reduced by a little care. Is it not worth while for each spinner to learn the proper band tension required for his special work, and then keep within those limits? The whole power required to run the frame would not vary in direct proportion to the varying resistance due to the friction of spindles at various pressures, because the resistance of the friction in other parts of the frame connected with the spindles, the actual spinning of cotton fibres, and the alternate contraction and expansion of the bands, are conditions which are more nearly constant and in no case do they vary in proportion with the friction of the spindle; yet the variation is large, as shown by the following experiment made with the frame:

Mr. Geo. Draper, in a communication to the *Industrial Record* of June 1, 1879, gives the following valuable data on this subject. "A frame of Sawyer spindles was taken spinning No. 30 yarn, ordinary twist, the front rolls running ninety-five revolutions per minute. The rings 1⅛ in. diam. and the traverse of the yarn on the bobbin 5½ in. The dynamometer was applied and the power required to drive the spindles with a side pull of the bands, averaging 2 lbs. to a spindle, was ascertained. The bands were then cut off and a new set put on with a side pull of 3 lbs. per spindle and the frame tested again, all things remaining as before. The operation was then repeated at 4, 5, 6, 7, 8, and 9 lbs. side pull per spindle with the result shown in the following table."

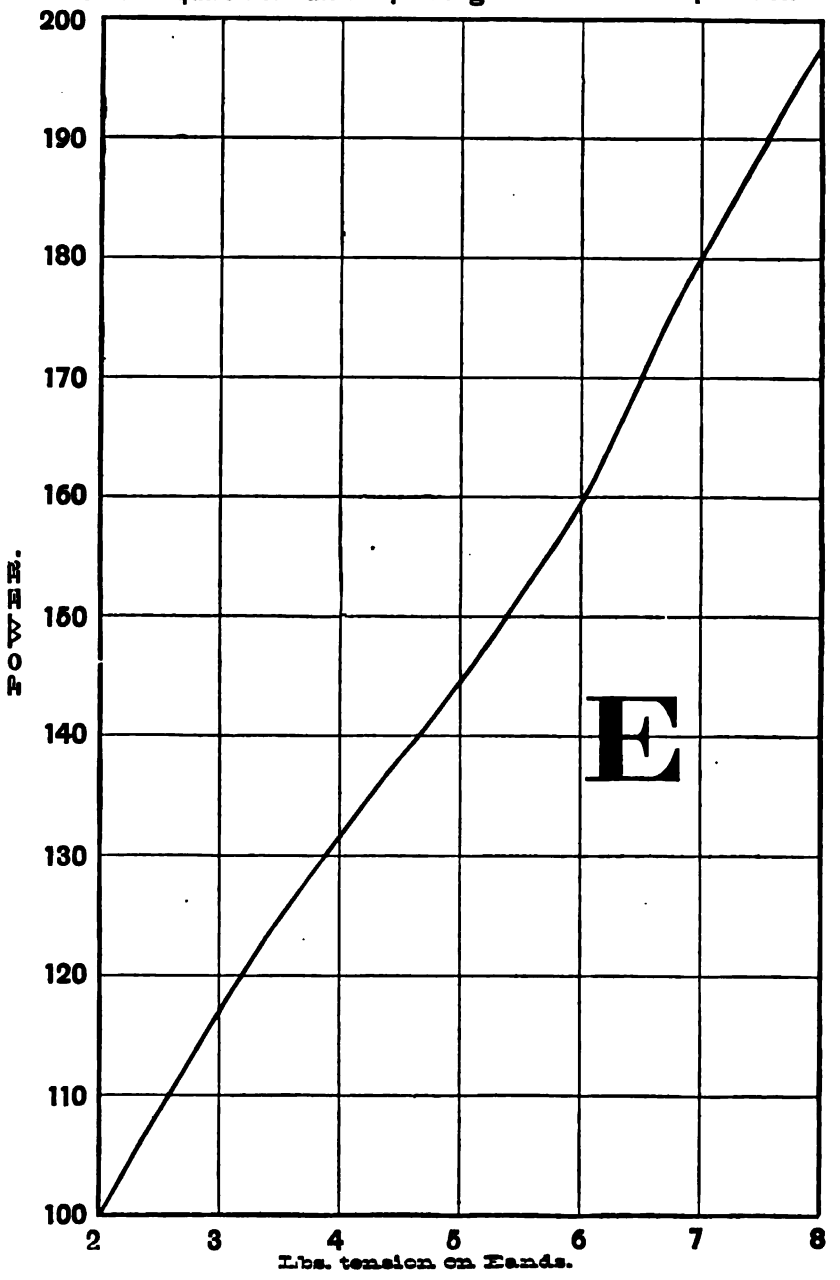
Calling the amount of power required to drive the spinning frame with:—

2 lbs. tension on the bands	100
3 " " " " "	117
4 " " " " "	131
5 " " " " "	144
6 " " " " "	159
7 " " " " "	177
8 " " " " "	197
9 " " considerably more than double.	

The results are shown graphically in chart E (p. 192).

The lubricant used is one of the most important factors in the cost of power. In the present condition of engineering science, it is impossible to state what exact proportion of the power used by a mill is lost in sliding friction, but in a print cloth mill only

Power required to run a Spinning Frame in full Operation.



about 25 per cent. of the power is utilized in the actual processes of carding, spinning, and weaving the fibre, not including the machinery engaged in the operation, leaving 75 per cent. of the power as absorbed by the rigidity of belts, the resistance of the air and friction. The coëfficient of friction, under the conditions submitted by my oil tester varies at 100° 500 revolutions, from 7.56 per cent. in the case of 32° Ex. machinery oil manufactured by the Downer Oil Co., to 24.27 per cent. in the case of neatsfoot oil; and the result of this investigation confirms me in the opinion that the successful operation of a spinning frame is far more closely dependent upon the individual management in respect to the conditions of band tension, lubrication, and temperature of the spinning room, than all other causes combined. Not that some forms of spindle are not superior to others, but that, without wise supervision, the most desirable forms of spindle must fail to show the merits due to the skill of their promoters. It may be stated that within a close approximation the lubricating qualities of an oil are inversely proportional to its viscosity; that is, the friction decreases with the cohesion of the globules of the oil, for each other. The endurance of a lubricant is in some degree proportional to its adhesion to the surfaces forming the journal. An ideal lubricant in these respects would be a fluid whose molecules had a minimum cohesion for each other, and a maximum adhesion for metallic surfaces. The viscous oils will also adhere more strongly to metals, and hence under the conditions of heavy bearings it is obligatory to use such thick lubricants, knowing that the employment of an oil with great frictional resistance is infinitely preferable to the attempt to use an oil so limpid that it could not be retained between the bearings. With light pressures the more fluid oils are admissible, and in all cases the oils should be as limpid as the circumstances will permit. Oils with great endurance are apt to give great frictional resistance, and in the endeavor to save *gallons* of oil, many a manager has wasted *tons* of coal. The true solution of solving the problem of lubricating the machinery of an establishment is to ascertain the consumption of oil, and the expenditure of power, both being measured by the same unit, viz., dollars.

The fluidity of the oils was measured by the following apparatus. A pipette was placed within a glass water-jacket where the temperature was controlled and kept constant by circulation from a

reservoir kept at the desired temperature. The capacity of the bulb is 28 cubic centimetres and the orifice measures three and a half inches long and .039 of an inch in diameter.

The oil was drawn into the bulb of the pipette, and after the whole was brought to the desired temperature, the time required for its discharge accurately noted by a stop watch.

These observations were made on each of the oils for a series of temperatures varying from 50° to 150° Fah.

If the fluidity of an oil is the measure of its lubricating qualities, these observations would not be identical with the frictional results, because the pressure in this case was that due to a head of about five inches of oil, or about one-sixth of a pound to the square inch and rubbing against a glass surface; while with the frictional machine the pressure was five pounds to the square inch and the surfaces bronze and steel.

In both cases, however, the character of the surfaces and the pressures were uniform conditions and therefore, they would not affect the relations of either set of experiments in their consistency with each other. If the lubrication and fluidity of oils followed the same law of variation with the temperature, the results of one would be directly proportional to those in the other, provided that all other conditions were preserved constant. Such comparisons showed that the relations of the fluidity to the lubricating qualities did not follow any uniform ratio. At a low range of temperatures, the fluidity increased faster than the lubricating quality of the oil; between about 70° and 110° the coincidence was quite close; at higher temperatures, the fluidity does not increase so fast as the lubrication. There was not a very close correspondence between the fluidity of oils at the same coëfficient of friction.

The result of these investigations upon the relation of fluidity to lubrication seems to indicate that fluidity is a concomitant rather than a cause of the anti-frictional qualities of a lubricant.

In the case of mining drills operated by condensed air, an intense cold is produced at the liberation of air, and on some such bearings, kerosene oil is the only lubricant which can be used. I think it extremely probable, that at these low temperatures, the viscosity of kerosene oil is equal to that of lubricating oils at the average temperature of bearings in general use. On the other hand, only the most viscous oils can be used in such extremely high temperatures as the cylinder and steam chest of steam engines.

According to the results which I have obtained, the coëfficient of friction at 50° is about 75 per cent. in excess of that at 75°, and it seems to me that the manager of every mill, which is run by steam, ought to consider the question of the temperature of the mill in early morning during the winter months, whether, as a matter of economy, it is cheaper to warm a mill by increased friction on Monday morning, or to keep the mill and machinery warm during the interval from the preceding week.

The humidity of the atmosphere is an important factor in the mechanical operation of textile machinery, as well as in the fabrication of cotton. A year ago I submitted, to the New England Cotton Manufacturers' Association, measurements showing the effects of humidity on textile bands, and I am also of the opinion that there is a difference of friction in machinery due to atmospheric influences upon the lubricant.

Possibly the moisture condensed upon the cold metal from the atmosphere becomes commingled with the oil and thereby reduces its viscosity, diminishing the friction.

The question of endurance of oils has not been given in these experiments, because the consumption of oil varies with the temperature, and it is proposed to investigate the matter subsequently by running the machine and controlling the temperature of the disks to 100° by the circulation of water. The amount of oil consumed could be very easily measured by the difference in the level of the glass feeding tube or the weight of the oil required to preserve it at that level during the experiment.

In the detailed results the friction is given for the whole range of temperatures, but in the following summary 100° has been selected as the temperature which most nearly corresponds to the heat of spindle bearings.

To ascertain these temperatures, holes were drilled in the rails of a spinning frame, passing as near the bolsters and steps as possible; the bulbs of thermometers were inserted in these holes, and while the frame was in operation 2586 readings were taken covering a period of four weeks. The temperature of the air was noted from a thermometer placed in the middle of the frame.

The mean temperature of the bolsters was 8.10° Fah., and of the steps 6.74° Fah. above the temperature of the room.

Other experiments were made, to learn the temperature of the bearings of the shafting. Holes about half an inch in diameter

were bored in the upper cap of such journals; and a thin copper tube, closed at the lower end, inserted and extended nearly to the shaft. This tube contained water and the temperature was measured by a thermometer placed therein. There were journals in good running order whose temperature at the frictional surfaces was 140° Fah. This method of using thermometers was first suggested by Mr. Edward Atkinson, and I consider it the most accurate test of the anti-frictional qualities of a lubricant at the service of those in charge of machinery.

Great pains have been taken to procure pure samples of the oils experimented with, and they were obtained directly from the manufacturers; and to the courtesy of Mr. Thos Bennett, jr., I am indebted for a large number of samples of sperm oils which were procured by him directly from the whale-ships or refiners.

The following table gives the coefficient of friction at 100° Fah. and 500 revolutions, with a pressure of five pounds to the square inch: —

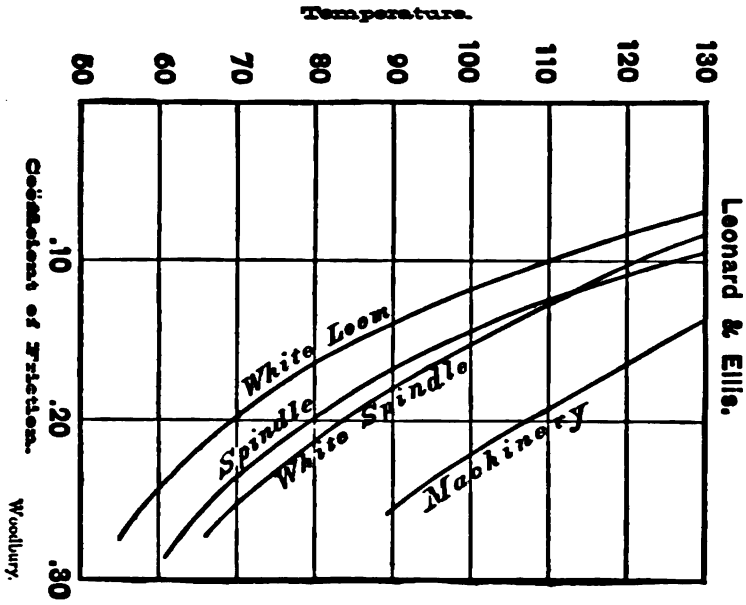
No. of sample.	Kind of oil.	Coëff. of friction at 100°
1.	Mineral oil, [Unknown]	.1635
28.	Downer Oil Co., Champion	.1732
10.	" " " Heavy Spindle	.1187
14.	" " " " "	.1233
19.	" " " " "	.1208
7.	" " " Light Spindle	.1113
20.	" " " " "	.1132
8.	" " " 32° Ex. Machinery	.0756
2.	Leonard and Ellis, Valvoline Spindle	.1476
11.	" " " White Valv. "	.1493
12.	" " " Valvoline White Loom	.1201
15.	" " " " Machinery	.2243
13.	Olney Bros., German Spindle	.1190
16.	" " A "	.1103
3.	John P. Squire, Lard	.2181
4.	Bleached Winter Sperm A	.1067
5.	" " " B	.1217
6.	" " " C	.1170
9.	Aiken & Swift, Bleached Winter Sperm	.0956
17.	" " " Unbleached " "	.1147
18.	" " " Bleached " "	.1141
21.	Seal Oil	.1608
22.	Alex Boyd & Sons, Neatsfoot	.2427
23.	Mixed animal and mineral oil [Unknown] 1	.1608
24.	" " " " " " 2	.1377
25.	" " " " " " 3	.1190

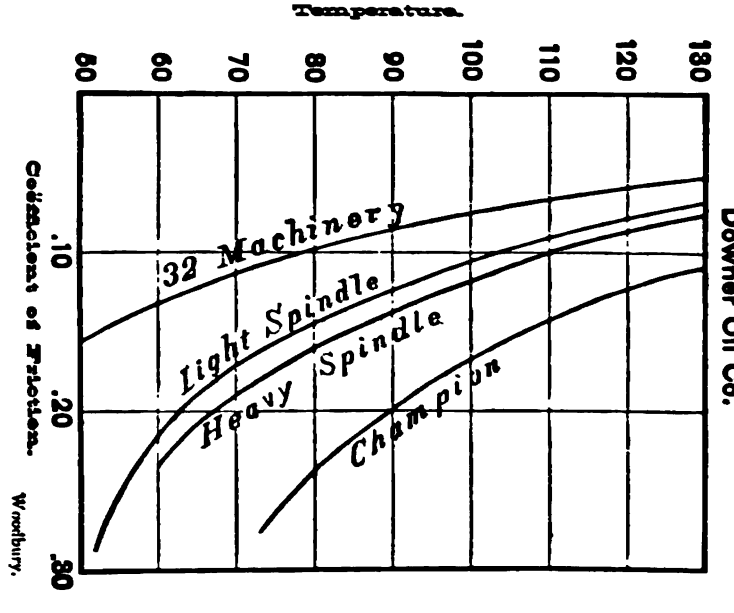
Chemical examinations of these oils by Mrs. Ellen H. Richards of the Women's Laboratory, Institute of Technology: —

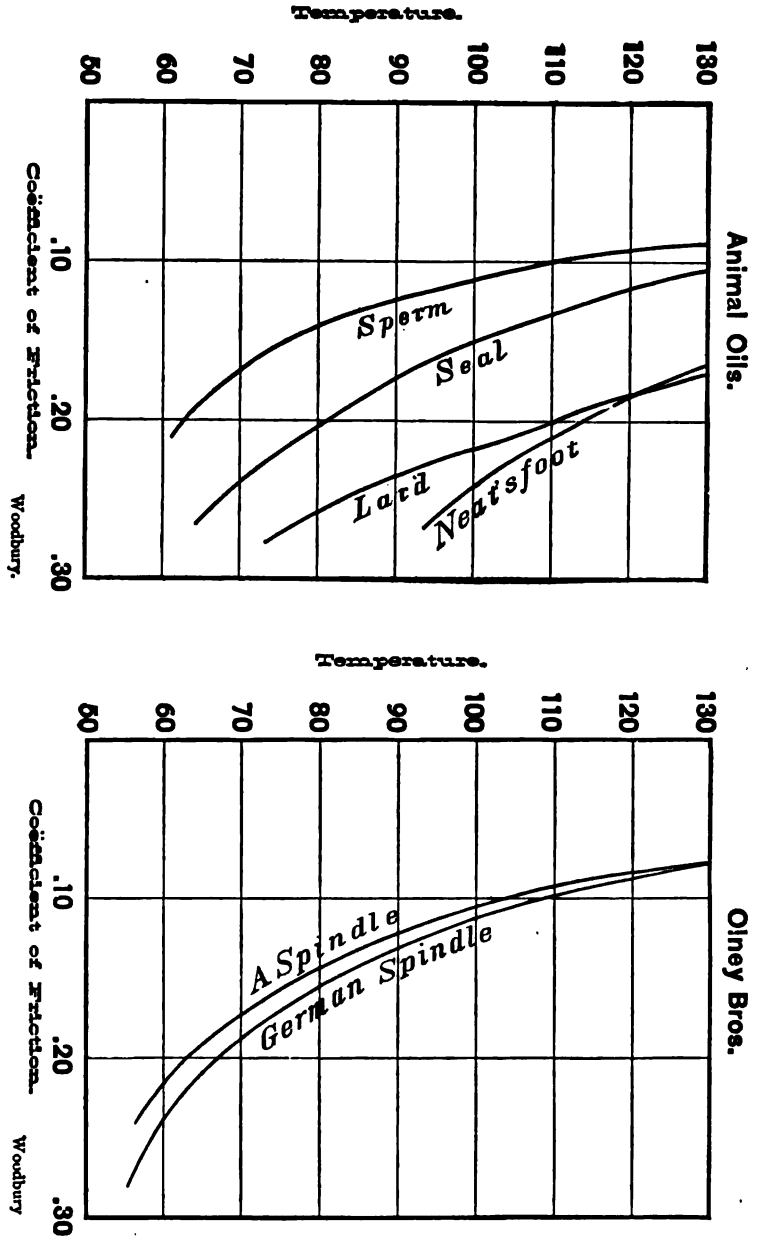
	Flash of vapor Fah.	Loss of evaporation in 12 hours at 140° *per ct.*	Nitro Sulphuric Acid test.
1.			
10.	338°	1.3	Dark yellow with much cake.
14.			
19.			
7.	314	2.7	Dark yellow, some cake.
20.			
8.	284	5.5	Slightly yellow only a few flakes of cake.
2.	316	3.7	Dark yellow very thin layer cake.
11.	324	3.9	Slightly yellow not on brown specks.
12.	318	3.3	Yellow, not a single flake, no solid matter.
15.	286	7.2	Turned dark, gives a black layer of gum.
13.	322	1.9	Quite an amount of cake.
16.	282	5.0	" " " " "
			[mass.
3.		+ 0.4	Hardened with much acid to a white solid
4.			
5.			
6.			
9.		+ 0.3	Thickened up a little like jelly.
17.			
18.			

With castor oil the friction was so great as to throw off the belt driving the machine; and as the time allotted for this work expired on that day, other arrangements for a wider belt could not be made, and it can only be said that its friction exceeds that of any other oil given in these tables.

The following diagrams (pp. 198, 199) give a graphical representation of the coëfficient of friction for the several oils at temperatures between 50° and 130° Fah.


Temperature.
50
60
70
80
90
100
110
120
130
Coefficient of Friction.
.10
.20
.30
White Loom
Spindle
White Spindle
Machinery
Leonard & Ellis.
Woodbury.


Temperature.
50
60
70
80
90
100
110
120
130
Coefficient of Friction.
.10
.20
.30
32 Machinery
Light Spindle
Heavy Spindle
Champion
Downer Oil Co.
Woodbury.


Temperature.
50
60
70
80
90
100
110
120
130
Coëfficient of Friction.
.10
.20
.30
Woodbury.
Sperm
Seal
Lard
Neat's foot
Animal Oils.
Temperature.
50
60
70
80
90
100
110
120
130
Coëfficient of Friction.
.10
.20
.30
Woodbury
A Spindle
German Spindle
Olney Bros.

The anti-frictional properties of these oils under the condition of these experiments are expressed in the following order: —

Manufacturers.	Oil.
Downer Oil Co.	32 Extra Machinery.
Aiken & Swift.	Bleached Winter Sperm.
Olney Bros.	A Spindle.
Downer Oil Co.	Light Spindle.
Aiken & Swift.	Bleached Sperm.
Aiken & Swift.	Unbleached Sperm.
Olney Bros.	German Spindle.
Leonard & Ellis.	Valvoline White Loom.
Downer Oil Co.	Heavy Spindle.
Leonard & Ellis.	Valvoline Spindle.
Unknown.	Seal.
Downer Oil Co.	Champion.
John P. Squire.	Lard.
Leonard & Ellis.	Valvoline Machinery.
Alexander Boyd & Sons.	Neatsfoot.

It is no disparagement to the qualities of an oil that it is low in the foregoing list, except so far as it relates to the resistance of friction under these conditions. For circumstances of great pressure and slow motion, I am of the opinion that the order of the list would be varied; and if the question of endurance were only to be considered, still another change in the order would be necessary.

A portion of a lot of unbleached sperm oil (sample 17) was bleached expressly for these tests (sample 18) but the results of the two are so nearly uniform as to be practically identical. The result of bleaching does not affect the anti-frictional properties of the oil, although it undoubtedly reduces its gumming qualities.

The friction of sperm oil is subject to sudden variations which occur at certain temperatures for the same sample of oil. The explanation of this lies in the fact that sperm oil consists of a large number of varieties of spermaceti, each of which is liquefied at certain temperatures, at which the oil is relieved of waxy or at least gelatinous particles and becomes a more perfect lubricant.

This is very apparent in the irregularities existing in the graphical representations of the tests of samples marked B and C.

The friction of lard oil for high temperatures exceeds that of any other lubricant in the list; and this adhesive quality enables it to remain on tools used for cutting iron.

In conclusion, it may be stated that the data necessary to determine the safety and efficiency of a lubricant comprise:

1. The flashing point of its vapor, which is ascertained by slowly heating a sample over an oil bath, quickly passing a small flame over the oil and noting the temperature at which the vapor first flashes. The danger from an oil does not arise from the point at which the oil actually ignites, but at the lower temperature when the inflammable vapor bursts into flames which communicate fire to a distance limited only by the extent of the vapor.

2. The quantity of such volatile matter is important both as respects safety and value. The heat of friction liberates that portion of the oil which is volatile at the temperature of the bearings; filling the mill with a dangerous, noxious vapor, and also dissipates in the air a portion of the oil which is paid for by the gallon, but does not serve to give any return of value in lubrication.

The quantity of matter volatile under 140° Fah. is measured by heating a known weight of oil in a watch glass and maintaining a constant temperature of 140° Fah. for twelve hours. This simulates the conditions of the temperature of the bearings mentioned previously, and the maximum time that it would be consecutively heated.

In the case of mineral oils the loss from evaporation varied from less than one up to thirty per cent. With animal and vegetable oils there is a slight gain in weight due to oxidation.

3. The tendency to spontaneous combustion is estimated by a uniform amount of cotton waste smeared with a certain quantity of oil.

A thermometer whose bulb extends to the centre of the mass indicates any rise of temperature due to oxidation. Any gain of weight during the preceding evaporation test shows a liability to spontaneous ignition.

4. Freedom from acid is an important factor in oil, because acid is a cause of corrosion of metals, and will tend to remove the oil from the frictional surfaces where adhesion is indispensable. The presence of acids is shown by corrosion of copper.

5. The anti-frictional properties of an oil can be measured only by direct trial under the desired conditions of pressure, velocity and temperature.

The results of these experiments show that a lubricant must have a certain adhesion to the frictional surfaces to maintain free lubrication; but beyond that point the adhesiveness of the oil resists the motion of the surfaces, increasing the friction. A thick oil

gives greater frictional resistance than a thin one; and, where ease in running is the object, the most limpid oil should be used, consistent with the conditions of the specific circumstances of the bearing.

In general terms: the specific gravity of an oil gives no indication of its value as a lubricant, in qualities of viscosity, body, or endurance.

This work is the result of a part of the ordinary service of certain mutual insurance companies. As there appears no relation between underwriting and an investigation involving questions of chemistry and mechanics, I beg to add a few words by way of explanation of the system of the factory mutual insurance companies. The better class of factories are associated together in mutually sustaining each other's losses by fire, forming the most powerful scheme of coöperation in the world. The amount of property so indemnified being some two hundred and six million dollars, or an amount exceeding three-fifths of the aggregate National Bank circulation of the United States.

The surplus of the year's work is distributed among the assured, giving the clients protection at the cost of the annual losses by fire and the expense of administering the company; the latter item amounting to a sum but little in excess of the interest accruing from the premiums deposited for indemnifying the claimants for losses.

Their policy has been to anticipate all preventable causes of fire, prescribing the ounce of prevention. Instead of charging a premium commensurate with the estimated hazard of a structure and thus betting against a chance; they demand that the mills shall be brought up to the standard of their associates in all matters of approved construction, equipment of fire apparatus and orderly supervision. Instead of making the rate conform to the risk, the risk is brought up to the rate. Mohammed does not go to the mountain, but the mountain comes to Mohammed. To achieve this end, all that theoretical knowledge can suggest, or practical experience accomplish, is utilized.

The result of this system has served to reduce the cost of indemnifying cotton mills against loss by fire, by a sum equal to saving

their capital every thirty-five years, if the saving be compounded at 6 per cent., reducing in cost every piece of New England cotton fabric.

In demanding a better class of mills as the art of their construction has been developed, in all that pertains to light, warmth, sanitation, ample stairways, protected elevators, and the abolition of garrets; in everything relating to physical comfort and safety they have incidentally accomplished more for the amelioration of the employed, than all the class legislation ever enacted in this state.

Furthermore, the capital employed in textile manufacturing has reaped an indirect benefit from mutual insurance. Besides the reduction in rates, the decreasing number of destructive fires in such mills has also diminished the losses arising from interrupted business; a risk which is not covered by mutual insurance.

This system of mutual mill insurance was founded by the Hon. Zachariah Allen, of Providence, one of the founders of the American Association for the Advancement of Science, who is present with us to-day, his zeal for science unabated, notwithstanding the burden of over eighty-five years.

This investigation of the subject of lubrication forms a part of a study of oils, undertaken on behalf of the Mill Mutual Insurance Companies, in order to determine the conditions of safety from fire and economy in use; the chief causes of danger in textile factories being from friction or spontaneous combustion. The whole subject has been under the charge of Prof. J. M. Ordway, Professor of Industrial Chemistry in the Massachusetts Institute of Technology.

In the conduction of the lubrication tests, I desire to express my gratitude to Professor Ordway for his kindly assistance and many suggestions, rendered especially valuable by his scientific attainments and practical experience.

Frictional test of Mineral [Unknown] Oil, on March 27 to 29, 1880. Sample nearly two years old. Rev. per min. 475.55, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces, 534.99 feet per minute. No. of sample 1. No. trials 10.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.2675	78	.2242	92	.1880	106	.1475	120	.1151
51	....	65	.2644	79	.2210	93	.1840	107	.1442	121	.1130
52	....	66	.2596	80	.2195	94	.1815	108	.1420	122	.1111
53	....	67	.2537	81	.2169	95	.1782	109	.1399	123	.1095
54	....	68	.2523	82	.2144	96	.1751	110	.1368	124	.1085
55	....	69	.2492	83	.2117	97	.1723	111	.1338	125	.1051
56	....	70	.2469	84	.2103	98	.1705	112	.1316	126	.1044
57	....	71	.2448	85	.2050	99	.1694	113	.1299	127	.1024
58	....	72	.2435	86	.2035	100	.1615	114	.1275	128	.1009
59	....	73	.2407	87	.1989	101	.1600	115	.1252	129	.0994
60	.2779	74	.2357	88	.1983	102	.1552	116	.1245	130	.0978
61	.2749	75	.2327	89	.1948	103	.1546	117	.1225		
62	.2718	76	.2283	90	.1918	104	.1517	118	.1200		
63	.2684	77	.2270	91	.1890	105	.1490	119	.1175		

Frictional test of Leonard & Ellis' Valvoline Spindle Oil, on April 9 and 10, 1880. Rev. per min. 445.52, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 501.21 feet per minute. No. of sample 2. No. of trials 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTIO .	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.2026	78	.2029	92	.1686	106	.1345	120	.1109
51	....	65	.2541	79	.1997	93	.1641	107	.1327	121	.1102
52	....	66	.2488	80	.1964	94	.1618	108	.1311	122	.1091
53	....	67	.2449	81	.1947	95	.1587	109	.1294	123	.1049
54	....	68	.2409	82	.1919	96	.1566	110	.1278	124	.1036
55	....	69	.2381	83	.1897	97	.1537	111	.1263	125	.1032
56	....	70	.2362	84	.1875	98	.1522	112	.1247	126	.1024
57	....	71	.2318	85	.1855	99	.1502	113	.1192	127	.1005
58	....	72	.2307	86	.1816	100	.1476	114	.1168	128	.0991
59	....	73	.2244	87	.1786	101	.1445	115	.1155	129	.0967
60	.2882	74	.2206	88	.1758	102	.1417	116	.1136	130	.0956
61	.2775	75	.2173	89	.1739	103	.1389	117	.1125		
62	.2735	76	.2093	90	.1719	104	.1368	118	.1118		
63	.2666	77	.2059	91	.1708	105	.1365	119	.1112		

Frictional test of John P. Squire & Co., Lard Oil, on April 9 to 14, 1880. Rev. per min. 501.10, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces, 563.74 feet per minute. No. of sample 3. No. of trials 7.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	....	78	.2627	92	.2364	106	.2059	120	.1848
51	....	65	....	79	.2620	93	.2332	107	.2056	121	.1824
52	....	66	....	80	.2608	94	.2323	108	.2045	122	.1813
53	....	67	....	81	.2596	95	.2307	109	.2041	123	.1796
54	....	68	....	82	.2514	96	.2283	110	.2015	124	.1781
55	....	69	....	83	.2519	97	.2261	111	.1993	125	.1763
56	....	70	....	84	.2492	98	.2230	112	.1953	126	.1742
57	....	71	....	85	.2473	99	.2201	113	.1952	127	.1728
58	....	72	.2798	86	.2449	100	.2181	114	.1933	128	.1711
59	....	73	.2778	87	.2445	101	.2167	115	.1919	129	.1690
60	....	74	.2722	88	.2416	102	.2146	116	.1899	130	.1678
61	....	75	.2712	89	.2405	103	.2140	117	.1883		
62	....	76	.2688	90	.2377	104	.2117	118	.1877		
63	....	77	.2649	91	.2371	105	.2073	119	.1867		

Frictional test of No. 25 A Bleached Winter Sperm Oil, on April 15 and 16, 1880. Rev. per min. 500.92, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces, 563.53 feet per minute. Number of sample 4. No. of trials 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRIC-	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.1550	78	.1245	92	.1143	106	.1023	120	.0930
51	....	65	.1510	79	.1236	93	.1127	107	.1019	121	.0924
52	.2142	66	.1473	80	.1224	94	.1119	108	.1013	122	.0916
53	.2025	67	.1419	81	.1214	95	.1112	109	.1005	123	.0902
54	.1946	68	.1394	82	.1208	96	.1107	110	.0996	124	.0896
55	.1870	69	.1366	83	.1201	97	.1100	111	.0966	125	.0889
56	.1830	70	.1336	84	.1193	98	.1082	112	.0957	126	.0884
57	.1789	71	.1317	85	.1193	99	.1076	113	.0951	127	.0879
58	.1769	72	.1300	86	.1181	100	.1067	114	.0948	128	.0875
59	.1731	73	.1289	87	.1175	101	.1051	115	.0952	129	.0869
60	.1684	74	.1274	88	.1173	102	.1046	116	.0947	130	.0852
61	.1629	75	.1262	89	.1162	103	.1039	117	.0949		
62	.1614	76	.1251	90	.1151	104	.1038	118	.0938		
63	.1570	77	.1250	91	.1149	105	.1028	119	.0936		

Frictional test of 25 *B Sperm Oil, on April* 16, 1880. *Rev. per minute* 501.81, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 564.60 *feet per minute. No. of sample* 5. *No. of trials* 3.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.2115	78	.1551	92	.1350	106	.1079	120	.0889
51	....	65	.2070	79	.1507	93	.1327	107	.1054	121	.0886
52	....	66	.2003	80	.1514	94	.1310	108	.1027	122	.0879
53	....	67	.1969	81	.1500	95	.1289	109	.1004	123	.0870
54	....	68	.1904	82	.1487	96	.1276	110	.0994	124	.0865
55	....	69	.1876	83	.1480	97	.1253	111	.0981	125	.0807
56	....	70	.1836	84	.1466	98	.1238	112	.0957	126	.0856
57	....	71	.1815	85	.1481	99	.1222	113	.0935	127	.0849
58	....	72	.1796	86	.1444	100	.1217	114	.0922	128	.0841
59	....	73	.1776	87	.1417	101	.1201	115	.0920	129	.0840
60	.2278	74	.1743	88	.1400	102	.1176	116	.0917	130	.0834
61	.2229	75	.1726	89	.1388	103	.1155	117	.0909		
62	.2189	76	.1700	90	.1369	104	.1132	118	.0902		
63	.2141	77	.1608	91	.1358	105	.1108	119	.0894		

Frictional test of 25 *C Sperm Oil on April* 19, 1880. *Rev. per minute* 517.59, 5 *lbs pressure per sq. inch. Velocity of rubbing surfaces* 582.28 *feet per minute. No. of sample* 6. *No. of trials* 2.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50		64	.1721	78	.1365	92	.1195	106	.1098	120	.0926
51		65	.1694	79	.1357	93	.1194	107	.1080	121	.0905
52		66	.1678	80	.1347	94	.1188	108	.1065	122	.6906
53		67	.1663	81	.1335	95	.1187	109	.1051	123	.0903
54	.2243	68	.1646	82	.1320	96	.1182	110	.1044	124	.0888
55	.2163	69	.1596	83	.1301	97	.1174	111	.1031	125	.0883
56	.2118	70	.1563	84	.1287	98	.1167	112	.1018	126	.0870
57	.2020	71	.1513	85	.1251	99	.1167	113	.1010	127	.0864
58	.1939	72	.1461	86	.1237	100	.1170	114	.1007	128	.0846
59	.1913	73	.1416	87	.1229	101	.1167	115	.0999	129	.0834
60	.1885	74	.1406	88	.1220	102	.1149	116	.0989	130	.0827
61	.1845	75	.1400	89	.1211	103	.1140	117	.0968		
62	.1787	76	.1380	90	.1203	104	.1139	118	.0952		
63	.1756	77	.1363	91	.1201	105	.1118	119	.0946		

Frictional test of Downer's Light Spindle Oil, on April 17 *to* 19, 1880. *Rev. per min.* 500.21, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 562.73 *feet per minute. No. of sample* 7. *No. of trials* 4.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.1995	78	1532	92	.1248	106	.0985	120	.0761
51	....	65	.1956	79	.1514	93	.1233	107	.0956	121	.0760
52	.2789	66	.1900	80	.1483	94	.1215	108	.0922	122	.0757
53	.2617	67	.1862	81	.1449	95	.1199	109	.0895	123	.0753
54	.2591	68	.1825	82	.1432	96	.1175	110	.0874	124	.0743
55	.2535	69	.1793	83	.1411	97	.1161	111	.0848	125	.0739
56	.2385	70	.1760	84	.1388	98	.1144	112	.0836	126	.0728
57	.2347	71	.1731	85	.1369	99	.1131	113	.0809	127	.0721
58	.2214	72	.1704	86	.1347	100	.1113	114	.0800	128	.0715
59	.2213	73	.1679	87	.1323	101	.1093	115	.0793	129	.0706
60	.2186	74	.1642	88	1305	102	.1068	116	.0782	130	.0699
61	.2129	75	.1612	89	.1291	103	.1050	117	.0774		
62	.2098	76	.1580	90	.1274	104	.1033	118	.0767		
63	.2048	77	.1557	91	.1259	105	.1009	119	.0763		

Frictional test of Downer Oil Co., 32, *Ex. Machinery Oil, on April* 20 *and* 21, 1880. *Rev. per minute* 503.91, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 566.81 *feet per minute. No. of sample* 8. *No. of trials* 6.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.1248	78	.0969	92	.0846	106	.0688	120	.0540
51	.1598	65	.1219	79	.0959	93	.0828	107	.0665	121	.0534
52	.....	66	.1191	80	.0946	94	.0814	108	.0658	122	.0533
53	.1545	67	.1175	81	.0944	95	.0787	109	.0658	123	.0530
54	.1516	68	.1154	82	.0953	96	.0783	110	.0650	124	.0525
55	.1496	69	.1138	83	.0946	97	.0781	111	.0643	125	.0500
56	.1482	70	.1126	84	.0934	98	.0766	112	.0632	126	.0498
57	.1452	71	.1112	85	.0929	99	.0759	113	.0626	127	.0497
58	.1400	72	.1072	86	.0913	100	.0756	114	.0621	128	.0489
59	.1365	73	.1068	87	.0906	101	.0749	115	.0606	129	.0485
60	.1351	74	.1068	88	.0897	102	.0736	116	.0607	130	.0482
61	.1309	75	.1040	89	.0880	103	.0718	117	.0598		
62	.1289	76	.1015	90	.0877	104	.0706	118	.0591		
63	.1270	77	.0989	91	.0872	105	.0693	119	.0573		

Frictional test of Aiken & Swift Bleached Winter Sperm Oil, on April 22 and 23, 1880. Rev. per min. 456.09, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 513.10 feet per minute. No. of sample 9. No. of trials 6.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEF. OF FRICTION.
50	....	64	.1533	78	.1229	92	.1068	106	.0907	120	.0812
51	.2405	65	.1489	79	.1215	93	.1056	107	.0899	121	.0789
52	.2175	66	.1425	80	.1200	94	.1044	108	.0889	122	.0780
53	.2100	67	.1387	81	.1178	95	.1009	109	.0879	123	.0774
54	.1984	68	.1377	82	.1168	96	.0908	110	.0868	124	.0762
55	.1924	69	.1363	83	.1159	97	.0988	111	.0866	125	.0759
56	.1853	70	.1348	84	.1149	98	.0978	112	.0861	126	.0745
57	.1801	71	.1339	85	.1139	99	.0964	113	.0854	127	.0738
58	.1769	72	.1313	86	.1134	100	.0956	114	.0847	128	.0735
59	.1730	73	.1310	87	.1116	101	.0946	115	.0843	129	.0713
60	.1682	74	.1282	88	.1099	102	.0940	116	.0834	130	.0711
61	.1651	75	.1262	89	.1085	103	.0935	117	.0829		
62	.1599	76	.1250	90	.1083	104	.0923	118	.0825		
63	.1570	77	.1242	91	.1075	105	.0913	119	.0824		

Frictional test of Downer's Heavy Spindle Oil, on May 23, 1880. Rev. per minute 503.43, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 541.57 feet per minute. No. of sample 10. No. trials 3.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.2143	78	.1598	92	.1315	106	.1071	120	.0745
51	.....	65	.2101	79	.1568	93	.1295	107	.1049	121	.0735
52	.....	66	.2064	80	.1544	94	.1283	108	.1030	122	.0726
53	.....	67	.2032	81	.1527	95	.1272	109	.0999	123	.0718
54	.....	68	.1993	82	.1511	96	.1251	110	.0976	124	.0709
55	.....	69	.1947	83	.1488	97	.1238	111	.0952	125	.0698
56	.....	70	.1895	84	1469	98	.1219	112	.0930	126	.0693
57	.2520	71	.1853	85	.1449	99	.1203	113	.0898	127	.0687
58	.2408	72	.1818	86	.1426	100	.1181	114	.0861	128	.0683
59	.2364	73	.1768	87	.1406	101	.1156	115	.0826	129	.0678
60	.2322	74	.1717	88	.1386	102	.1140	116	.0782	130	.0671
61	.2283	75	.1682	89	.1371	103	.1125	117	.0773		
62	.2226	76	.1653	90	.1354	104	.1114	118	.0761		
63	.2185	77	.1623	91	.1331	105	.1099	119	.0755		

Frictional test of Leonard & Ellis White Spindle Oil, on April 23, 24 *and* 26, 1880. *Rev. per min.* 485.46, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 546.15 *feet per minute. No. of sample* 11. *No. of trials* 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	....	78	.2159	92	.1782	106	.1344	120	.1062
51	....	65	....	79	.2147	93	.1768	107	.1316	121	.1043
52	....	66	....	80	.2120	94	.1726	108	.1302	122	.1015
53	....	67	.2653	81	.2093	95	.1685	109	.1277	123	.0988
54	....	68	.2581	82	.2076	96	.1624	110	.1256	124	.0955
55	....	69	.2503	83	.2046	97	.1598	111	.1239	125	.0938
56	....	70	.2467	84	.2016	98	.1561	112	.1219	126	.0899
57	....	71	.2422	85	.1985	99	.1533	113	.1192	127	.0873
58	....	72	.2403	86	.1963	100	.1493	114	.1170	128	.0843
59	....	73	.2387	87	.1935	101	.1478	115	.1164	129	.0831
60	....	74	.2289	88	.1905	102	.1448	116	.1146	130	.0823
61	....	75	.2270	89	.1873	103	.1434	117	.1126		
62	....	76	.2243	90	.1868	104	.1382	118	.1106		
63	....	77	.2183	91	.1867	105	.1367	119	.1086		

Frictional test of Valvoline White Loom Oil, on April 26, 1880. *Rev. per minute* 512.51, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 576.59 *feet per minute. No. of sample* 12. *No. of trials* 2.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.2332	78	.1740	92	.1335	106	.1093	120	.0798
51	.....	65	.2273	79	.1701	93	.1322	107	.1077	121	.0785
52	.....	66	.2207	80	.1659	94	.1300	108	.1068	122	.0778
53	.....	67	.2168	81	.1600	95	.1287	109	.1054	123	.0770
54	.....	68	.2104	82	.1574	96	.1266	110	.1019	124	.0767
55	.2739	69	.2056	83	.1553	97	.1253	111	.1000	125	.0756
56	.2725	70	.2022	84	.1539	98	.1232	112	.0979	126	.0749
57	.2687	71	.1982	85	.1518	99	.1218	113	.0960	127	.0742
58	.2676	72	.1954	86	.1482	100	.1201	114	.0915	128	.0742
59	.2635	73	.1918	87	.1458	101	.1190	115	.0851	129	.0737
60	.2563	74	.1879	88	.1430	102	.1166	116	.0840	130	.0737
61	.2504	75	.1854	89	.1401	103	.1151	117	.0827		
62	.2450	76	.1825	90	.1383	104	.1135	118	.0821		
63	.2398	77	.1779	91	.1367	105	.1117	119	.0813		

Frictional test of Olney Bros. German Spindle Oil, on April 30, *and May* 1, 1880. *Rev. per min.* 481.41, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 541.59 *feet per minute. No. of sample* 13. *No. of trials* 6.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.2079	78	.1642	92	.1337	106	.1090	120	.0874
51	....	65	.2069	79	.1622	93	.1323	107	.1075	121	.0856
52	....	66	.2039	80	.1592	94	.1307	108	.1048	122	.0846
53	....	67	.1995	81	.1574	95	.1291	109	.1039	123	.0832
54	....	68	.1958	82	.1554	96	.1266	110	.1028	124	.0830
55	.2773	69	.1928	83	.1531	97	.1245	111	.1018	125	.0825
56	.2685	70	1889	84	.1504	98	.1230	112	.0999	126	.0820
57	.2567	71	1860	85	.1485	99	.1215	113	.0989	127	.0814
58	.2481	72	1808	86	.1463	100	.1190	114	.0967	128	.0803
59	.2451	73	.1774	87	.1445	101	.1173	115	.0959	129	.0793
60	.2403	74	.1737	88	.1424	102	.1151	116	.0957	130	.0783
61	.2354	75	.1713	89	.1408	103	.1126	117	.0935		
62	.2280	76	.1696	90	.1382	104	.1115	118	.0907		
63	.2196	77	.1671	91	.1352	105	.1104	119	.0889		

Frictional test of Downer Heavy Spindle Oil, on May 1 *and* 3, 1880. *Rev. per minute* 498.56, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 560.88 *feet per minute. No. of sample* 14. *No. of trials* 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.2184	78	.1143	92	.1377	106	.1106	120	.0800
51	.....	65	.2139	79	.1106	93	.1356	107	.1081	121	.0791
52	.....	66	.2094	80	.1081	94	.1341	108	.1061	122	.0783
53	.....	67	.2056	81	.1061	95	.1319	109	.1035	123	.0776
54	.....	68	.2016	82	.1035	96	.1305	110	.1007	124	.0768
55	.....	69	.1976	83	.1007	97	.1287	111	.0978	125	.0761
56	.....	70	.1936	84	.0978	98	.1263	112	.0948	126	.0757
57	.....	71	.1885	85	.0948	99	.1252	113	.0932	127	.0753
58	.....	72	.1844	86	.1497	100	.1233	114	.0878	128	.0748
59	.....	73	.1815	87	.1476	101	.1214	115	.0873	129	.0740
60	.2339	74	.1762	88	.1451	102	.1197	116	.0855	130	.0735
61	.2331	75	.1754	89	.1431	103	.1176	117	.0821		
62	.2267	76	.1719	90	.1414	104	.1150	118	.0808		
63	.2231	77	.1159	91	.1393	105	.1143	119	.0806		

Frictional test of Valvoline Machine Oil, on May 5, 1880. *Rev. per minute* 526.39, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 592.18 *feet per minute. No. of sample* 15. *No. of trials* 6.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.....	78	.2745	92	.2495	106	.2084	120	.1703
51	.....	65	.....	79	.2710	93	.2446	107	.2056	121	.1681
52	.....	66	.....	80	.2684	94	.2417	108	.2028	122	.1666
53	.....	67	.....	81	.2673	95	.2375	109	.1998	123	.1634
54	.....	68	.....	82	.2681	96	2364	110	.1941	124	.1618
55	.....	69	.....	83	.2661	97	.2336	111	.1937	125	.1586
56	.....	70	.....	84	.2612	98	.2308	112	.1903	126	.1552
57	.....	71	.....	85	.2620	99	.2283	113	.1886	127	.1536
58	.....	72	.....	86	.2591	100	.2243	114	.1862	128	.1502
59	.....	73	.....	87	.2586	101	.2188	115	.1829	129	.1478
60	.....	74	.....	88	.2574	102	.2169	116	.1803	130	.1461
61	.....	75	.....	89	.2548	103	.2155	117	.1783		
62	.....	76	.....	90	.2533	104	.2131	118	.1762		
63	.....	77	.276	91	.2510	105	.2109	119	.1727		

Frictional test of Olney Bros. A Spindle Oil, on May 8 *and* 10, 1880. *Rev. per minute* 512.27, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 576.30 *feet per minute. No. of sample* 16. *No. of trials* 7.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.1905	78	.1508	92	.1229	106	.1012	120	.0863
51	.....	65	.1947	79	.1486	93	.1211	107	.0998	121	.0860
52	.....	66	.1906	80	.1472	94	.1191	108	.0982	122	.0853
53	.....	67	.1863	81	.1434	95	.1172	109	.0965	123	.0846
54	.....	68	.1827	82	.1412	96	.1158	110	.0944	124	.0828
55	.....	69	.1774	83	.1393	97	.1140	111	.0937	125	.0819
56	.....	70	.1744	84	.1374	98	.1138	112	.0933	126	.0811
57	.....	71	.1717	85	.1354	99	.1123	113	.0931	127	.0801
58	.2550	72	.1682	86	.1333	100	.1103	114	.0929	128	.0799
59	.2367	73	.1646	87	.1318	101	.1087	115	.0917	129	.0797
60	.2222	74	.1621	88	.1297	102	.1075	116	.0913	130	.0791
61	.2167	75	.1592	89	.1277	103	.1062	117	.0901		
62	.2092	76	.1549	90	.1262	104	.1045	118	.0899		
63	.2075	77	.1530	91	.1245	105	.1033	119	.0882		

Frictional test of Aiken & Swift Unbleached Winter Sperm Oil, on May 12 and 13, 1880. Rev. per minute 495.54, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 557.48 feet per minute. No. of sample 17. No. of trials 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.1801	78	.1440	92	.1228	106	.1076	120	.0977
51	.....	65	.1754	79	.1423	93	.1217	107	.1074	121	.0973
52	.....	66	.1718	80	.1408	94	.1205	108	.1065	122	.0973
53	.....	67	.1676	81	.1399	95	.1199	109	.1044	123	.0970
54	.....	68	.1657	82	.1388	96	.1191	110	.1033	124	.0956
55	.....	69	.1639	83	.1376	97	.1189	111	.1023	125	.0957
56	.....	70	.1624	84	.1362	98	.1177	112	.1011	126	.0913
57	.....	71	.1582	85	.1343	99	.1165	113	.1009	127	.0888
58	.....	72	.1545	86	.1326	100	.1147	114	.1009	128	.0879
59	.....	73	.1531	87	.1310	101	.1139	115	.1012	129	.0873
60	.2054	74	.1511	88	.1303	102	.1128	116	.1023	130	
61	.2023	75	.1496	89	.1288	103	.1113	117	.1021		
62	.1917	76	.1475	90	.1278	104	.1097	118	.1013		
63	.1859	77	.1461	91	.1261	105	.1089	119	.0993		

Frictional test of Aiken & Swift Bleached Winter Sperm Oil, on May 14, 1880. Rev. per minute 490.87, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 552.23 feet per minute. No. of sample 18. No. of trials 4.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.1973	78	.1496	92	.1245	106	.1081	120	.0963
51	.....	65	.1924	79	.1474	93	.1232	107	.1077	121	.0960
52	.....	66	.1874	80	.1453	94	.1215	108	.1071	122	.0951
53	.....	67	.1824	81	.1434	95	.1206	109	.1060	123	.0943
54	.....	68	.1779	82	.1414	96	.1196	110	.1054	124	.0935
55	.....	69	.1734	83	.1394	97	.1186	111	.1045	125	.0926
56	.....	70	.1694	84	.1374	98	.1176	112	.1035	126	.0918
57	.....	71	.1654	85	.1354	99	.1166	113	.1026	127	.0910
58	.....	72	.1614	86	.1334	100	.1156	114	.1017	128	.0902
59	.....	73	.1574	87	.1314	101	.1146	115	.1008	129	.0894
60	.2134	74	.1534	88	.1294	102	.1136	116	.0999	130	.0886
61	.2094	75	.1494	89	.1274	103	.1126	117	.0990		
62	.2054	76	.1454	90	.1254	104	.1116	118	.0981		
63	.2014	77	.1414	91	.1234	105	.1106	119	.0972		

Frictional test of Downer Heavy Spindle Oil, on May 15, 1880. *Rev. per minute* 458.02, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 515.27 *feet per minute. No. of sample* 19. *No. of trials* 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	.2242	78	.1749	92	.1367	106	.1118	120	.1053
51	....	65	.2206	79	.1718	93	.1344	107	.1103	121	.1047
52	....	66	.2164	80	.1690	94	.1325	108	.1091	122	.1025
53	....	67	.2121	81	.1644	95	.1303	109	.1084	123	.1012
54	....	68	.2083	82	.1618	96	.1282	110	.1061	124	.0998
55	....	69	.2045	83	.1590	97	.1265	111	.1051	125	.1002
56	....	70	.2009	84	.1563	98	.1238	112	.1041	126	.1008
57	....	71	.1957	85	.1532	99	.1219	113	.1044	127	.0996
58	....	72	.1930	86	.1499	100	.1208	114	.1024	128	.0984
59	.2565	73	.1901	87	.1479	101	.1189	115	.1028	129	.0965
60	.2471	74	.1869	88	.1465	102	.1172	116	.1059	130	.0955
61	.2394	75	.1841	89	.1439	103	.1158	117	.1063	131	.0932
62	.2363	76	.1805	90	.1413	104	.1142	118	.1050		
63	.2322	77	.1776	91	.1388	105	.1130	119	.1048		

Frictional test of Downer Light Spindle Oil, on May 17 *and* 18, 1880. *Rev. per minute* 489.63, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 550.83 *feet per minute. No. of sample* 20. *No. of trials* 3.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.2012	78	.1565	92	.1260	106	.1100	120	.0995
51	.....	65	.1965	79	.1541	93	.1249	107	.1094	121	.0983
52	.....	66	.1923	80	.1512	94	.1230	108	.1086	122	.0966
53	.....	67	.1899	81	.1473	95	.1217	109	.1081	123	.0953
54	.....	68	.1857	82	.1456	96	.1190	110	.1081	124	.0884
55	.....	69	.1827	83	.1436	97	.1172	111	.1066	125	.0859
56	.....	70	.1788	84	.1425	98	.1152	112	.1057	126	.0846
57	.....	71	.1700	85	.1405	99	.1144	113	.1039	127	.0793
58	.....	72	.1743	86	.1390	100	.1132	114	.1026	128	.0823
59	.....	73	.1711	87	.1370	101	.1120	115	.1024	129	.0798
60	.2256	74	.1686	88	.1342	102	.1125	116	.1027	130	.0757
61	.2161	75	.1649	89	.1312	103	.1125	117	.1039		
62	.2089	76	.1616	90	.1296	104	.1121	118	.1022		
63	.2049	77	.1598	91	.1274	105	.1115	119	.1009		

Frictional test of Seal, on May 19-21, 1880. *Rev. per minute* 467.34, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 525.76 *feet per minute. No. of sample* 21. *No. of trials* 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.....	78	.2133	92	.1767	106	.1477	120	.1273
51	.....	65	.2027	79	.2108	93	.1749	107	.1450	121	.1221
52	.....	66	.2025	80	.2071	94	.1723	108	.1439	122	.1210
53	.....	67	.2615	81	.2045	95	.1705	109	.1428	123	.1186
54	.....	68	.2487	82	.2019	96	.1679	110	.1414	124	.1168
55	.....	69	.2429	83	.1983	97	.1658	111	.1390	125	.1156
56	.....	70	.2406	84	.1951	98	.1644	112	.1367	126	.1143
57	.....	71	.2374	85	.1936	99	.1623	113	.1355	127	.1135
58	.....	72	.2334	86	.1908	100	.1608	114	.1329	128	.1128
59	.....	73	.2303	87	.1892	101	.1586	115	.1310	129	.1126
60	.....	74	.2257	88	.1857	102	.1562	116	.1296	130	.1114
61	.....	75	.2231	89	.1838	103	.1549	117	.1278		
62	.....	76	.2195	90	.1818	104	.1520	118	.1258		
63	.....	77	.2147	91	.1788	105	.1496	119	.1249		

Frictional test of Alexander Boyd & Sons Neatsfoot Oil, on May 22, 1880. *Rev. per minute* 526.15, 5 *lbs. pressure per sq. inch. Velocity of rubbing surfaces* 591.92 *feet per minute. No. of sample* 22. *No. of trials* 4.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	....	78	....	92	.2710	106	.2243	120	.1862
51	....	65	....	79	....	93	.2687	107	.2204	121	.1826
52	....	66	....	80	....	94	.2648	108	.2164	122	.1805
53	....	67	....	81	....	95	.2581	109	.2141	123	.1779
54	....	68	....	82	....	96	.2530	110	.2125	124	.1755
55	....	69	....	83	....	97	.2517	111	.2106	125	.1729
56	....	70	....	84	....	98	.2499	112	.2081	126	.1698
57	....	71	....	85	....	99	.2467	113	.2053	127	.1689
58	....	72	....	86	....	100	.2427	114	.2019	128	.1680
59	....	73	....	87	....	101	.2395	115	.1997	129	.1672
60	....	74	....	88	....	102	.2377	116	.1972	130	.1654
61	....	75	....	89	....	103	.2343	117	.1948		
62	....	76	....	90	....	104	.2293	118	.1913		
63	....	77	....	91	....	105	.2277	119	.1888		

Frictional test of Unknown [Mixed] Oil, on May 24, 1880. Rev. per minute 490.46, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 571.77 feet per minute. No. of sample 23. No. trials 4.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.....	78	.2083	92	.1773	106	.1470	120	.1231
51	.....	65	.2652	79	.2055	93	.1756	107	.1455	121	.1221
52	.....	66	.2623	80	.2028	94	.1735	108	.1432	122	.1206
53	.....	67	.2537	81	.2001	95	.1712	109	.1410	123	.1192
54	.....	68	.2505	82	.1972	96	.1689	110	.1395	124	.1178
55	.....	69	.2448	83	.1950	97	.1664	111	.1386	125	.1164
56	.....	70	.2410	84	.1927	98	.1645	112	.1365	126	.1147
57	.....	71	.2376	85	.1907	99	.1625	113	.1340	127	.1132
58	.....	72	.2316	86	.1884	100	.1608	114	.1321	128	.1117
59	.....	73	.2277	87	.1873	101	.1594	115	.1304	129	.1102
60	.....	74	.2225	88	.1849	102	.1594	116	.1290	130	.1084
61	.....	75	.2189	89	.1827	103	.1539	117	.1277		
62	.....	76	.2155	90	.1806	104	.1514	118	.1262		
63	.....	77	.2115	91	.1789	105	.1492	119	.1244		

Frictional test of Unknown [Mixed] Oil, on May 24 and 25, 1880. Rev. per minute 471.86, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 530.84 feet per minute. No. of sample 24. No. of trials 4.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	....	78	.1845	92	.1534	106	.1272	120	.1051
51	....	65	.2469	79	.1825	93	.1512	107	.1254	121	.1038
52	....	66	.2407	80	.1800	94	.1489	108	.1240	122	.1020
53	....	67	.2308	81	.1775	95	.1468	109	.1232	123	.1008
54	....	68	.2226	82	.1754	96	.1446	110	.1205	124	.0987
55	....	69	.2153	83	.1738	97	.1438	111	.1192	125	.0975
56	....	70	.2101	84	.1710	98	.1416	112	.1177	126	.0955
57	....	71	.2066	85	.1693	99	.1393	113	.1159	127	.0948
58	....	72	.2019	86	.1674	100	.1377	114	.1143	128	.0923
59	....	73	.1983	87	.1655	101	.1363	115	.1128	129	.0902
60	....	74	.1956	88	.1608	102	.1343	116	.1115	130	.0885
61	....	75	.1931	89	.1593	103	.1323	117	.1096		
62	....	76	.1890	90	.1575	104	.1309	118	.1076		
63	....	77	.1875	91	.1555	105	.1292	119	.1063		

Frictional test of Unknown [Mixed] Oil, on May 25 and 26, 1880. Rev. per minute 485.06, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 545.89 feet per minute. No. of sample 25. No. trials 4.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	.....	64	.....	78	.1677	92	.1335	106	.1086	120	.1021
51	.....	65	.2345	79	.1646	93	.1315	107	.1073	121	.0997
52	.....	66	.2185	80	.1622	94	.1298	108	.1059	122	.0971
53	.....	67	.2059	81	.1600	95	.1278	109	.1046	123	.0964
54	.....	68	.2033	82	.1575	96	.1255	110	.1035	124	.0947
55	.....	69	.1975	83	.1549	97	.1243	111	.1024	125	.0933
56	.....	70	.1918	84	.1517	98	.1213	112	.1015	126	.0912
57	.....	71	.1888	85	.1494	99	.1205	113	.1014	127	.0901
58	.....	72	.1851	86	.1472	100	.1190	114	.1016	128	.0887
59	.....	73	.1809	87	.1439	101	.1172	115	.1017	129	.0867
60	.....	74	.1784	88	.1415	102	.1153	116	.1012	130	.0854
61	.....	75	.1751	89	.1396	103	.1144	117	.1021		
62	.....	76	.1726	90	.1378	104	.1128	118	.1026		
63	.....	77	.1702	91	.1354	105	.1101	119	.1034		

Frictional test of Downer Oil Co., Champion Oil, on May 27, 1880. Rev. per minute 508.69, 5 lbs. pressure per sq. inch. Velocity of rubbing surfaces 572.27 feet per minute. No. of sample 28. No. of trials 5.

TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.	TEMPERATURE.	COEFF. OF FRICTION.
50	....	64	....	78	.2426	92	.1945	106	.1579	120	.1264
51	....	65	....	79	.2373	93	.1921	107	.1556	121	.1241
52	....	66	....	80	.2343	94	.1899	108	.1534	122	.1220
53	....	67	....	81	.2306	95	.1881	109	.1511	123	.1204
54	....	68	....	82	.2276	96	.1851	110	.1490	124	.1183
55	....	69	....	83	.2227	97	.1810	111	.1457	125	.1168
56	....	70	....	84	.2197	98	.1785	112	.1447	126	.1150
57	....	71	.2871	85	.2160	99	.1762	113	.1416	127	.1131
58	....	72	.2730	86	.2128	100	.1732	114	.1392	128	.1115
59	....	73	.2646	87	.2107	101	.1709	115	.1370	129	.1098
60	....	74	.2577	88	.2070	102	.1687	116	.1343	130	.1083
61	....	75	.2536	89	.2041	103	.1667	117	.1325		
62	....	76	.2498	90	.2007	104	.1637	118	.1308		
63	....	77	.2465	91	.1977	105	.1608	119	.1286		

Fluidity Experiments. Valvoline Spindle Oil. Leonard & Ellis. Sample 2.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	70.	120	124.25	90	230.	60	501.
140	91.5	110	151.75	80	290.	50	646.25
130	104.25	100	185.50	70	345.		

Fluidity Experiments. Lard Oil. John P. Squire. Sample 3.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	150.	120	242.5	90	428.	60	854.
140	172.	110	285.	80	524.		
130	203.5	100	345.	70	682.		

Fluidity Experiments. 32° *Extra Machinery Oil. Downer Oil Co. Sample* 8.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	43.	120	59.	90	90.	60	166.
140	48.	110	66.	80	106.75	50	210.
130	52.50	100	76.	70	130.75		

Fluidity Experiments. Bleached Winter Sperm Oil. Aiken & Swift. Sample 9.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	79.	120	115.5	90	182.	60	334.
140	88.5	110	130.	80	219.		
130	99.	100	155.	70	265.5		

Fluidity Experiments. Valvoline White Spindle Oil. Leonard & Ellis. Sample 11.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	93.50	130	125.50	110	186.5	90	284.
140	106.	120	150.5	100	229.	80	353.

Fluidity Experiments. Valvoline White Loom Oil. Leonard & Ellis. Sample 12.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	68.	120	103.	90	190.	60	400.
140	78.	110	127.	80	234.		
130	90.	100	152.	70	302.		

Fluidity Experiments. German Spindle Oil. Olney Bros. Sample 13.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	72.	120	116.	90	208.	60	452.
140	89.5	110	145.5	80	264.		
130	94.5	100	168.	70	363.		

Fluidity Experiments. Heavy Spindle Oil. Downer Oil Co. Sample 14.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
140	66.	110	122.5	80	231.5		
130	76.	100	149.	70	304.		
120	106.	90	183.	60	399.		

Fluidity Experiments. Valvoline Machinery Oil. Leonard & Ellis. Sample 15.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	166.	120	297.	90	605.	60	134.9
140	198.	110	368.	80	781.		
130	242.	100	471.	70	105.9		

Fluidity Experiments. A Spindle Oil. Olney Bros. Sample 16.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	60.5	120	91.75	90	159.	60	345.
140	69.75	110	106.50	80	210.5		
130	79.	100	129.	70	276.		

Fluidity Experiments. Bleached Winter Sperm Oil. Aiken & Swift. Sample 18.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	82.	120	121.	90	197.7	60	345.5
140	91.75	110	137.	80	226.75		
130	105.	100	161.5	70	281.75		

Fluidity Experiments. Light Spindle Oil. Downer Oil Co. Sample 20.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	62.	120	93.	90	161.	60	323.
140	69.	110	112.	80	203.	50	416.
130	79.	100	131.	70	250.		

Fluidity Experiments. Seal Oil. Sample 21.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	121.75	120	185.5	90	315.5		
140	137.	110	220.	80	469.5		
130	159.	100	285.5	70	578.5		

Fluidity Experiments. Neatsfoot Oil. Alexander Boyd & Sons. Sample 22.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	175.	120	293.5	90	531.5		
140	212.	110	351.	80	681.		
130	245.5	100	434.	70	840.		

Fluidity Experiments. Champion Oil. Downer Oil Co. Sample 28.

Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.	Temperature.	Seconds required to empty pipette.
150	95.	120	162.5	90	306.	60	726.
140	113.	110	196.5	80	413.		
130	131.	100	244.5	70	554.		

Determination of the Comparative Dimensions of Ultimate Molecules; and Deduction of the Specific Properties of Substances. By William A. Norton, of New Haven, Conn.

In papers on topics pertaining to Molecular Physics, read before the National Academy of Sciences, and subsequently published in the American Journal of Science,[1] I have presented an alternative conception of the constitution of an ultimate molecule to that commonly entertained, viz.: that, instead of being a group of similar atoms endued with inherent forces, of repulsion at the more minute distances and attraction at all greater distances, it consists of a single atom surrounded by an envelope of electric ether, immersed in and permeated by the luminiferous ether condensed upon the atom; and that this investiture with an ethereo-electric atmosphere gives to the atom its panoply of power. I have shown that this conception leads to that of the operation of forces of attraction and repulsion, emanating from different centres, and varying according to the law of inverse squares; and given a mathematical expression for the result of the joint action of these forces, which I term the *Force of Effective Molecular Action*. I have also shown that the general mechanical laws and properties of bodies may be deduced from the force thus precisely expressed; that numerical determinations of tenacities, etc., may be obtained, in close correspondence with experimental results,—that every substance has its own precise formula and corresponding curve of effective molecular action, capable of determination, which represents all its mechanical properties, general and specific,—and that to the liquid and gaseous states of a substance answer particular curves which sustain numerical tests no less successfully than that which represents the solid state.[1]

It has also been made evident that upon the conception adopted, the ultimate molecule should have the property of *variability*, in its dimensions and forces, which seems to furnish a key to the satisfactory explanation of a variety of phenomena; as the set of materials after a force of stress has been applied to them, viscosity, and numerous other consequences of imperfect elasticity, and possibly also of chemical changes.[2]

In the present paper I propose to consider the conditions of

[1] In Journal of Science, May and June, 1870.
[2] In Journal of Science, March, 1870.

equilibrium of individual molecules, and undertake the theoretical determination of the comparative dimensions and mechanical features of the molecules of different substances, as well as the mathematical deduction of the comparative properties and relations of substances, from the mechanical, physical, and chemical points of view.

In giving a detailed exposition of the *mechanical constitution of a molecule*, we have to consider:—

1. The *Nucleus*, or atom, surrounded by an ethereal atmosphere condensed upon it by the attraction, or virtual attraction, of the nucleus.

2. The *Electric Envelope*, consisting of a less subtle ether, the atoms of which are at the same time attracted by the nucleus and repelled by the condensed ether resting on the nucleus. Each atom is solicited by an effective force equal to the difference of these individual forces. This is represented by the expression

$$u = \frac{C m}{R^2} - \frac{C' m}{r^2 (R-r)^2} \qquad (1)$$

in which m = mass of nucleus = atomic weight of substance;
C = coëfficient of attraction of nucleus; C' = coeff. of repulsion of ethereal atmosphere;
R = distance from centre of nucleus to any point of envelope;
r = distance from centre of nucleus to centre of ethereal repulsion;
u = effective force soliciting atom of envelope.

The equilibrium obtains, at all points of the envelope between the entire compressing action due to this effective force and the elastic resistance of the compressed ether. If C' be expressed in fractional parts of C regarded as unity, the expression for the effective force becomes

$$u = m \left(\frac{1}{R^2} - \frac{C'}{r^2 (R-r)^2} \right) \qquad (2)$$

Let $C_1 = \sqrt{C'}$, and $C_2 = \sqrt[3]{C'}$; then for the zero value of u, $R = \frac{r^2}{r - C_1}$; and for max. value of u (or u'), $R' = \frac{r_2^2}{r_2^2 - C_2}$.

For the *rate of variation* of u with R, we have

$$v = \frac{du}{dR} = 2m \left(-\frac{1}{R^3} + \frac{C'}{r^2} \times \frac{1}{(R-r)^3} \right) \qquad (3)$$

The equilibrium here considered is a *dynamical* one. It is a fundamental hypothesis of the theory, that the attractive and repulsive forces exerted on the envelope of a molecule consist of incessantly recurring impulses (or may be so represented). Between the contiguous atoms of the entire ethereo-electric atmos-

phere a *statical* force of mutual repulsion is, at the same time, in operation, by virtue of which its two constituent ethers are media capable of opposing an elastic resistance to compression, and of transmitting impulses by wave propagation.

The first fact to be noted with regard to the dynamical action exerted upon a molecular envelope, by the nucleus and ether immediately surrounding it, is that it originates inward acting, or attractive waves, which are thence propagated indefinitely outward. The intensity of the wave force thus developed, at any point of the envelope, depends on the inequality of action exerted on its constituent atoms lying at slightly different distances from the nucleus; and thus upon the rate of variation of u regarded as a function of R. It may thus be regarded as proportional to $\frac{du}{dR}$. For this, as we have already seen, we have the equation

$$\frac{du}{dR} = 2m\left(-\frac{1}{R^3} + \frac{C'}{R^2} \times \frac{1}{(R-r)^3}\right).$$

This has its maximum positive value at the distance, R, at which $u = 0$; and decreases as R increases until the maximum value of u is reached, where it becomes zero. It may be seen, then, that the system of attractive waves under consideration will have a resultant centre lying in the vicinity of the lower or inner surface of the envelope. The entire action of this wave system should be nearly proportional to the value of $\frac{du}{dR}$ at the zero point of u, which we will call v'. In former papers I have denoted the coëfficient of this attractive action of one molecule on another, by n.[3] We have then $n = \text{const.} \times v'$.

But the dynamical repulsive action exerted between the constituent atoms of the envelope should also originate a system of repulsive waves. The resultant centre for these should lie toward the outer surface of the effective envelope. The coëfficient of this wave system I have denoted by m. Regarding this, for the present, as constant for all molecules at the same temperature, we have $\frac{n}{m} = \text{const.} \times v'$.

Another system of repulsive waves should be developed, in the luminiferous ether below the envelope, by the dynamic collapses of the envelope. These will be propagated by the luminiferous ether. The expression given in former papers for *the force of effective molecular action*,[3] represents the result of the joint action of the three systems of waves, on a contiguous molecule. It will be

[3] See Journal of Science, May 1879, p. 346.

seen, then, that we have a mathematical relation established between a definite mechanical feature of the molecular envelope, indicated by v', and the ratio $\frac{n}{m}$ upon which the force, or representative curve of effective molecular action, depends. We shall see hereafter that definite relations may also be derived between the mechanical condition of the molecule and the *physical* and *chemical* properties which it exhibits.

Let us now take up the question of the *determination of the comparative dimensions of the ultimate molecules of different substances.* If the nuclei (*i. e.*, the single central atoms) of molecules are all of the same density, the radius, r', of any nucleus, and approximately r in our formulæ, should be proportional to the $\sqrt[3]{m}$, or $\sqrt[3]{\text{at. weight}}$. But the marked differences of property, exhibited by certain elements of nearly the same atomic weight, show that their constituent atoms must differ in some other particular than mass, or weight. The simplest and most probable supposition that can be made is that they differ in size for the same mass, or in other words in *density.* This theoretical conclusion accords with the speculative hypothesis I ventured to suggest several years since, from other considerations, that the atoms of bodies might be masses of condensed ether, but does not necessitate this supposition. A similar conception of the probable constitution of the atom has been propounded by Sir William Thomson, and other eminent physicists.

Adopting the idea that the nuclei of molecules may vary in density, the value of C′ may vary from one element to another by reason of the varying density of the molecular nucleus, and also by reason of the varying distance between the nucleus and the envelope; since the mass of ether acting repulsively on the envelope will vary with this distance. If the comparative densities of the atoms of elements were known, in addition to their comparative weights, or masses, we could by means of our formulæ deduce their dimensions, and also the diverse capabilities of action of the elements considered; and could test our theory in a direct and decisive manner. In the absence of this à priori knowledge the only possible mode of proceeding is to deduce from certain recognized properties of any element under consideration, as experimentally determined, the values of r, r', C′, R, R′, and v', and all the mechanical features of the molecule; and from these derive, if possible, expressions for the other comparative properties of the

element, and then compare the numerical results obtained with the results of actual experiment. The process of calculation actually pursued is this. The experimental data assumed are the molecular volume of the substance, and its tenacity, or instead, its coëfficient of elasticity (E). If the substance be considered in the liquid state, and at the boiling point, the molecular volume alone suffices. The tenacity, or coëfficient of elasticity, makes known the comparative ratio $\frac{n}{m}$, and the molecular curve, and thus the comparative value of v' by means of the relation already given. From the molecular volume we derive the radius of the molecular volume, and thence the radius, R′, of the effective molecule, by means of the neutral distance derivable from the value of $\frac{n}{m}$. We then have, to find r and C′, the equations

$$C' = \frac{r^2(R-r)^3}{R'^3} \quad (a); \quad \left(1-\frac{\sqrt{C'}}{r}\right)^3\left(-\frac{1}{r^2}+\frac{1}{r\sqrt{C'}}\right) = v_1 r \quad (b)$$

$$\text{and } v_1 = \frac{v'}{2 \times \text{at. weight}} \quad (c)$$

The numerical results obtained for a variety of substances are given in Table I. For the designations of the captions of the columns, v', c', R′, R, r, r', and u', see *ante*. The values of x are the exponents of the power to which the atomic weight is to be raised to produce r, and the values of x' are the corresponding exponents answering to the values of r'. The comparative values of r', R′, and v', for the elements, are graphically represented in Figs. 1 and 2. The comparative densities of the atoms, or nuclei of the molecules, are also shown in Fig. 2.

Upon the present theory the heat pertaining to a molecule consists in the vibratory movement of its envelope, or the correlative pulsation of the ether at the surface of the nucleus; for the two are related as action and reaction. This conception gives for the specific heat of a molecule the expression,

$$\text{Specific heat} = \frac{r'^2}{v\,(R-r')^2} \times (R'-R) \times u' \quad (4)$$

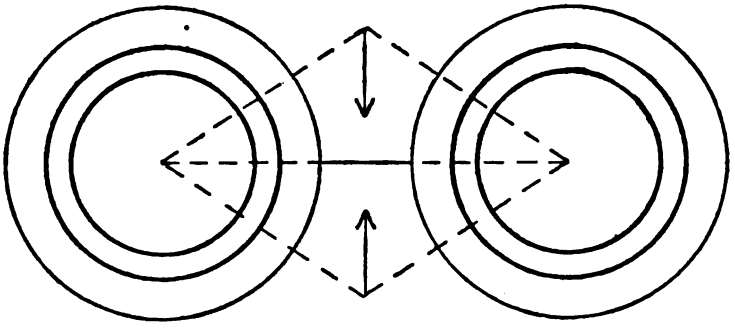
where r' denotes the radius of the actual nucleus, under the supposition that the ethereal heat-pulses are reflected from the surface of the nucleus. I find that this expression gives the value of the molecular heat obtained by experiment, if a value of r' be taken somewhat less than that of r. We may assume, then, that the radius of the actual nucleus is equal to the value of r' determined, or is somewhat less than this. The results of the calculations of the molecular heat are given in Table II. The assumed values of r' are given in Table I.

Before proceeding farther with the calculations proposed I will consider briefly two or three topics which have a bearing on the deductions that remain to be made.

(1). *Indefinite extension of a single isolated molecule.* That no theoretical limit would exist, to the extension of the envelope of a single isolated molecule, appears from the value of u (Eq. 2). This increases from zero to a maximum, and then decreases indefinitely.

(2). *Condition of the electric ether between molecules, as to tension and density.* On examining Fig. 3, it will be seen that

FIG. 3.



the effective attraction exerted by two molecules, on the electric ether lying to one side of the line of their centres, will give rise to an effective force directed toward this line and condensing the ether upon it. At the surface of a body all the pairs of contiguous molecules will exert a similar compressing action, directed inward and thus determining a certain density and tension of the electric ether occupying the interstitial spaces in the interior of the body.

(3). The relation between the tension of the interstitial electric ether and that of the luminiferous ether between the molecular envelopes and the nuclei. Between these tensions an equilibrium should subsist, or tend to subsist. *If this equilibrium should be in any way disturbed, the immediate consequence would be either a contraction or expansion of the molecular envelopes, with attendant electric or ethereal wave movements.* The tendency of such wave movements, emanating from the disturbed molecules, should be to urge the envelopes of neighboring molecules either toward or from

their nuclei, according as they occur in the interstitial electric ether or in the luminiferous ether. Such disturbances of equilibrium of tension, with these attendant results, may result from a change of either of the two individual tensions, ethereal or electric. The ethereal tension may be increased by the access of heat pulses. The interstitial electric tension may be altered, by a surface contact with a substance of greater or less electric tension, or by the mixture of liquids or gases, in which the electric tension, u, of the molecules is unequal.

We are now in a position to make definite determinations of the electric and chemical relations of substances. From what has been stated it may be seen that if the ethereal tension below a molecular envelope be increased, or the interstitial electric tension be diminished, an instantaneous effective force will come into operation that will tend to urge the envelope outward, and initiate wave impulses in the interstitial electric ether. The force of electric tension thus developed I will designate by f. The *energy of the electric movement* originated by this force I denote by f'' and call the *electro-motive force*. For f I obtain the following expression.

$$f = \frac{R^3}{(R - r')^2} \times u' \times \text{neutral distance.} \qquad (5)$$

$$\text{Let } f' = \frac{f}{v'} \text{ then } f'' = f' \times \text{molecular volume.} \qquad (6)$$

The values of f, f', and f'', computed for the elements included in Table I, are given in Table II. The comparative values of f, as well as of f'', for these elements, are also represented in Fig. 4. It is to be observed that the substances marked with an * are taken in the molecular condition in which they exist in certain liquid compounds at the boiling point. Thus the quantities given in the Table for oxygen answer to oxygen molecules in water at its boiling point. The same is true of Table I. By the neutral distance, in Eq. (5), is meant the distance between two contiguous molecules of a body, in its ordinary undisturbed state. It is the value of x, in the expression for the force, F, of effective molecular action, at which $F = 0$. (See Journal of Science, May, 1879, pp. 346 and 347. The neutral distance is represented by Oa in the molecular curve on p. 347.)

When the molecules of two substances brought into contiguity act on each other chemically, the *process* consists in an expansion

of the one set of molecular envelopes and an attendant contraction of the other set. That it may be initiated it is essential that the values of f should be unequal for the two substances. The molecular envelopes of the substance which has the larger value of f will expand, and those of the other contract. The former may be termed electro-positive with regard to the latter; and the latter electro-negative with regard to the former. In the chemical process the electric movement is from the former to the latter. Thus when zinc combines with the oxygen of water in the voltaic cell, the envelopes of the zinc molecules expand, and those of the oxygen molecules contract, and the electric movement is from the former to the latter. The final result is that the ratio $\frac{n}{m}$ is increased in value for the oxygen molecules, and diminished for the zinc molecules, and thus brought approximately to the same value for the two sets of molecules (see Journal of Science, May, 1879, p. 346). The *process of "chemical combination"* consists in a modification of the condition of molecular envelopes (by expansion and attendant contraction) by which the two sets of molecules are brought into correspondent conditions (whether answering to the solid, liquid, or gaseous state). The *initiating cause* is an inequality in the values of the force f, for the two substances, and the *effective force* in operation is the excess in the intensity of f for the electro-positive substance over that for the electro-negative. The *energy* of the process of change depends on the excess in the value of f'' for the former substance over that for the latter.

It is important to observe that f and f'' have not certain fixed values for each particular substance. The value of each, for any substance, varies with the temperature and with the state of aggregation. The chemical relations of substances may thus vary materially with the temperature at which they are brought together. These are definitely shown by the comparative values of f, at the temperature considered. They are, however, liable to material modification by the proximity of other substances whose forces, f, come into simultaneous operation. The case of contact along a definite extent of surface (as in the contact of a zinc plate with the water in a voltaic cell) is to be distinguished from that of the mixture of two liquids. In the former case the distance between the molecules of each substance becomes a modifying factor; and it is conceived that a nearer approximation to the chemical relations will be obtained by dividing f and f'' by $\sqrt{\text{neut. dist.}}$ (See columns 9 and 10, Table II).

As to the *effect of heat to promote chemical combination* it may be readily seen that its general tendency is to raise the value of f. It may thus promote oxidation and combustion by raising the value of f for the combustible above that which obtains for oxygen. Heat, it is to be observed, in such cases, brings the molecules into the condition in which an effective force, f, comes into operation. The heat subsequently developed,— the "heat of combination,"— is the result of the electro-motive energy that comes into play. It is also in part due to the diminution of the neutral distance between the molecules whenever a lower state of aggregation is reached.

The *chemical relations of substances to oxygen* are shown by the comparative values of f. On consulting Table II it will be seen that the values of f for the metals, lithium, sodium, and potassium, are greater than for oxygen in the condition in which it exists in water at the boiling point, and accordingly these metals should be electro-positive to the oxygen, and have a strong tendency to combine with it when brought in contact with water. For the other metals mentioned in the table the value of f is less than for oxygen, and the electro-positive state, indicated by the value of f, requires to be exalted by heat before such combination can take place; or else the combination must be promoted by favorable conditions afforded by the presence of other substances. The value of f is the lowest for platinum. The large values of f'' for the alkaline metals give intimation of great energy in the process of combination with oxygen. The general chemical relations of the elements to oxygen may be seen at a glance by examining Fig. 4. The theoretical chemical relations of the elements to hydrogen, or any other individual element, may be obtained in a similar manner from Table II, or by examining Fig. 4.

In the direct union of two gases, heat, or the electric spark, is the inciting cause of the transformation of the molecules.

The proportions in which two substances may combine should depend on their comparative values of f, and the range of variation which these may experience under the influence of heat and other external relations. It is to be observed here that of two substances, A and B, A may be electro-positive to B under certain conditions of temperature, etc., and under other conditions electro-negative to it; since A may have a larger value of f than B in the one case, and a smaller value in the other case. By examining the ascertained values of f for the elements in our Table we may

get an insight into the physical peculiarities correspondent to the terms *univalence*, *bivalence*, etc., used by chemists; but this topic cannot now be enlarged upon. We will only state a general principle that here becomes applicable, viz., that in the process of a chemical combination that molecule which expands has its value of f reduced thereby, and that which contracts has the value correspondingly augmented.

Mendelejeff's Law of the Periodicity of the Elements is clearly indicated in the results of our calculations. Compare the values of f, as well as those of v', given in Tables I and II, and represented in Figs. 2 and 4. The values of f give intimation of chemical relations; and those of v' of mechanical relations—but these are not wholly dependent on v'. The law would be more completely indicated if we were to insert the approximate values of f and v' for certain substances, as fluorine, silicon, etc., for which the data necessary for accurate calculations have not yet been obtained. The theoretical explanation of this law is, that it results from periodic fluctuations in the densities of the nuclei of the ultimate molecules (*i. e.*, of the atoms of the elements). On comparing Figs. 2 and 4, the general correspondence between these fluctuations of density and those of the values of v' and f, may be seen—it being observed that an increase in the density of the atom should tend to increase v' and diminish f.

Another important theoretical deduction that may be made is that of an expression for the comparative *hardness of substances*. This should depend on the greater or less susceptibility of the molecular envelopes to compression in the direction of the compressing force and to expansion in the lateral direction. From this point of view we have for the theoretical expression for the comparative hardness of substances, $\frac{f'}{f''}$, or $\frac{f'}{f' \times \text{molec. vol.}'}$, or $\frac{1}{\text{molec. vol.}}$. Now we are informed (Phil. Mag., Oct., 1879), that Bettone has determined the hardness of substances by finding the time required for a steel drill to penetrate to a certain depth, and has by this means shown that in fact the hardness of an element is almost precisely inversely proportional to its molecular volume.

I can only briefly allude to another theoretical determination by means of which an additional test can be applied to the general theory. It is that of *the points of fusion of* the elements whose molecular status has been determined. The essential criterion of the point of fusion of an element is that condition of expansion and vibration of the molecular envelopes at which the ratio $\frac{n}{m}$ for

the diagonal forces of the elementary cube, becomes 4.92, or the molecular curve for these forces becomes tangent to the axis of distances.[4] The temperature of the point of fusion above the absolute zero is accordingly assumed to be proportional to the amount of heat energy required to raise the molecules to this condition. From this point of view it may be seen that if we designate by T the rise of temperature, from the absolute zero to the point of fusion, we shall have $T = \frac{\text{const.}}{d.v.f''}$; d representing the neutral distance between the molecules, v the molecular volume, and f'' the electro-motive force. In a few instances, for reasons that cannot be adequately given, the calculations have been made by the formula $T = \frac{\text{const.}}{f'.d.(7)^2}$; 7 being the molecular volume of iron and $f' = \frac{f}{v}$. Now, let T_1 = value of T for wrought iron, as given by the formula, T_0 = temperature, in Fahrenheit degrees, above absolute zero, at the point of fusion of wrought iron, and T′ the corresponding temperature for the substance considered; and we have $T' = T_0 \frac{T}{T_1}$, and for the point of fusion, P°, $P° = T' - 459°$. In the calculations made I have taken $T_0 = 4400°$ F., which is a little less (141° F.) than the value for pure iron, given by Wedding (a standard German authority), and about the same amount, greater than the value assigned by Carnelly (Phil. Mag., Oct., 1879).

The results of the calculations are given in Table II. It is not claimed that the above formulæ are exact, nor can it reasonably be supposed that all the molecular data employed have been accurately determined in this first attempt in an entirely new field of investigation. It would seem, however, that the calculated results are sufficient approximations to the experimental determinations to lend an additional support to the general theory, and justify the assumptions made in the process of calculation; as well as furnish a desirable verification of previous molecular computations.

In bringing this paper to a conclusion, I would ask attention to the fact that in this and my former papers, to which reference has been made, definite numerical results have been obtained in several departments of Molecular Physics in which no exact investigations have been found possible from the ordinary molecular point of view, and precise experimental tests have been applied. If these results do not suffice to induce the reader to abandon his own molecular standpoint for that which is here taken, he may still be

[4] See Journal of Science, May, 1879, pp. 346-7-8, and June, 1879, p. 447.

not unwilling to receive the molecular theory that has been discussed as *a scheme of representation*, and as a definite *working hypothesis* furnishing a basis for numerical computations in mechanical, physical and chemical fields of research. Let it here be understood that the point of view we have assumed does not call upon us to discard any of the established principles of the *mechanical theory of heat*, nor to abandon the *doctrine of energy*. For the basis of the present molecular theory is as truly dynamical as is that of the alternative theory commonly held. It is the dynamical system alone that is different.

But little assured progress has hitherto been made toward a physical theory of chemical phenomena. It is true, diligent use has been made of the cumbrous hypothesis of compound molecules, capable of breaking up into a host of independent atoms, at a summons issuing from the depths of the unknown, and then marshalling themselves anew into other complex groups; but no definite conception has yet been reached of this great complexity of change as a mechanical process. It is at best but a hypothetical representative scheme. In place of such vague and complex conceptions we now offer the hypothesis of simple ultimate molecules, consisting of single atoms of each substance, surrounded by ethereal atmospheres which expand and contract as a consequence of inequalities in their mechanical state, and thus bring the dissimilar molecules into corresponding mechanical conditions and capabilities. We trust that what has now been offered in its support may secure for it serious consideration.

Let me, in conclusion, epitomize the whole discussion undertaken in this and previous papers, in two general statements.

(1). It has been shown that the mechanical laws and specific relations of bodies, in the three different states of aggregation, may be deduced from one general molecular formula; and that from their atomic weights and certain comparative dimensions assigned to their atoms, may be derived definite expressions representative of the various properties of special substances.

(2). We see that the diverse phenomena of inanimate nature are but different consequences of variations of ethereal tension, produced by propagated ethereal waves of varying tension; and that, contemplated from the highest point of view, they may be conceived to result from the operation of one primary form of force on one primordial form of matter.

TABLE OF RESULTS. I.

Sub.;	At. Wt.	Molec. Vol.	v'	c'	R'	R	R'—R	r	x	r'	x'	u'
H*	1	5.5	3.555	0.0208	2.0355	1.8078	0.247	1.65		1.65		0.1902
Li	7	11.9	5.00	0.00672	6.209	5.983	0.270	5.80	0.913	5.715	0.8958	0.1694
C	12	3.318	7.00	0.0189	4.098	3.676	0.422	3.440	0.497	3.130	0.459	0.600
N*	14	17	3.08	0.0490	5.997	5.507	0.490	5.273	0.630	4.920	0.6044	0.3416
O*	16	7.43	3.63	0.2491	4.350	3.673	0.683	3.108	0.409	2.990	0.400	0.594
		7.8 (obs.)										
Na	23	22.13	4.223	0.0420	7.595	7.0612	0.534	6.850	0.6137	6.416	0.593	0.361
S	32	15.87	6.00	0.05099	7.101	6.5484	0.553	6.30	0.5311	5.90	0.5121	0.563
S*	"	23	3.593	0.1918	6.541	5.767	0.774	5.278	0.48	4.870	0.4538	0.604
Cl*	35.5	22.8	3.45	0.2208	6.882	6.001	0.821	5.548	0.48	4.810	0.440	0.607
K	39.14	44.8	3.90	0.02721	10.850	10.327	0.520	10.16	0.0322	9.440	0.6250	0.313
Fe	56	7.1	7.05	0.3141	5.392	4.582	0.810	3.930	0.34	3.825	0.3333	1.454
Cu	63	7.1	6.06	0.4904	5.331	4.382	0.949	3.5	0.3019	3.533		1.464
Zn	65	8.7	5.49	0.4969	5.787	4.856	0.971	4.0	0.3321	3.94	0.3285	1.313
Br*	80	27.8	3.63	0.5533	7.320	6.220	1.100	5.357	0.383	4.880	0.302	1.000
Ag	108	10.17	5.00	0.5328	5.972	4.964	1.008	4.074	0.300	3.780	0.284	2.003
I	127	25.3	4.14	0.9022	7.850	6.5038	1.346	5.30	0.350	5.00	0.3722	1.908
I*	"	34.9	3.56	0.9846	8.384	7.00	1.384	5.80	0.'63	5.00	0.3522	1.247
I't	197.4	9.23	7.00	1.081	5.9231	4.5018	1.331	3.00	0.208	3.40		2.79
Pb	207	18.02	7.50	0.970	7.519	6.181	1.338	4.952	0.300	4.055	0.290	2.42
Bi	210	20.74	8.57	0.6340	7.943	6.702	1.181	5.839	0.230	5.300	0.3123	2.436
C*	12	11.50	3.63	0.0810	5.18	4.665	0.515	4.387	0.595	3.329	0.484	0.379

TABLE OF RESULTS. II.

Sub.; At. Wt.	Molec. Vol.	Density of Nucleus.	Molec. Heat		f	f'	f''	$\frac{f}{\sqrt{d}}$	$\frac{f'}{\sqrt{d}}$	Point of Fusion.	
			Cal.	Obs.						Cal.	Obs.
H* 1	5.5	0.221	2.16	2.3	74.67	21.00	115.5	43.08	66.68		
Li 7	11.9	0.077	6.6	6.6	101.1	20.22	240.6	92.3	219.7	377° F.	356° F.
C 12	3.318	0.299	1.76	1.76	21.12	3.017	10.02	24.09	13.03	10,000°	
C* "	11.59	"			13.87	3.82	43.03	8.01	25.36		
N* 14	17	0.117	4.94	5 to 5.5	94.08	25.57	434.69	54.32	251.0		
O* 16	7.43	0.60	3.7	4	51.57	14.40	107.00	29.77	61.78		
Na 23	22.13	0.069	6.7	6.7	79.43	17.163	379.82	56.0	293.8	247° F.	190° F.
S 32	15.87	0.153	5.77	5.7	51.41	8.568	135.9	54.3	143.5	56°	210°
S* "	23	"	5.74		74.91	20.9	470.7	43.25	271.7		
Cl* 35.5	22.8	0.319	3.2		70.35	20.4	465.12	40.62	268.6		
K 39.14	44.8	0.048	6.6	6.5	70.97	16.81	733.09	54.90	582.5	262°	136°
Fe 56	7.1	1.000	6.4	6.4	40.8	5.83	41.4	46.0	46.1	3041°	4082°
Cu 63	7.1	1.50	6.0	6.0	34.66	5.78	41.0	36.7	44.0	2475°	2487°
Zn 65	8.7	1.058	6.16	6.2	36.67	6.68	61.18	36.9	61.6	907°	773°
Br* 80	27.8	0.786	6.58	6.7	67.75	19.0	528.2	35.9	280		
Ag 108	10.17	2.00	6.33	6.2	39.04	7.81	79.4	37.6	76.5	1931°	1873°
I 127	25.3	1.016	7.05	6.9	47.5	11.47	290.0	34.1	208.2	454°	226°
I* "	34.9	"			45.8	12.9	583.7	26.4	337		
Pt 197.4	9.23	7.30	6.4	6.4	17.84	2.55	23.53	20.3	26.8	5485°	
Pb 207	18.02	2.00	6.4	6.4	29.92	3.99	72.0	35.4	85	611°	612°
Bi 210	20.74	1.40	6.52	6.5	32.35	3.77	78.0	41	99	520°	437°

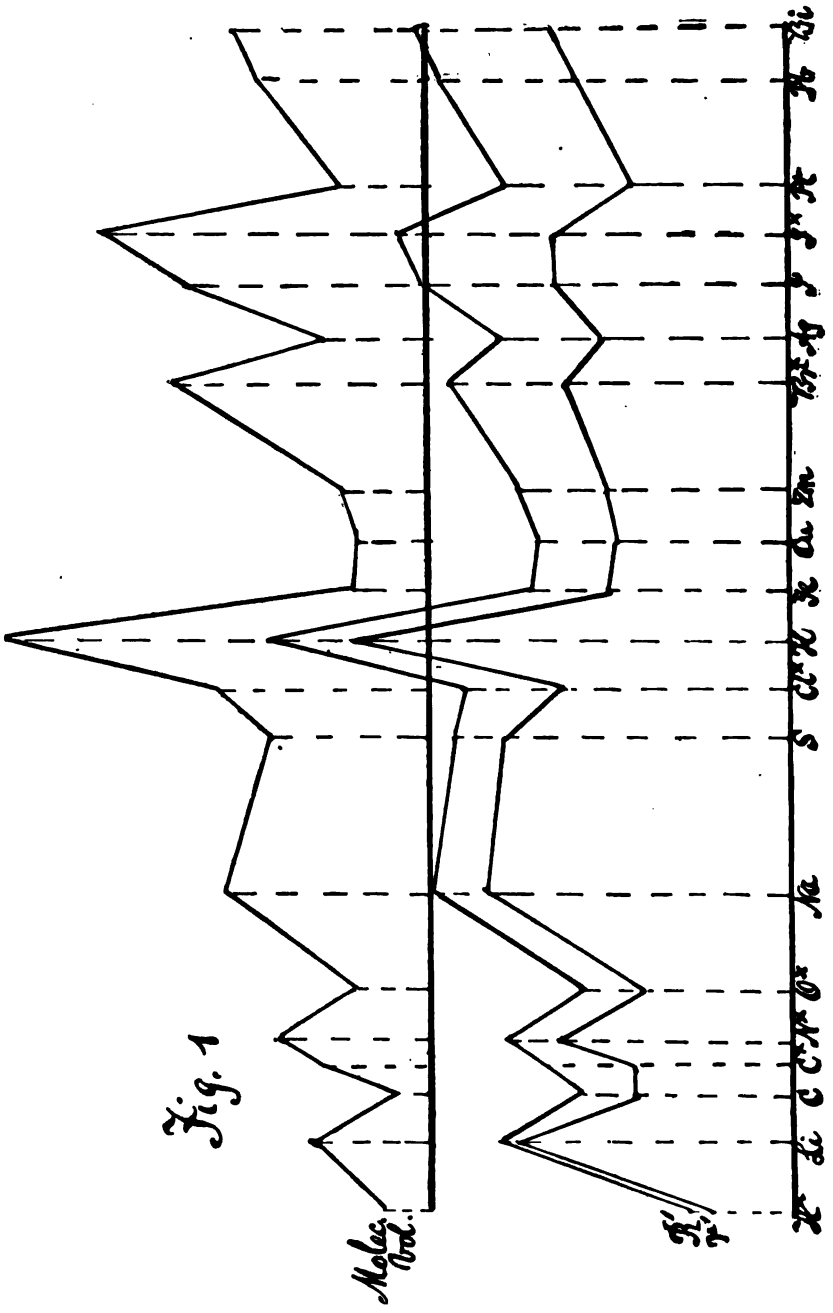


Fig. 1

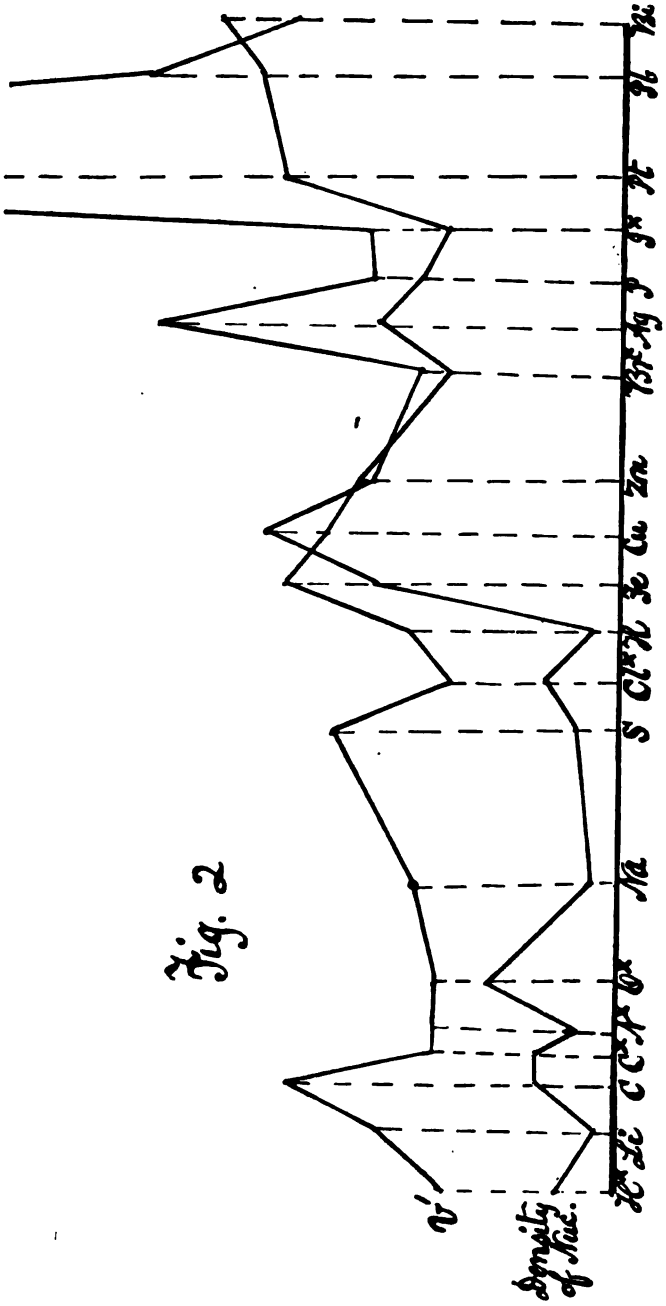


Fig. 2

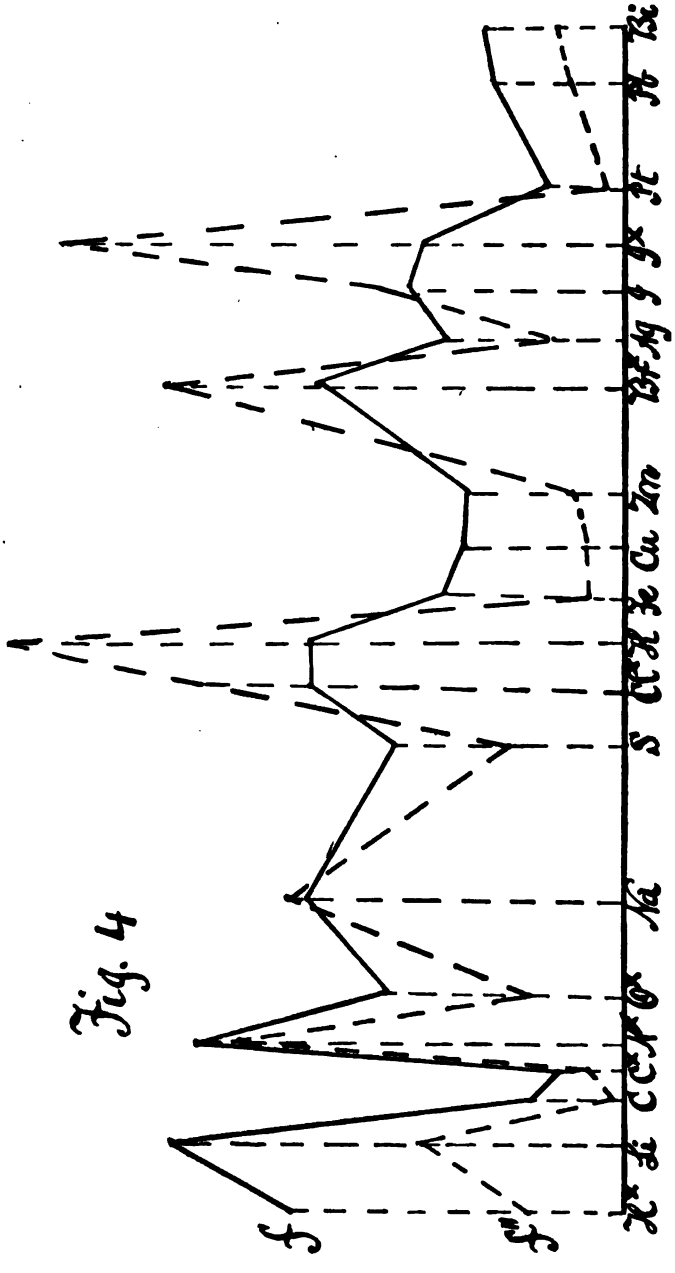


Fig. 4

On the Force of Effective Molecular Action; and the Mechanical Properties and Laws dependent on it. By William A. Norton, of New Haven, Conn.

[ABSTRACT.]

This paper is, for the most part, a connected review of several topics discussed in former papers published in the American Journal of Science.[1] It was read by title only.

On a Simple Device for Projecting Vibrations of a Liquid Film without a Lens. By H. S. Carhart, of Evanston, Ill.

Sedley Taylor's Phoneidoscope, for obtaining vibrations in a liquid film, employs a horizontal film, and conveys the vibrations to an enclosed mass of air by means of a rubber tube and a funnel. In this case the figures obtained are viewed directly without projection.

Before the publication of Mr. Taylor's method in "Nature," March 28, 1878, I had already succeeded in obtaining projections of such sound-figures by means of the lantern. A tin tube, five centimetres in diameter, was closed at one end with parchment, and at the other with a film of soapy water strengthened with glycerine. This film was held obliquely in the light in front of the lantern condenser; a lens in the path of the reflected beam formed an image of the film crossed with colored bands. The vibrations of the voice, taken up by the parchment, are communicated to the enclosed air and thence to the film. This method possesses the very important advantage of not disturbing the film with the breath, as in the case of a tube open next to the mouth.

Accident led to a simplification of this method when sunlight is used. The simplest apparatus that will effect the desired object is most serviceable in illustrating science. I do not hesitate,

[1] See Journal of Science, March, 1879, May and June, 1879.

therefore, to present this very simple instrument, designed to project on a screen by means of sunlight, the sound-figures in a liquid film produced either by the voice or by an organ-pipe. No lens or mirror is employed, the film being made to project an image of itself. With sunlight directed horizontally into a window by means of a porte-lumière, the instrument contains in itself all that is necessary for projection.

A short, thick tube of wood is furnished at one end with a telephone mouth-piece and ferrotype plate; the other end has attached to it a funnel about ten centimetres in diameter, blackened within and without. Near the middle of the tube a stop-cock is inserted. A film is obtained in the open funnel in the usual way, and is then slightly distended by blowing air into the enclosure through the stop-cock. The stop-cock being closed, the apparatus is air-tight, and the film retains a nearly constant curvature. This convex film, held in the beam of light at the proper angle, causes the reflected rays to diverge and produces a greatly enlarged image of itself on the screen. The degree of magnification is completely under control, since it is dependent on the curvature of the film. If the curvature has been made too great, the contractile power of the film, due to its surface tension, may be made to expel some of the enclosed air through the open stop-cock.

Upon singing a sustained note at the mouth-piece, concentric circles, distorted into ellipses by oblique projection, appear upon the screen. These can be kept sufficiently steady to permit of photographing them. Two photographic negatives were taken from the screen on which the projections were made. One of them exhibits clearly what I have not been able to make out on the screen, viz.:—a division of the film into segments with indistinct nodal lines radiating from the centre, like the nodal lines on a circular plate of glass clamped at its centre.

By placing a cap provided with a rubber band, and having a square opening made in it, into the open end of the funnel, a film of different shape is obtained. It is then observable that only notes of a definite pitch at the mouth-piece agitate this film in a well-defined manner; that is, produce in it "stationary waves." When a definite configuration is obtained, it is found to consist of a reticulated pattern of lozenge-shaped figures. Bright points are noticeable at every other intersection of the lines, appearing like knots tied in the interlaced cords of a net. Organ pipes produce

these figures with more certainty and definiteness than the voice.

With a triangular opening in the cap, the distended film is too much distorted from the spherical form to yield a clear image. In this case, with a flat film a lens may be employed to obtain the image. Let a clear musical note then be sung near the mouth-piece of the apparatus; immediately the field of color is covered with an exquisite pattern of fixed, hexagonal figures, the colors presenting at the same time the appearance of flowing, sometimes irregularly and sometimes around fixed centres. With a clear, sustained note, nothing can exceed the beauty of this combined acoustic and chromatic display.

NOTE ON THE ZODIACAL LIGHT. By HENRY CARVILL LEWIS, of Germantown, Pa.

[ABSTRACT.][1]

THE results of a series of observations, made by the writer, upon the zodiacal light extending over a period of nearly five years, are here recorded. The special precautions taken, both to train the eye to detect faint lights, and to prevent bias on the part of the observer, are given in detail.

The zodiacal light is divided into three portions—the *zodiacal cone;* the *zodiacal band;* and the *gegenschein.* This division is convenient in observation, saves confusion in description, and may be in part a natural one.

The zodiacal cone.—This, the zodiacal light proper of most authors, is the well-known cone of light rising along the ecliptic, and best seen in the winter months in the west, immediately after the disappearance of twilight. The time of shortest twilight coincides with its greatest brilliancy. Several observations are given when the writer saw it cast a distinct *shadow* at that time. Its comparative brightness with the Via Lactea at different seasons are given, and its relation to the ecliptic discussed. It was stated that the cone in our latitude is not symmetrical; and that while its

[1] Published in full in Amer. Jour. Sc. and Arts, December, 1880.

axis of greatest brightness lies exactly upon the ecliptic, its axis of symmetry is north of that line. An inner short cone of greater brightness was described. The warm color was thought to be due to atmospheric absorption. No pulsations were ever observed which could not be explained either by atmospheric changes or by changes in the eyesight of the observer. No periodic changes in the zodiacal light were observed; the same series of changes occurring each year with an equal amount of brilliancy. It was shown that while the zodiacal cone is frequently seen by moonlight, the moon appears to have no appreciable influence upon it. The account of the zodiacal cone closes with a description of its *spectrum*, which is always continuous and free from bright lines.

The zodiacal band.—This is an extremely faint zone of light, somewhat wider than the Via Lactea, which, like a strip of gauze, is stretched across the sky along the zodiac from horizon to horizon, and which can be seen at all times. It is a belt which forms a very faint prolongation of the zodiacal cone, and which, like it, is best seen when the ecliptic makes a large angle with the horizon. It is so faint that it can only be seen with difficulty. It is brightest along an inner line, and fades off more suddenly on its southern than on its northern edge. It has a width of about 12°, and its central line is slightly north of the ecliptic. Observations prove the zodiacal band to be a constant and invariable phenomenon.

The gegenschein.—The gegenschein is a faint patch of light, apparently some 7° in diameter, which nightly appears in that part of the zodiacal band which is 180° from the sun. Night after night it shifts its place so as to keep opposite to the sun. It is decidedly brighter than the zodiacal band, and frequently a central nucleus about 2° in diameter, of greater brightness, can be observed. While the brighter portion of the gegenschein is circular, its faint boundaries have sometimes the form of an oval, whose major axis is parallel to the ecliptic. A large number of maps of its position among the stars have been made, which show that while its central point is always 180° in longitude from the sun, it has a latitude of + 2°.

The moon zodiacal light.—An oblique cone of light in the proximity of the moon was described by Rev. G. Jones, but has not been detected by the writer. The light preceding moonrise rises at right angles to the horizon, and seems purely atmospheric. One observer has described comet-like tails on either side of the moon.

The writer holds that such appearances are caused by diffraction through floating vapor, since they are not seen on clear nights.

The horizon light.—The phenomenon to which this name is applied, though having no connection with the zodiacal light, is so continually observed with the latter, and at certain seasons is so apt to be confounded with portions of it, that it is necessary to take it into account. The horizon light is a faint band of light with parallel sides, lying all around and parallel to the horizon, and separated from it by an interval of darkness. It is brightest and terminates most abruptly, on its lower edge. This sharp lower edge is 5° above the horizon, while the diffuse upper edge varies in altitude with the state of the atmosphere. The horizon light has a mean width of about 15°. It is purely atmospheric and appears to be caused by reflected starlight. It becomes very bright when the moon is above the horizon. Below the horizon light is a dark space, here called for convenience, the *absorption band.* This quenches the light of the Via Lactea, the zodiacal cone, and all but the largest stars and planets. When the ecliptic is low, the horizon light frequently blends with the zodiacal band.

THE AURORA AND ZODIACAL LIGHT OF MAY 2, 1877. By HENRY CARVILL LEWIS, of Germantown, Pa.

ALTHOUGH the Aurora of May 2, 1877, as seen from Germantown, Pa., was not a remarkable one, either for brilliancy or for beauty of coloring, yet special interest is attached to it from the fact that it was seen in conjunction with the Zodiacal Cone, and direct comparisons could be made between the two phenomena.

No auroras had been noticed for several years previously, it being a period of minimum auroral intensity. The aurora was first observed at 8.35 P. M., when it was brighter than at any subsequent time. It then consisted of a bright, nebulous mass of light along the northern horizon, arched above, extending from Auriga to Cygnus. Polaris was over about the centre of this

arch, from all parts of which numerous streamers darted upward toward the zenith.

Meanwhile, the zodiacal cone rose obliquely along the ecliptic, farther west. It was a much less conspicuous object than it had been two months before, when twilight was shorter. It was in no way altered from its usual appearance at this season, by the presence of the aurora. The southern edge of the cone was, as usual, better defined and more vertical than the northern edge; while the axis of greatest brightness, corresponding closely with the ecliptic, lay south of the axis of symmetry. Its base was widened out by atmospheric diffraction, and its apex merged into the zodiacal band.[1] It was of about the same pale color as the aurora, having lost the warm color which can be noticed at its maximum of brightness. Unlike the flickering streamer of the aurora, it remained perfectly steady, and showed no fluctuations of brightness. It was bright until about nine o'clock, and then, as its lower portion sunk below the horizon, gradually became more and more faint, until it could be no longer recognized.

The aurora went through a number of changes in detail, although maintaining the same general appearance for more than an hour. An arch, entirely disconnected with the lower arch, would sometimes form completely in a few seconds. This upper arch was composed of a number of very short rays placed laterally one along side of the other; and the arch extended by the addition of more short rays. Sometimes small clouds of light, not unlike cirrus clouds, formed near the horizon upon the longer rays, without interrupting them. Once an arch composed of short rays, each of which was some 3°–4°, and every fourth or fifth of which was longer and brighter than the others, formed suddenly and simultaneously across the sky, stretching from the zodiacal cone, against which one extremity apparently rested, to a point in Cepheus. New streamers were continually added to it in the east, and the short streamers composing it apparently drifted very slowly westward. At 9.27, this arch disappeared entirely in the course of a few seconds. At 9.34, it was again formed, stretching from β Taurus to β Cepheus, and consisting as before, of short rays.

At the formation of each arch, or when a new streamer is shot forth, the nebulous mass of light near the horizon would greatly

[1] Note on the Zodiacal Light, by H. C. Lewis. Amer. Jour. Science, December, 1880.

brighten, as though it were a cloud illuminated by flashes of light. The streamers were nearly parallel to one another, their centre of divergence lying below the horizon. They were quite straight, and, unlike clouds, sharply defined on their edges. Often a streamer seemed best defined on the edge nearest the centre of the aurora. At one time, it was thought that a number of the streamers on the eastern half of the aurora were bent over at their upper portions toward the east, as though blown by a wind. The streamers were brighter and higher at the extreme east and west of the aurora, than at its central northern portion. The color of the streamers and of the bank of light below them was pale greenish-white. At first, a few short streamers of a warmer tint were occasionally seen.

At 9.33, the zodiacal cone being now low down towards the horizon, and pointing towards Præsepe, a very remarkable auroral streamer formed low down in the west. It was a bright, short streamer, probably 2° or more in width, which pointed very slightly west of θ Gemini. It started from near Aldebaran, now below the horizon, passed between β and ζ Tauri, and reached Auriga between $\varkappa$ and θ. It was thus close to the edge of the zodiacal cone.

This streamer remained in the same position for nearly an hour. It was remarkably steady, and was not unlike a pale grayish-green comet's tail. For the first ten minutes, it was without any fluctuations. The arch died away, but this remained, until at 9.43, it too nearly disappeared, leaving only a trace of its position. Before ten o'clock, it was again bright. At 10.03, the rest of the aurora was dim, and no other streamers appeared. It was now very conspicuous, and remained so until nearly half-past ten, when it finally died away,—a faint haze on the northern horizon alone remaining. All this time, as seen from the position of this streamer back of certain trees and its angle with the horizon, it had maintained precisely the same position with reference to the earth.

Meanwhile, the heavens had revolved past it, the stars first noticed had moved beyond it, and the zodiacal cone had set. The streamer had remained like a great pointer fixed to the earth, marking its motion. While the zodiacal cone gradually sank below the horizon, the auroral streamer had retained its position,—a circumstance well illustrating the cosmical character of the former and the terrestrial character of the latter.

Another fact pointing to the same conclusion was found in the difference of their spectra. In the early part of the evening, while both the zodiacal cone and the aurora were at their brightest, an opportunity was taken to compare their spectra. The observations were made with Eaton's direct vision spectroscope.

The zodiacal cone gave a faint, short, continuous spectrum, brightest near its least refrangible end. It was most sharply terminated at the same end, fading off gradually toward the more refrangible end. It was very faint throughout, and could be seen only through a wide slit.

The aurora gave a spectrum much longer than that of the zodiacal cone, though of about the same pale greenish color. At or close to the less refrangible end was a sharp, bright line of a grayish-green color. The spectrum from this line toward the more refrangible end was continuous and gradually diminishing in brightness, except that a very faint brightening near the blue showed that a brighter aurora might have given a line at that place. The green line of the aurora could be seen with a very much narrower slit than that required to see the spectrum of the zodiacal cone. If the spectra of the aurora and the zodiacal cone could be superposed, it would be found that the green line of the former is close to the more refrangible end of the spectrum of the latter.

The spectra show that while the light of the zodiacal cone is polychromatic, that of the aurora is nearly monochromatic, or, as it might be expressed, *oligo-chromatic*. The former is such as would be given by sunlight reflected on diffuse matter in space; the latter might be given by an electrical discharge through a gas.

Later in the evening, the faint zodiacal band was observed stretching completely across the sky along the ecliptic, while a round patch of light on the boundary between Virgo and Libra, lying apparently about 3° above the ecliptic, is recognized as the Gegenschein.

It is perhaps worth noting that no change of consequence in the weather or temperature took place during the two or three days preceding and following this aurora.

Experiments on the Strength of Yellow Pine. By Prof. R. H. Thurston, of Hoboken, N. J.

[ABSTRACT.]

In a paper read at the Saratoga meeting of the American Association for the Advancement of Science the writer presented the result of a series of experiments on the strength of timber, in which were given several unusual figures.[1]

To determine how far these results were due to peculiarities of the selected samples supplied from the Navy Yard, and to determine to what extent size affects the resistance, a more extended series of transverse tests were made, and the results of experiments upon yellow pine, of the ordinary market qualities and of various dimensions, are now presented below, as determined in the Mechanical Laboratory of the Stevens Institute of Technology.

In the paper referred to, the modulus of elasticity was given for yellow pine as an extraordinarily high figure. It will be observed that the best wood here described gives also very high values of E, and a comparison of the pieces of the first with these test specimens shows the selected Navy Yard specimen to have been of better material than either of the latter.

Samples marked F_1, F_2, F_3, were from the same plank—a piece of yellow pine cut in Georgia, April, 1879, and tested after several months of seasoning, when it had become thoroughly dry. The three specimens were considered good material. F_3 was not straight-grained and broke obliquely, giving a much lower modulus of elasticity, as well as of mixture, than its companion specimens.

Samples B_1, B_2, B_3, were cut from a stick ten inches square in section, which had been lying under cover, seasoning, nine or ten years. Numbers 1 to 12, inclusive, were small sticks sawn out of the middle of a plank, originally four inches thick, one foot wide, and twenty-four feet long. A stick was first cut three inches square and twenty-four feet long, which was then cut into strips of varying smaller dimensions. The wood was selected from lumber-yard stock, and was considered to be fairly representative of average timber. It was cut in Florida, in October, 1879, and reached Hoboken in January, 1880.

[1] Among these were several due to printers' errors, which, owing to the illness of the writer, were not corrected in proof. The tenacity of yellow pine, for example, was made 2070.2 when it should have been given at 20,702.

Specimens 1 to 8, inclusive, were too green for use in construction; Nos. 9, 10 and 11 were kiln-dried 56 hours, at a temperature of 130° Fahr., No. 12 was dried 12 hours, at 210° Fahr., at which temperature the pitch exuded from the wood quite freely. Still another specimen, not here recorded, was heated 1½ hours at 420° F., and was somewhat charred. Under test it gave a modulus of rupture, $R = 9000$.

In the table containing a *résumé* of results is also given the figures obtained in the earlier experiments on selected Navy Yard material, marked J and those of a sample, K, tested by another observer.[2]

Specimens A, A_1, were picked up in a workshop, and used simply to determine specific gravity; they were probably not the best southern long-leaved pine, such as it was intended that the samples tested should be.

[2] Van Nostrand's Magazine, Feb., 1880, p. 165.

The following table contains the values of the Moduli of Rupture and of Elasticity deduced from the whole series of experiments:

TABLE I.—*Strength of Yellow Pine Timber.*

Condition.	No. of Test Piece.	Dimensions. l = distance between supports.			$R = \frac{3}{2}\frac{Wl}{bd^2}$	$E = \frac{Wl^3}{4.5bd^3}$	Weight per cub. ft.	Spec. Grav.
		l ins.	b ins.	d ins.			Pounds	
Very dry.	F_1	54	2.939	2.946	12,700	.....	41.03	0.66
	F_2	54	2.972	3.008	12,050	1,962,000	45.72	0.73
	F_3	54	3.016	3.000	9,850	1,825,000	40.27	0.64
Old and thoroughly seasoned.	B_1	54	3.046	3.036	13,850	2,178,000	48.89	0.78
	B_2	54	3.016	3.050	13,710	1,949,090	47.14	75
	B_3	54	3.012	3.017	14,780	2,193,080	46.38	2.74
Too green for use.	1	40	1.285	1.315	12,150	2,171,000	52.85	0.84
	2	40	1.285	1.295	12,530	1,796,000	62.85	1.006
	3	40	1.285	1.295	10,810	1,953,000	.....	.....
	4	40	1.270	1.287	11,350	.....	.....	.....
	5	54	2.930	2.930	10,790	2,427,000	.....	.....
	6	54	2.925	2.925	10,940	2,231,000	.....	.....
	7	24	3.06	0.70	12,000	.....	.....	.....
	8	24½	3.032	0.675	.....	2,279,000	.....	.....
	8a	40	3.032	0.675	13,240	2,066,000	.....	.....
Kiln-dried 56 hours at 130° F.	9	54	2.885	2.875	12,100	.....	.....	.....
	10	40	1.249	1.275	17,280	2,564,000	.....	.....
	11	40	1.244	1.274	14,560	2,543,000	54.845	0.88
12 hs. at 210° F.	12	40	1.229	1.242	13,870	2,489,000	51.857	0.83
	K	40	1.23	1.23	14,654	1,892,510	37.72	0.60
	J	54	3.0	3.0	16,740	3,534,727	47.95	0.77
	A	.....	.....	.....	.....	.....	39.64	0.63
	A_1	.....	.....	.....	.....	.....	39.50	0.63
Sapwood from Average	F_3	.....	.....	.....	.....	.....	35.96	0.57
	.....	.....	.....	.....	.....	2,239,000	.....	....

Calculating $E=\frac{W l^3}{4 A b d^2}$ for heavy loads and considerable deflections, the following figures were obtained for this *pseudo*-modulus:

TABLE II.— *Values of E at one-third to one-half total load.*

No.	*E.*	No.	*E.*	No.	*E.*
F_2	2,136,000	1	2,055,000	7	.....
F_3	1,689,000	2	1,776,000	8	2,086,000
B_1	2,394,000	3	2,032,000	9	2,251,000
B_2	2,334,000	4	.....	10	2,869,000
B_3	2,489,000	5	2,272,000	11	2,574,000
		6	2,231,000	12	2,380,000
			Average		2,098,000

As a general rule, the maximum figure was given at one-third, rarely at one-half, and about as often at the beginning of the test. The latter case occurred most frequently with the unseasoned timber.

From the test of B_1 we obtained

TABLE III.

W.	*E.*	*W.*	*E.*	*W.*	*E.*
50	2,178,000	450	2,288,000	1050	2,363,000
150	2,249,000	650	2,327,000	1250	2,381,000
250	2,259,000	850	2,350,000	1450	2,394,000
				4750	Broke.
				Average	2,309,888

Heavy loads, long applied, produced fracture of pieces, the companions to which resisted considerably more when the load was steadily increased up to the moment of fracture. The maximum permanent load was apparently something less than one-third and probably greater than one-half the maximum load which could be sustained under ordinary test.

A set of four yellow pine beams of large section was tested to determine the value of the Modulus of Elasticity in large timbers, and also to ascertain whether experiments upon small sticks, such as are usually tested, may be relied upon to give correct values of this coëfficient.

The following figures were obtained:

TABLE IV. — *Elasticity of Yellow Pine Timber.*

Size. Inches.			Weight. Lbs. per cu. ft.	Spec.	Max. load.	Max. deflection.	$E=\frac{P^1 l^3}{4 \Delta b d^3}$
b.	*d.*	*l.*		Grav.	P	Δ	Average.
6.187	8.5	144	40.13	0.64	5460	0.7652	1,110,247 (1)
6.75	8.	200	41.2	0.66	5813	3.8946	895,029
6.875	8.25	150 to 200	....	....	1850 to 5900	0.1875 to 2.29	1,768,070 (2)
6.75	8.375	490	45.27	0.72	650	3.234	.....
			Average of all experiments made				1,387,156

[1] Average of trials at $l = 150$ to 200.
[2] Average of trials at $l = 150$ to 200, and beams in different positions.

Nos. 1 and 2 of this lot were of poor quality; the latter, particularly, was somewhat shaky, yet it fairly represents much of the timber which finds its way into important constructions.

From the whole series of experiments, of which the briefest possible statement of the results obtained is here given, the conclusion may be drawn that:

The elasticity of yellow pine timber as used in construction is very variable, the modulus varying from one to three millions, the average being about two millions in small sections, and a little above one and a half millions in large timber (Tables I, IV).

The highest values are as often given by green as by seasoned timber, and that, under sixteen square inches section and fifty-four inches length, at least, the magnitude of the modulus of elasticity is independent of the size of the piece (Table I).

The density of the wood does not determine the modulus; since the figure varies sometimes directly and sometimes inversely with the density, even where the wood is as nearly as possible in the same condition as to seasoning.

A high modulus usually accompanies high tenacity and great transverse strength, but it is not invariably the fact that maximum ultimate strength is accompanied by initial stiffness (Table I).

The pseudo moduli, determined by taking considerable deflections, are usually not greatly different from those determined from

small deflections and light loads. The values of these moduli may increase with increase in deflection (Table III).

An inspection of the woods tested plainly indicates, in the opinion of the writer, that the density of the pines is so considerably modified by the amount of pitch contained in the sap channels that it cannot be regarded as indicative of the strength of the timber. Where quite free from sap, the wood usually exhibits increase of strength and elastic resistance to deflection, with increase of density.

The strength of timber, otherwise similar, is greatly affected by its structure, and the resistance offered to stresses applied transversely is greatest when the sections of the timber taken transversely exhibit most nearly vertical lines of grain.

The modulus of rupture by transverse stress varies, for yellow pine, from $R = \frac{3}{8}\frac{Wl}{bd^2} = 10{,}000$ to 17,000, the highest values being usually obtained from well-seasoned wood. An average value may be taken as $R = 13{,}000$ for good timber, which in the formula $W = C\frac{bd^2}{l}$ gives $C = 8{,}666$ pounds, or, practically, $W = 9{,}000\frac{bd^2}{l}$, for good yellow pine.

The modulus of rupture varies as irregularly, and with as little regard to size or density of the material as does the coëfficient for elasticity.

In the use of such materials, the only safe course for the designing and constructing engineer is evidently to adopt a moderate value of the modulus in proportioning his work, and by careful inspection and test to secure the rejection of all material which is not of good quality.

Careful inspection may sometimes lead to the selection of material—like the Navy Yard stock here described—twenty-five per cent. superior to the average of good timber, and fifty per cent. more valuable than the lower grades such as are often sold in our markets.

[For the complete paper see Journal of the Franklin Institute, September, 1880].

The Velocity of Shot. By Alfred M. Mayer, of Hoboken, N. J.

About a year ago, I determined to make a series of experiments with an undeniably accurate chronoscope, which would give direct measures of the velocity of fowling-piece shot of various sizes. At that time, I was not aware that any determination of this kind had ever been made, though similar ones on the velocities of the balls of ordnance and rifles were numerous. Thus I began my work entirely free from preconceived ideas as to what the experiments would reveal. It was only toward the end of my work, when on a visit to my friend Professor Rice, of the United States Naval Academy, that the professor told me that he had made a short series of similar experiments with a Le Boulangé chronoscope, and had published them in the "Rod and Gun" in July, 1875. This paper I then read, and I here reprint it as an appendix to my paper. My attention has subsequently been called to the works of other experimenters in the same direction; of these I have taken references, but have purposely avoided reading them.

I have made this preamble to my paper to show that my experiments are to be taken as entirely independent of other similar work, for I was not guided nor influenced during my experimenting by a knowledge which might cause me to reject certain experiments as erroneous, because they did not give results which I had thought *they should have given;* for no matter how conscientious an experimenter may be, he is sometimes influenced by preconceived notions, and unwittingly desires his experiments to bring out certain results. I have always in my work endeavored to avoid this mental prejudice, and have therefore, whenever possible, worked in this manner: viz., I write down the numbers given by the experiments as they are successively made, and do not reduce or compare them till after the whole series has been finished. The reader will therefore find in my results apparent anomalies, even contradictions, or, if you please, absurdities. These anomalies are those numbers which always catch the eye of him who does not know, from experience, the difficulties and exhausting toil of a protracted series of delicate experiments; and on these anomalies he will dwell to the exclusion of what, in the main, the experiments conclusively show. To such I say, with the experience of twenty-five years of experimenting: make the experiments for yourself and do better,

and for your better work science will be indebted and will surely credit you with your superior skill and patience. Each experimental investigation is a step in progress to something better; but without the first step is made, the firmer tread will not be gained. *C'est le premier pas qui coûte.*

Nature, however, does not work in an anomalous, contradictory, or absurd manner. These anomalies are of our own production, and are due either to the errors of the apparatus we use, or to certain peculiar actions in the phenomena which accompany those we are particularly studying. The apparatus detects and measures both of these classes of actions, and it is the office of the investigator, after his experiments have been finished, to unravel one from the other. Thus, for example, in the flight of shot, we have many moving pellets, and they do not all move with the same velocity. No apparatus known can give the velocity of each pellet in a charge so that we may therefrom deduce the average velocity of the charge. So it happens that with charges of large shot, when you have fewer pellets, and especially when these charges are shot over great distances, some pellets of high speed may strike the target and record their velocity, while in the next experiment, pellets with lower velocity may record themselves; and this will unavoidably happen, no matter how accurately we hold the gun on to the target. But these very differences, shown in the time of flight of the various pellets, point out an interesting and important fact; showing that such work as we have begun requires a long time for its completion, for the measures of the velocities of many charges of the same weight of powder and shot must be made to get the *average* velocity of all the pellets. It may be here asked why I did not do this. To this question I reply that I cannot afford to give a year of time to make the first step in this subject of investigation. A rich field I have pointed out for other experimenters to enter; and I trust that, since our large American arms manufacturers are now making fowling-pieces, they will devote a mite of their profits to just such investigations as this one, on which I have spent much of my thought, time and private means.

The errors of the records of the apparatus itself can easily be investigated, and they have been accurately determined. They are exceedingly minute and cannot affect appreciably the results of our experiments.

The deductions which I make from these experiments are those which these experiments show, and not what may be shown in the experiments of others. I give these experiments of mine as entirely independent evidence on the subject of the velocity of shot, and of course my readers may give them just as little or as great weight as their individual judgments may determine. That they have been conscientiously and carefully made, no one will doubt who has had any experience in the use of chronoscopes used in measuring the velocity of projectiles.

I do not think that the sportsman and experimental physicist are often found in one person, even in this country — still less in Europe. From my youth I have been devoted to everything relating to the gun, and this experience has, I presume, given me an advantage in this investigation over other physicists. I had also the good fortune to have had a brother sportsman and experimenter for my assistant — Professor B. F. Thomas, now of the University of Missouri — who entered enthusiastically into this work, aiding me with his ingenuity as an experimenter and with his suggestions derived from his extensive experience as a sportsman.

Description of the Chronoscope and a Determination of the Magnitude of the Error in its Results.

The chronoscope used in these experiments is very simple. It consists of a metal cylinder turning on an axle on which is cut a screw. This screw moves in a stationary nut, and this arrangement gives the cylinder a lateral motion when it is revolved on its axle. The cylinder is covered with fine printing paper, which is then smoked with burning camphor. A tuning-fork is screwed into one end of a thick piece of wood. The other end of this piece of wood is hinged on to a base. To the end of one of the prongs of the fork is cemented with shellac a small, triangular piece of foil. The fork is vibrated by a bow, and then the hinged board is brought down against a stop so adjusted that the point of the foil on the fork just touches the smoked paper. On now turning the cylinder, a wavy trace will be written on it by the vibrations of the fork.

To determine the number of vibrations made in one second by the fork, a good clock, accurately rated, sent at each second an electric spark from an induction coil out of the tracing point and through the paper. Thus the sinuous traces of the fork were

punctured by electric sparks. The number of waves of the fork's trace contained between two of these punctures is the number of vibrations made by the fork in one second. A multitude of experiments showed that the range of the determination of the number of vibrations per second of the fork was very small, and the means of several such measures did not vary from one another by more than one-tenth of a vibration, or, expressed in time, the variation did not surpass the 1–2560th of a second. This fact showed that the chronoscope, so far as its records were concerned, was sufficiently constant and accurate for measures on the velocity of projectiles.

The effect of temperature on the vibratory period of the fork had been determined in a previous research. It amounts to an increase of .000045 of the periodic time of the fork's vibration for an increase of 1 deg. Fahr. in the temperature of the fork.

The guns used in the experiments had rebounding locks. The primary current of an induction coil passed through a break-piece fixed under the rebounding hammer, so that at the instant the cartridge was exploded the electric current was broken and then immediately formed again. The current which passed through this break-piece was led by a wire to an upright piece of tin plate whose front surface leaned against a thick copper wire. Another wire led from the tin plate (which stood in a shallow trough of mercury) back to the battery. One terminal of the secondary coil of the inductorium is connected with the axis of the metal cylinder, the other terminal with the foot of the fork.

This chronoscope is worked as follows: One person vibrates the fork with a bow, and then brings the pointed foil down on the smoked paper and rotates the cylinder. While the fork is marking its sinuous trace he cries "fire," and the other person discharges the gun at the tin plate. At the instant the cartridge explodes, a minute spark issues from the tracing point of the fork and cuts a small hole through the blackened paper, in the sinuous trace of the fork; and, when the tin plate is knocked over by the shot, another similar spark flies from the tracing point.

We know the distance between the breach of the gun and the tin plate; the number of flexures in the trace of the fork contained between the two spark holes gives the time the shot took to go over the known distance; whence the velocity of the shot per second is readily computed.

The fork used in these experiments made about 256 vibrations,

or flexures, in the trace in one second; so if there should appear 32 flexures between the two spark holes the record would give 32–256ths, or one-eighth of a second for the time of flight of the shot from the gun to the distant target. Two guns were used in these experiments, one of 12 the other of 10 gauge. They were "full choke-bored," and were choked exactly alike. They were made by the Colt Arms Manufacturing Co., of Hartford, Ct.

The following tables give the results of our experiments:

I. 10 Colt gun, 5 drs. Curtis & Harvey powder, 1 1–4 oz. shot.

Size of shot.	Vel. 30 yds.	Vel. 40 yds.	Vel. 50 yds.
No. 1 buck . .	1153	1067	—
FF	1147	1132	—
BB	1146	1126	—
No. 3 . . .	1066	1015	928
No. 6 . . .	1012	963	859
No. 8 . . .	995	880	775
No. 10 . . .	908	803	716

II. 10 Colt gun, 4 drs. Curtis & Harvey powder, 1 1–4 oz. shot.

No. 1 buck .	1067	1018	—
FF	1017	1009	967
BB	1000	967	897
No. 3 . . .	989	911	872
No. 6 . . .	966	883	806
No. 8 . . .	920	874	776
No. 10 . . .	848	756	669

III. 12 Colt gun, 3 1–4 drs. Curtis & Harvey powder, 1 1–8 oz. of shot

No. 1 buck . .	—	—	—
FF	—	—	—
BB	862	795	667
No. 3 . . .	844	754	696
No. 6 . . .	825	739	600
No. 8 . . .	816	749	607
No. 10 . . .	796	680	610

IV. 12 Colt gun, drs. Curtis & Harvey powder, 1 1–4 oz. of shot.

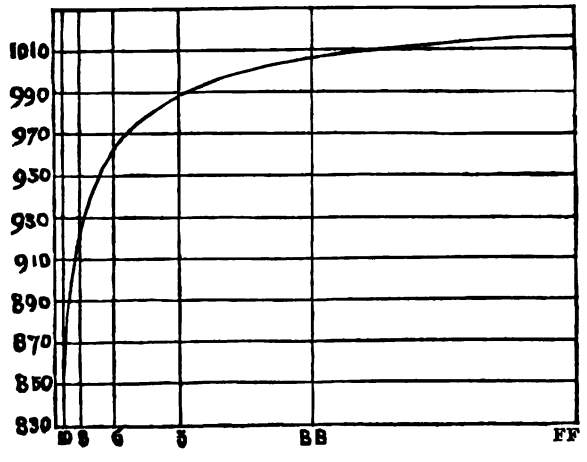
No. 8 . . .	847	722	671
No. 10 . . .	748	657	596

Each measure of velocity given in these tables is the mean value obtained from several experiments, varying in number from three to six. The headings, "velocity 30, 40 and 50 yards," mean that the numbers under them give the average velocities of the flight of shot over these distances, and not the velocities at 30, 40, and 50 yards from the gun.

It will be observed that the shot used were Nos. 10, 8, 6, 3, BB, FF, and No. 1 buck shot. They were so selected because a pellet of any number in the above series weighs very nearly double the preceding one. Thus, a pellet of No. 8 weighs double one of No. 10, a pellet of No. 6 weighs double one of No. 8, and so on. These relations of weight among the pellets were obtained so that I could readily reach the relations existing between the velocity of gun-shot and the weight of the pellet. The shot used was kindly furnished me by Tatham & Bros., of New York, who used carefully gauged sieves in their manufacture. The powder used was Curtis & Harvey's Diamond Grain No. 6. The powder and shot in each cartridge fired had been carefully weighed out in an accurate balance.

A glance at the tables at once shows the rapid increase in the velocity of gun-shot from No. 10 up to No. 3. With the heavier pellets the increase in velocity is less marked. Thus the table headed, "10 Colt gun, 4 drs. Curtis & Harvey, 1 1–4 oz. shot," shows that No. 8 shot has 72 feet per second velocity over No. 10 shot, and No. 6 has 46 feet over No. 8, while No. 3 has only 23 feet over No. 6, and BB shot gains only 11 feet over No. 3.

The relations between velocity and weight of pellet shown in this table may be taken as a type of all the experiments, and I



have graphically shown their relations in the accompanying curve.

The divisions on the scale, measured from the bottom line

upward, give the velocity per second of the pellets. One unit on this scale equals 20 feet of flight of a pellet, and a unit of the scale, measured from right to left on the diagram, equals one unit of weight of pellet. The weight of a pellet of No. 10 shot is here taken as the unit of weight. The numbers of the shot are written under the horizontal line; the velocities on the vertical line. When the curve intersects these lines we find the velocity given on the vertical scale corresponding to the number or weight of shot given on the horizontal line of the diagram.

Professor Rice has found that this curve can be expressed by a mathematical formula. The curve is very nearly the curve of secants, and the formula for it is:

$$\frac{y}{b} = \sec.^{-1} \frac{x^n}{a}$$

Denoting by x the velocity, by y the weight of a pellet, and a, b, and n being undetermined constants.

So far as the experiments with these two special guns show, there is no doubt a great superiority in the 10- over the 12-gauge gun, when each is loaded with the same weight of powder and shot. Thus, with the same charge of powder and shot, 4 drs. powder and 1 1–8 oz. shot, fired from the 10-gauge, gives a velocity of 100 feet per second more than that given by the 12-gauge. This fact is conclusively shown in the comparison of the figures in the two tables headed "10 Colt gun, 4 drs. C. & H. powder and 1 1–4 shot;" and the difference in velocity was in favor of the 10-gauge in each of the sixty separate experiments, which were made to get the numbers (contained in the above mentioned tables) on the lines of No. 8 and No. 10 shot.

With No. 10 shot the mean velocity given by the 10-gauge gun over the first 30 yards is 848 feet. With the same charge in the 12-gauge the velocity is 748 feet, showing a difference of 100 feet in favor of the 10-gauge. With No. 8 shot the difference amounts to 72 feet. The average difference in favor of the 10-gauge in the flight of shot Nos. 8 and 10 over 40 yards amounts to 110 feet.

If we assume, as we certainly may without grave error, that the penetration of shot varies as the square of its velocity, these experiments will give the relative penetrations of the 10 to the 12-gauge about as 9 is to 7. These experiments show that the recent movement in favor of small-bore guns is one in the wrong

direction. It appears that a 10, or even an 8-gauge gun, if of about 8 lbs. weight, would be the best fowling-piece for upland shooting.

That the 10-gauge shows such superiority over the 12 may be accounted for by the fact that the same charge occupies less length in a 10 than it does in a 12-bore, and hence there are fewer pellets in contact with the barrel of the former than of the latter, to oppose by their friction the projectile force of the powder; and secondly, the powder in a 10-gauge is exploded nearer the centre of its volume, and thus does not have so much chance of blasting before it the unburnt powder contained in the portion of the charge removed from the point of ignition.

I also venture to predict that with the same weight of barrels the 10-gauge will not heat as much as the 12, because the motion of the shot, lost by the greater resistance it opposes in a 12-gauge cartridge, must appear in the form of heat.

The third fact which these experiments show is that the proper charge of shot in a 12-gauge gun for upland shooting is 1 1–8 oz., and not 1 1–4 oz., as has of late years been the practice to use; for the tables show that with 1 1–8 oz. of shot and 3 1–4 drs. of powder an average velocity is obtained which requires 4 drs. of powder to give 1 1–4 of shot a velocity equal to that given by 3 1–4 drs. to 1 1–8 oz. Now 4 drs. of powder, if not fired from a gun weighing at least 9 lbs., and from a good, strong, muscular shoulder, is disagreeable. The effect on the body, and especially on the brain, is neither conducive to pleasant nor to good shooting. The number of pellets in a charge of 1 1–4 oz. of No. 8 shot is 499. In a charge of 1 1–8 oz. of the same shot there are 449, therefore only 50 pellets more in a charge of 1 1–4 oz. than in a charge of 1 1–8 oz.; and surely the want of the 50 will not cause a good shot to miss his bird with 449 pellets, nor will the addition of the 50 give a bad shot any more chance of bringing his bird to bag with his 499 pellets.

I wish now to show to the Association, and especially to those members of it who are sportsmen, other applications of these experiments to the art of shooting on the wing.

There are two styles of shooting on the wing—one is called "snap-shooting," where the shooter, on selecting the bird which he wishes to bag, quickly brings the gun to his shoulder and, at the instant it is in place, fires. If the bird is a cross shot, he deter-

mines, at the moment of fire, the distance to which he should direct his gun ahead of its flight, this distance depending on the velocity of the bird's flight and on his distance from it. This manner of shooting is practised the more generally by upland gunners in shooting quail, grouse and woodcock.

The other style of shooting may be designated as "the swing shot," in which the gunner swings his gun ahead of the cross flight of the bird till he attains the proper distance ahead of it, and then fires; but he keeps his gun moving, with a regular angular velocity, till even after its discharge. This method of shooting is, in my opinion and from my experience, the proper method whenever it can be practised, and is certainly the only one which has been found successful in the shooting of bay fowl, as ducks, brant and wild geese. Yet there are sportsmen who will contend that they merely follow the bird with the gun, and discharge it while it is pointing directly at the bird. I put this opinion to the test this summer in the following manner: Four willets came over the decoys flying in line with a good speed. With my gun I followed the first bird coolly and accurately, and kept the gun moving regularly after its discharge. Instead of killing the first bird, the third from the leader dropped dead.

To give a rule applicable to all gunners, for the distance at which a gun should be held ahead of a bird, is not possible. Some sportsmen follow a bird, and then, after reaching before it the proper distance, suddenly stop the angular motion of the gun, and then fire. Others, after following the bird a short distance, give a quick, lateral motion to the gun, and then fire. Others, again, bring the gun, with a lateral motion, ahead of the bird and keep the gun moving till their experience decides the proper distance ahead of its flight, and then fire while the gun is keeping its previous, regular angular velocity.

For the simple illustration of the bearing of these experiments on the art of shooting on the wing, I will suppose that, at the moment of fire, the gun is stationary; in other words, that we are firing "snap shots." If the bird has a velocity across the line of sight of 30 miles an hour (*i. e.*, 44 ft. per sec.), and we are using charges in a 12-gauge gun of 3 1–4 drs. of Curtis & Harvey powder and 1 1–8 oz. of shot, we shall have to shoot about 5 feet ahead of the bird if it is flying at a distance of 30 yards; at 7 feet ahead if at a distance of 40 yards, and 11 feet ahead of the bird if at a distance of 50 yards.

These distances ahead, for cross shots at birds flying at the rate of 30 miles an hour, may appear out of all reason with the experiences of many sportsmen; yet, if you will place a stick 5 feet long at 40 yards, and ask the same gunners if they would hold ahead of a bird by that length, if it were going with a velocity of 30 miles, I venture to say, from my experiments with them, that they will say, "Of course: that is only about 18 inches;" so difficult is it to determine a length at a distance, while sighting along the barrel of a gun.

I will conclude with the remark that the study of this paper will not make a good shot on the wing, any more than a description of how to perform on the violin will make an accomplished violinist. But the results of these experiments are of value, and cannot but improve the shooting of an accomplished sportsman, if they are practically applied.

It has been said, perhaps rather strongly, "reading and writing come of schoolmasters, but a crack shot is the work of God."

APPENDIX.

"A bird at the distance of forty yards is going at full speed (say sixty miles per hour) in a direction perpendicular to the plane of fire; how much shall we hold ahead? It is difficult to find two good shots who will give, even approximately, the same answer to this question; there are those who will say they allow little or nothing, and others who allow from ten to fifteen feet.

It is obvious, that before we can discuss this question to any purpose, we must know approximately the velocity of a charge of shot. It is hoped, therefore, that the following experiments will be of interest to sportsmen.

These experiments were made on the 27th of June last, at the U. S. Naval Experimental Battery at Annapolis, Maryland, with the permission and able assistance of Commander Joseph D. Marvin, U. S. Navy, in charge of the Battery. The instrument used to determine the velocities was a Le Boulangé Chronograph; the gun, which was fired from the shoulder, was a pin-fire breech-loader, No. 12-gauge, thirty-inch barrels, weighing seven and a half pounds.

The chronograph was manipulated, and the observations were taken by Lieut. W. W. Kimball, U. S. Navy, assistant to Commodore Marvin.

The cartridges were not prepared for experimental purposes, but were such as the writer happened to have loaded at the time the experiments were made.

In the table below, H denotes Hazard's Electric Powder, No. 5 grain, and D denotes Dupont's Ducking Powder.

The column headed *Range* gives the distance between the two screens, one of which was about six inches from the muzzle of the gun.

The velocity given is the mean velocity of the shot between the two screens. When the range is fifty feet, it may be taken as the velocity at 25 feet from the muzzle of the gun; and when the range is 100 feet, at 50 feet from the muzzle.

The first screen consisted of four very fine copper wires placed vertically about half an inch apart; the second screen was about the size of a sheet of common note paper, and was formed of fine copper wire stretched from side to side at intervals of about one-fourth of an inch.

EXPERIMENTS.

Powder.	Drachms.	Size of Shot.	Ounces.	Range in feet.	Mean Velocity in feet per second.
H	2¼	2	1¼	50	1013
H	2¼	2	1¼	100	865
H	2¼	2	2¼	100	854
D	3	7	1¼	100	776
D	3	7	1¼	100	783
D	3	7	1¼	50	855
H	2¼	2	1¼	50	995

It will be noticed that the charges of powder were somewhat smaller than those commonly used by sportsmen.

If we assume the mean velocity of shot for forty yards to be 800 ft. per second, the time of flight will be 0.115 sec. A bird, flying at the rate of 60 miles an hour, goes at the rate of 88 ft. per second, or 13.2 ft. in 0.15 seconds. Again, if we assume the mean velocity of shot for forty yards to be 1,000 ft. per second, the time of flight will be 0.12 seconds, and the distance traversed by the bird, 10.56 ft.

Ducks, when going at full speed, attain a velocity exceeding sixty miles an hour; it would therefore seem that an allowance of 10 or 15 feet, in the case of a duck passing a point under full headway, is not too much, if the distance equals or exceeds 35 yards.

Some successful sportsmen have a habit of making an allowance for the velocity of the bird, by jerking the gun forward at the instant they pull the trigger.

The time which elapses between the instant at which the message which causes the finger to pull the trigger is sent from the brain, and the instant at which the shot leaves the muzzle of the piece, is finite, and doubtless greater than we are apt to suppose. This interval of time probably varies with different individuals; hence arises a larger personal equation. For this and other reasons it is impossible to frame rules which are equally applicable to all sportsmen. Each, if he would shoot well, must make his own rules, which must be carefully modified to accord with his experience and observation. A tolerably accurate knowledge of

the velocity of shot is, notwithstanding, of great service to the thoughtful sportsman.

In these experiments, the time of flight was such as to permit the shot to fall about four inches in going one hundred feet, a distance which is of little importance in comparison with the distance traversed by the bird while the shot is in the air. In the case of most double guns the elevation of the rib at the breech is more than sufficient to compensate for the distance through which the shot will fall in going forty yards. A sight one-tenth of an inch in height, at thirty inches from the eye, will exactly cover a line four inches in length at a distance of one hundred feet. The elevation of the rib of most double guns is greater than one-tenth of an inch."

U. S. Naval Academy, July, 1875. J. M. RICE.

[From the "Rod and Gun," July 31, 1875.]

A NEW METHOD OF OBTAINING A PERMANENT TRACE OF THE PLANE OF OSCILLATION OF A FOUCAULT-PENDULUM. By ALFRED M. MAYER, of Hoboken, N. J.

[ABSTRACT.]

LAST year, in the month of October, I mounted, in the physical laboratory of the Stevens Institute of Technology, a Foucault-pendulum formed of a cannon ball suspended by a steel wire. By floating the ball in mercury, I determined the point on the ball to which the wire should be attached, so that this point and the centre of gravity of the ball should be in the same vertical line. This line, having been prolonged as a diameter of the ball, determined the spot into which I screwed a pointed index. The point of this index, when the ball was stationary, was about 1-40 of an inch above a piece of smoked paper placed on a plate of metal which had been carefully brought into a horizontal plane. The pendulum was now drawn from the vertical by the tension of a delicate cord, one end of which was attached to the ball, the other fastened to a fixed support. The pendulum was started in the usual manner by burning this string. After a few oscillations, a current of electric sparks from an induction coil were passed through the suspending wire, and from the point of the index of

the pendulum through the smoked paper to the metal plate, and thus was obtained a trace of the path of oscillation of the pendulum. At successive and known intervals of time I obtained similar traces, which were rendered permanent by passing the smoked paper through spirit varnish. The intersection of these traces gives the centre around which the angles are measured.

The advantages of the method are: 1st, There is no friction of a tracing point; 2nd, The traces give the centre around which the angles are measured; 3d, The traces are permanent; and 4th, The angles of inclination of the traces can be leisurely and accurately measured.

Last May I described this experiment to Professor Cross of the Massachusetts Institute of Technology, and he then informed me that the same idea had occurred to him, though he had not put it in practice, and also that he had recently mentioned this plan of experimenting before the American Academy of Arts and Sciences in Boston; therefore his name and mine should be always associated in designating this method of obtaining a permanent trace from the Foucault-pendulum.

ON A SIMPLE MEANS OF MEASURING THE ANGLE OF INCLINATION OF THE MIRRORS USED IN FRESNEL'S EXPERIMENT ON THE INTERFERENCE OF LIGHT. By ALFRED M. MAYER, of Hoboken, N. J.

[ABSTRACT.]

THE inclination of the Fresnel mirrors is measured by pasting a disk of known size on a window pane, and then ascertaining the distance at which the disk, when viewed by reflection from the mirrors, appears double and the two disks are tangential to each other. From the known diameter of the disk, and the distance of the disk from the line of intersection of the planes of the mirrors, the angle of inclination of the mirrors is readily calculated.

New Planetary Nebulæ. By Edward C. Pickering, of Cambridge, Mass.

[ABSTRACT.]

During the past year, a search has been made for new planetary nebulæ with the large telescope of the Harvard College Observatory. A direct vision prism, placed between the objective and eyepiece, converts the image of a star into a colored line of light. A planetary nebula, on the other hand, being nearly monochromatic, appears as a point of light, and is distinguished at a glance from a star. Many thousand stars may be quickly examined in this way, and a single nebula selected from among them. Two new nebulæ have thus been found, having the positions, R.A. $18^h\,25^m\,10^s$, Dec., 25° 13′, and R.A. $18^h\,4^m\,19^s$, Dec., 28° 12′. Either of these objects would be mistaken for a star if examined in the usual way. On the evening of August 28, an object was found with a very singular spectrum. The light consists mainly of two bright bands, one in the yellow, a little more refrangible than the D line, the other in the green or blue. The approximate wave lengths are from 5,800 to 5,830, and from 4,670 to 4,730. There is also a faint continuous spectrum. This object cannot in other ways be distinguished from an ordinary star, and in fact, has been observed as one, by Argelander, and at the Washington Observatory, about thirty years ago. It does not seem to have changed since then in position or magnitude. It is designated as Œltzen, 17681, and its position for 1880 is, R.A. $18^h\,1^m\,17^s$, Dec., 21° 16′. The comparative faintness of the continuous spectrum makes it more nearly resemble a planetary nebula than a star, but since the bands are differently placed, the material must be different.

New Method for Finding the Numerical Roots of Equations below the Fourth Degree.

By James D. Warner, of Brooklyn, N. Y.

[ABSTRACT.]

This method is new for complete equations, and for those cubic equations where x appears in more than one term. For others it is not wholly new, but the arrangement is claimed as an improvement.

For finding the roots of the incomplete quadratic, the following rule is given, viz.:

Under the number write the figures of the root, commencing under the first figure of the last half, or of the larger latter part when divided as nearly as possible into two equal portions. Having ascertained the first figure of the root, take its square from the number, writing all of the different figures of the remainder over the number, in their proper numerical places.[1] Under the figure of the root, write its double, and with this as a trial divisor, find the next figure of the root. The square of this, together with the product of itself into the trial divisor, taken from the remainder, gives the next remainder. Adding the double of the last found figure of the root, to the line of the double of the root previously found, will give a new trial divisor, by which the next figure of the root can be ascertained. The same process pursued with this and each succeeding figure found, will finally evolve the whole root.

EXAMPLE.

$x^2 = 531441$: or extraction of the square root of 531441.

$$\begin{array}{r} 1 \\ 430 \\ 531441- \\ \hline 729 \\ \hline 144 \end{array}$$

Observe, that when any figure in the lines of remainders has been used, and there is no figure over it, or over a succeeding figure, to cancel it, by either striking a line through it or over it; also, whenever in doubling the root, there is anything to carry, to put it as a subscript to the previous figure, and use the sum of the two thereafter. Any remainder can be set off, by a long cancelling line through or over the next preceding figure on the upper

[1] In subtracting, add a unit to amount to be carried when the unit figure of the subtrahend exceeds the figure from which it is subtracted.

line of the remainders, or a dash with the remainder written after it, may be used.

The same method answers for finding the roots of the complete quadratic, by adding in the coefficient of the first power of x to the first trial divisor.

EXAMPLES.

$x^2 + 3x = 22348.$

23		2
1202		1269
22348		22348
148	or	−151
283		−197
91		−248
		− 9

$x^2 + 3x = 5.$

131		131
14939		14939
5.		5.
1.1926	or	−1.1926
428		−128
53		−53

If the coefficient of x^2 is other than unity, multiply each figure of the root by it, before every multiplication of the figures of the root.

EXAMPLE.

$2x^2 + 3x = 6.$

5
12562
6.
1.138
526
742
5

A nearly similar method is applicable for finding the roots of $x^3 = a$, or extracting the cube root.

RULE.

In extracting the cube root, the first figure of the root is placed under the first figure of the last part of the number, when the number of the figures have been divided as nearly as possible into three equal parts. The first line of figures under the root is three times the root, and is formed continuously from the root as found, just before obtaining the true divisor, and may be desig-

nated, for convenience, the line of triple products. The lines under the line of triple products are successive trial and true divisors. A trial divisor is always found by multiplying by the last found figure of the root into its double and that part of the line of triple products previously found, and adding the sum of the two products to the last true divisor. A true divisor is found by multiplying the last found figure of the root and that part of the line of triple products already found, by the trial figure of the root, and adding the products of the last trial divisor. This last sum, if found to be correct after testing, is to be multiplied by the found figure of the root, and the product taken from the remainder will give the next remainder.

EXAMPLE.

$x^3 = 387420489.$

PROCESS TO FIRST TRUE DIVISOR.	PROCESS TO SECOND TRIAL DIVISOR.	COMPLETE.
		1
44	44172	44172
387420489	387420489,	387420489—
72	72	729
21	21	216
147	147	147
5124	5124	5124
	552	55221
		747

When a cubic equation has been divested of its second term, and the coëfficient of x^3 has been reduced to unity, the same method is applicable by adding the coëfficient of the first power of x, to the first trial divisor.

EXAMPLE.

$x^3 - 12x = 28.$

1
667
129302
28.
430213
129
36
3969
4347
9

The complete Cubic Equation can also be solved, by observing, that before each multiplication of a figure of the root, it must be multiplied by the coëfficient of x^3, and the coëfficient of x^2 must be added to the line of triple products.

EXAMPLES.

$x^3 + 60x^2 - 800x = 60000.$

		4
		3212
		60000
1		30537
60		150
−800		5500
		75
		650

$8x^3 - 2x^2 + x = 8.$

		6
		1441
		132756
		8.
1		1.1418
−2		7,96
1		6
		73
		7491
		817
		813
		5

A similar method may be used for finding the roots of equations of a higher degree than the third, but as it is too complicated for ordinary mental operation, Horner's method is preferable; but it can be applied to the extraction of the higher roots, with a little subsidiary work.

EXAMPLES.

Fourth root of 252047376.

		—
		14468
		252047376
		126
		48
		6,64
		4
		5368
		6912
		7447896

Fifth root of 2196527536224.

		14
		18754126
		2196527536224
		294
		10,5
		4,0,1 0
		8,6,0,8 9 0
		80
		1923461
		3536405
		6353159056

Subsidiary work, to assist the mental operation, with explanation (see graphic explanation):

6561	$r \cdot r^3$	26244	$r \cdot 4r^3$	256	$4 \cdot 4^3$
7290	$5R \cdot r^3$	21870	$5R \cdot 3r^3$	9280	$5(R+r)\,3 \cdot 4^3$
3240	$10R^2 \cdot r^2$	6480	$10R^2 \cdot 2r^2$	134560	$10(R+r)^2 2 \cdot 4^2$
720	$10R^3 \cdot r$	720	$10R^3 \cdot r$	975560	$10(R+r)^3 \times 4$

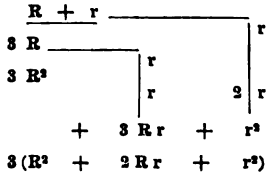
Sixth root of 1909037487458569.

1499	SUPPLEMENTARY.	SUPPLEMENTARY.	
3471749686		16807	7.7^4
1909037487458569	$2401 = 7^4$	734706	$(R+r)\ 7^4$
517	$7203\ 6R.7^4$	13382145	$(R+r)^2\ 7^3$
306	$14406\ 6r.7^4$	129997980	$(R+r)^3\ 7^2$
$37_{1}5_{5}15$		710346105	$(R+r)^4\ 7$
$25_{1}0_{5}0_{3}020$			
$93_{3}7_{7}5_{2}8015$		15	$6r.\ \frac{4}{2}r^5$
18750		300	$6R.\ 10r^3$
971287801		2250	$15R^2.\ 6r^2$
2070151506		7500	$20R^3.\ 3r$
14249955208631			

SUPPLEMENTARY.		SUPPLEMENTARY.	
117045	$3.15(R+r)^3$	132651	$50(R+r)^3$
156060	$40.15(R+r)^3$	2653020	$(R+r)^3$
117045	$300.15(R+r)^3$		

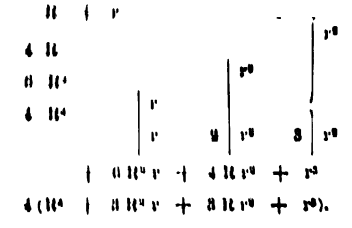
The following figures are intended to be a graphic explanation of the application of the method for the extraction of the higher roots. Of the roots as far as found, r represents the last figure, R the portion preceding. A quantity at the beginning of a bent brace is to be multiplied by the multipliers along the brace, the upper for the true, and the lower for the trial divisors. For the fourth, fifth, and sixth roots, supplementary figures are shown, which are to be used for obtaining quantities, which when multiplied by proper powers of the figure of the root, found by a trial divisor, are added to the successive trial divisors to make the true divisor.

THIRD ROOT.



The parts of the roots and the work are in position required by the method.

FOURTH ROOT.



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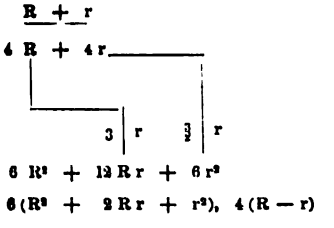
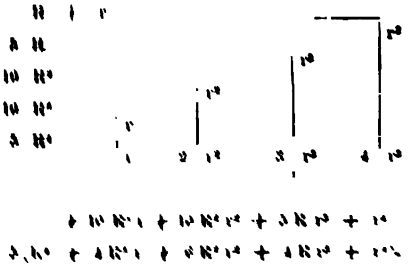
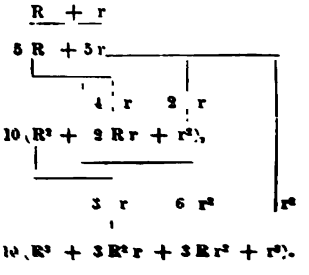


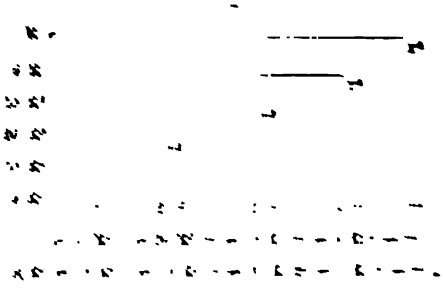
FIFTH ROOT.



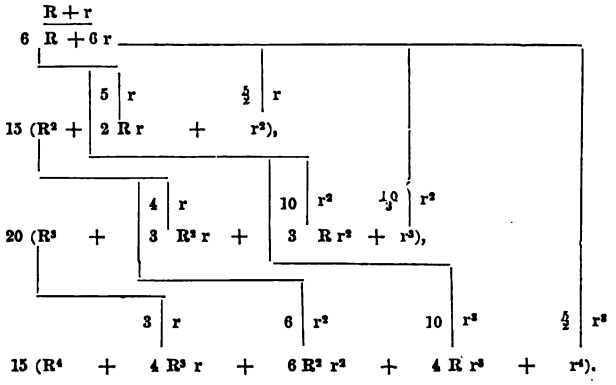
SUPPLEMENTARY.



SIXTH ROOT.



SUPPLEMENTARY.



The expressions found by the several multiplications can be seen to be the proper expressions which are sought. The upper one being the true divisor, and the lower, the trial divisor.

For extracting the nth root, the following is general. The multiplicands are the several powers of R below $n-1$ multiplied by the coëfficients of a binomial to the nth power. The upper expression at the end of the braces is the expansion of $(R+r)^n$ divested of its first term and divided by r. The lower expression is the expansion of $(R+r)^{n-1}$ multiplied by n. The upper set of multipliers are the several powers of r, in order below that of $n-2$ the higher one being repeated at last brace. The lower multipliers are the same powers of r as the upper ones multiplied by the digits in order. For the multipliers of a supplementary figure, the powers of r for the last term in each line agree in order with the number of the line commencing with the upper one: the coëfficients can be obtained by using the expression $\frac{(n-1)(n-2.....m}{2,3,4,5.....m}$, m being taken equal with the exponent of r of the multiplier. For the other multipliers, the exponent of r in each agrees with the formed columns in order, and the coëfficients of the columns are the same as the following differential series in respective position.

$n-1$
$n-2$
⋮ ⋮ ⋮
5 15 35.....
4 10 20.....
3 6 10 15.....etc.

It can readily be seen how each series is formed.

Division is a process of evolving one factor, where another factor and their product are given. The method proposed is applicable, and its advantages are: that there will be no such thing as Long Division, the division by large numbers being as easy as dividing by a single figure; that fewer figures are used; and that the result is in its proper numerical position.

EXAMPLE.

Divide 64852 by 31.

```
  2 6
31)64852
  2092
```

The divisor can be placed at the right as is the custom of the French.

EXAMPLE.

```
  —
  2
 1131
 16778727
1234560789 | 234
  527593   27
           234
```

RAILWAY TIME SERVICE. By P. H. DUDLEY, of New York, N. Y.

CORRECT time signals from various observatories have been daily sent over the telegraph wires of several railways for many years past, for the purpose of keeping all the clocks of a line alike. If the clock was in error the operator at each station must set it. Practically, the operators did not set their clocks if within a minute or two of correct time.

The problem was to arrange a practical system so that all of the clocks upon a line would be under the direct control of the Train Despatcher, though one or any number of miles away.

I have solved this problem by taking an ordinary clock with pendulum and weight, and arranging an electro magnetic apparatus, so that at given times the hands could be set. One of these clocks is

put in each station. By a three point switch, one side of which is connected to the clock, the other side to the sounder, and a wire to the relay, the clock or sounder may be put in circuit at pleasure. A few seconds before the time of sending the time signal as received from the Observatory, the Train Despatcher calls up his line to *switch the clocks into circuit;* and, when he receives the correct time signal he sends one dash to all the clocks, correcting them if either *fast* or *slow*. The clocks are now switched out, and the sounder put in, thus using existing telegraph wires only.

When this was successfully accomplished a demand was at once made for clocks beating seconds, to have the second hand corrected, at the same time of the minute hand, and for the past eighteen months, many clocks have been put in various railway stations, giving the public the benefit of Observatory time to the second. From many interviews with railway officials, I believe they will be ready to adopt a uniform standard of time for the entire country whenever the public demand it.

This question is so important, involving so many interests, it seems to me it should further engage the attention of the eminent men of this body, who have already done so much in this field, so that we may have the most practical solution of the question possible.

TITLES OF OTHER PAPERS READ IN SECTION A.

LECTURE EXPERIMENT FOR THE DIRECT DETERMINATION OF THE VELOCITY OF SOUND. By W. A. Anthony, of Ithaca, N. Y.

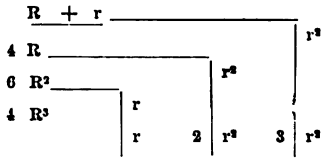
A LECTURE EXPERIMENT, SHOWING THE MOVEMENT OF A HORIZONTAL CURRENT IN THE EARTH'S MAGNETIC FIELD. By W. A. Anthony, of Ithaca, N. Y.

A PROPOSED IMPROVEMENT IN THE CONSTRUCTION OF THE GRAMME ELECTRIC MACHINE. By W. A. Anthony, of Ithaca, N. Y.

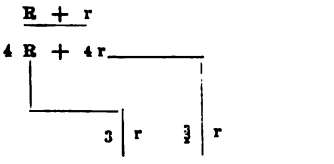
A TABLE OF REMAINDERS OF 2^n TO VARIOUS PRIME MODULI. By E. P. Austin, of Boston, Mass.

THE ASTRAL LANTERN. By F. H. Bailey, of Hillsdale, Mich.

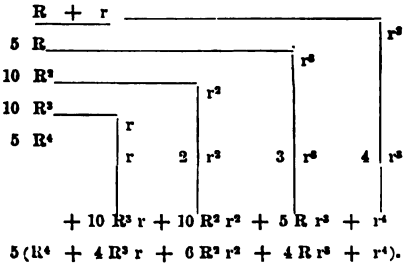
FOURTH ROOT.



SUPPLEMENTARY.



FIFTH ROOT.



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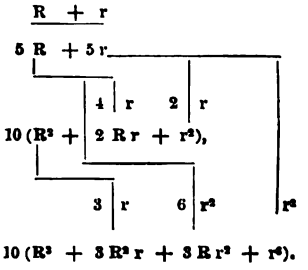
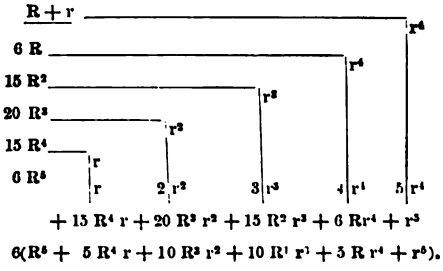


SIXTH ROOT.



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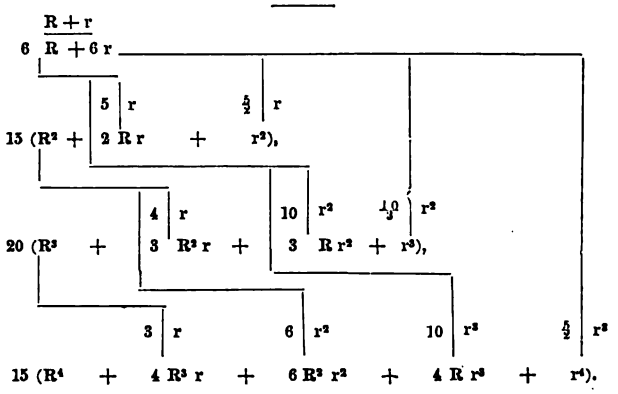

The expressions found by the several multiplications can be seen to be the proper expressions which are sought. The upper one being the true divisor, and the lower, the trial divisor.

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$n-1$
$n-2$
⋮ ⋮ ⋮
5 15 35.....
4 10 20.....
3 6 10 15.....etc.

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THE COEFFICIENT OF EXPANSION OF GAS SOLUTIONS. By E. L. Nichols and A. W. Wheeler, of Baltimore, Md.

RESULTS OF A MAGNETIC SURVEY OF MISSOURI. By Francis E. Nipher, of St. Louis, Mo.

UNITY, INVERSION, AND SEMI-INVERSION IN LINEAR ASSOCIATIVE ALGEBRA. By Benjamin Peirce[1], of Cambridge, Mass.

USEFUL PRACTICAL FORMS OF LINEAR ASSOCIATIVE ALGEBRA. By Benjamin Peirce, of Cambridge, Mass.

COMETS OF MINIMUM PERIHELION DISTANCE. By Benjamin Peirce, of Cambridge, Mass.

COOLING AND POSSIBLE AGE OF THE SUN. By Benjamin Peirce, of Cambridge, Mass.

A GAUGE FOR MEASURING THE PRESSURE OF LIQUIDS AND GASES. By R. H. Richards, of Boston, Mass.

PROGRESS MADE AT THE OBSERVATORY OF HARVARD COLLEGE IN THE DETERMINATION OF THE ABSOLUTE COÖRDINATES OF 109 FUNDAMENTAL STARS. By W. A. Rogers, of Cambridge, Mass.

A SIMPLE AND EXPEDITIOUS METHOD OF INVESTIGATING ALL THE DIVISION ERRORS OF A MERIDIAN CIRCLE. By Wm. A. Rogers, of Cambridge, Mass.

THE SYSTEMATIC ERRORS OF THE GREENWICH RIGHT ASCENSIONS OF SOUTHERN STARS OBSERVED BETWEEN 1816 AND 1831. By Wm. A. Rogers, of Cambridge, Mass.

PRELIMINARY DETERMINATION OF THE EQUATION BETWEEN THE BRITISH IMPERIAL STANDARD YARD AND THE METER OF THE ARCHIVES. By Wm. A. Rogers, of Cambridge, Mass.

THE PROBABLE ERROR OF A SINGLE OBSERVATION AT SEA, DEDUCED FROM THE OBSERVATIONS OF W. H. BACON, CUNARD STEAMER "SCYTHIA." By Wm. A. Rogers, of Cambridge, Mass.

THE ERRORS OF A FEW ENGLISH, FRENCH, AND AMERICAN STAGE MICROMETERS. By Wm. A. Rogers, of Cambridge, Mass.

INVESTIGATIONS FOR RAPID AND SAFE LOCOMOTION. By Clinton Roosevelt, of New York, N. Y.

[1] These papers were read by Professor Peirce some time before he was ill, and which he hoped to have ready for the press; but he was unable to complete his investigations, the subjects which he undertook [illegible]

REMARKS ON C. S. PEIRCE'S PAPER ON THE GHOSTS IN RUTHERFORD'S GRATING. By H. A. Rowland, of Baltimore, Md.

ON A CONTINUATION OF ARGELANDER'S DURCHMUSTERUNG. By Ormond Stone, of Mount Lookout, Ohio.

ON THE CONSTRUCTION OF A MICROMETER FOR DOUBLE STAR OBSERVATIONS. By Ormond Stone, of Mount Lookout, Ohio.

ON THE DRAWING OF NEBULAE. By Ormond Stone, of Mount Lookout, Ohio.

ON UNIFORM TIME. By Ormond Stone, of Mount Lookout, Ohio.

HEAT PRODUCED BY MAGNETIZING AND DEMAGNETIZING IRON AND STEEL. By John Trowbridge, of Cambridge, Mass.

METHODS IN USE AT THE OBSERVATORY OF YALE COLLEGE FOR THE VERIFICATION OF THERMOMETERS, AND THE TESTING OF TIME-PIECES. By Leonard Waldo, of New Haven, Ct.

THEORY OF THE TIDES. By James D. Warner, of Brooklyn, N. Y.

"ACTIO IN DISTANS" AND ITS EFFECTS. By J. D. Warner, of Brooklyn, N. Y.

ON THE REFRACTIVE INDEX OF METALLIC SILVER. By A. W. Wright, of New Haven, Ct.

ON A FORM OF VACUUM TUBE FOR SPECTROSCOPIC WORK. By A. W. Wright, of New Haven, Ct.

SPECTROSCOPIC NOTES: BEING A NOTICE OF CERTAIN SPECTROSCOPIC OBSERVATIONS, PRINCIPALLY SOLAR, MADE IN 1879 AND 1880. By C. A. Young, of Princeton, N. J.

THE THERMO-ELECTRIC ELECTRO-MOTIVE POWER OF IRON AND PLATINUM IN VACUO. By C. A. Young, of Princeton, N. J.

Many of the above papers have been printed since the meeting in the AMERICAN JOURNAL OF SCIENCE, the AMERICAN JOURNAL OF OTOLOGY, and in SCIENCE.

PERMANENT

SUBSECTION OF CHEMISTRY.

ADDRESS

OF

PROFESSOR JOHN M. ORDWAY,

CHAIRMAN OF THE SUBSECTION OF CHEMISTRY.

GENTLEMEN OF THE SUBSECTION OF CHEMISTRY:—

IT is well for us to pause sometimes in the midst of our busy and wearisome labors and consider the progress of the past, the wants of the present, and the promise of the future. It is best for us to have both humiliation and encouragement mingled in just proportion, and a clear-eyed outlook is always sure to show room for both.

Chemistry as an art and Chemistry as a science jointly occupy fields so extensive and so diversified that we can hope to make but a very partial survey in the time allotted to a discourse, and I trust that no one will feel aggrieved, if, in this imperfect sketch, the fruits of his own "little field well tilled" shall receive a mere glance or even escape notice altogether.

The past year has been one of laborious and fruitful activity in various departments of chemistry, but we cannot yet see that the year is marked by any of those epoch-making discoveries that have sometimes startled the world, and sometimes have come almost as silently as the dew of night to refresh the fields of science. Thus in 1807, when Davy, with his magnificent voltaic battery, isolated potassium and closed the record in his note book with the triumphant exclamation "Capital experiment,—proving the de-

composition of potash!" mankind applauded and reëchoed "Capital experiment!" In 1807, too, friend Dalton's atomic theory was quietly made known to the world and for a time it was quietly received. It was chiefly a theory to be sure, but it had a long leverage and a solid fulcrum, and it moved science to its very foundations. No other chemical theory has borne equally well the test of time. The ποῦ στῶ, the standing place, has been shifted a little by the discovery of allotropic states and isomerism. Even elementary atoms are not now allowed to exist singly, but must be linked into molecules. Dalton's theory has been expanded and developed; there is, at present, no sign, no prospect of its being superseded. It seems barely possible that Meyer's new method of investigating dissociation may lead to some further step in the evolution of Dalton's theory. Meyer's recent apparent resolution of the chlorine molecule has not, indeed, been verified in the carefully devised experiments of Crafts, but Crafts does seemingly confirm the change of iodine by an intense heat. Further trials are needed, and till they have been made with bromine and chlorine, and have given unquestionable, positive results, we cannot judge whether 1879 and 1880 will rank hereafter as years in which Meyer found means to throw new light on the nature of the haloids.

Within the past year Cooke has further discussed the atomic weight of antimony; and his many experiments have been carried out with a patience and thoroughness and skill that will enable them to endure any reasonable criticism. It were well, if some one would study bismuth and tin with the same conscientious exactness.

Twenty-four years ago, Perkin sought for artificial quinine and found instead a better than royal purple. Then, by various hands and in rapid succession, red and yellow and black and brown and blue and green were brought out from what proved to be something more than aniline. And now the novelty is past and the announcement of a new dye creates hardly a ripple of excitement, unless it can be shown to have some new and desirable qualities. Yet real and wonderful progress continues to be made in this grand branch of industrial chemistry. The twelve years old synthesis of alizarine has given us colors for cotton goods purer, brighter, faster, and cheaper than those of the now obsolescent madder root. Of late, wool has been provided for, and the extinction of cochineal plantations is threatened by reds of surpassing

brilliancy, durability, and ease of application, but with such dysophonous and long trailing designations as "betaoxyazonaphthaline-sulpho-acid."

Recently we have been told that Bayer has succeeded in the long sought, economical synthesis of indigo, and there is no good reason to doubt it; but it would greatly assist our faith to see and handle the product. The problem which appeared simple twenty years since has hitherto proved difficult and evasive, and the dye which is among the first to have been expected is really one of the last to appear. Tropical indigo plantations may in time share the fate of the madder fields of France and Turkey. But indigotine itself will not continue to satisfy the demands of an exacting age. Rosaniline and eosine, anisol red, Hoffmann's violets and malachite green, have accustomed us to hues of a delicacy and richness that no one dared dream of twenty-five years ago. So the æsthetic nicety of this generation has been too much pampered, and dyers will soon be calling for something uniting the brilliancy of the aniline blues, by gaslight and daylight, with the fixedness of indigo and its equal adaptedness to wool and cotton.

It is interesting to recall how, when aniline colors were new, a British chemist prophesied that England instead of the tropical countries would thereafter supply the dyestuffs of the world, from fossil crops. Alas, for human foresight! In fact, the fugitive colors made from aniline and its homologues have become of secondary importance and new ones which know not aniline have come to the front. In fact, it is not England but Germany that shares most largely in the world's present enormous production of coal tar colors. The country which has done most towards studying out the illimitable transformations of the carbon compounds is furnishing the most of the industrial fruits of those seemingly unpractical researches, and is thus doing most to vindicate the pursuit of theoretical science. Investigation costs, investigation pays. In more senses than one, our science "opens wide her ever during gates on *golden* hinges turning."

Still it must be confessed that, out of the hundreds of new organic substances which are studied out beforehand and built up with so much pains, very few have any reference to our material wants or throw any new light on the dark places of science. It is not unfair to say that a very large part of the chemical researches

which are made nowadays serve little other purpose than to educate the investigators themselves. It might be quite as well, even for mere intellectual training, to select for study more of the subjects which really require elucidation.

The passing years are occasionally bringing to light additional elementary bodies. But new metals are becoming somewhat like the new asteroids of astronomers, which have too little mass to influence the orbits of other bodies, and are too much out of sight to concern many besides their discoverers. Within five years, fourteen new metals have called for recognition, and names have been found for all except one, without resorting to the Grecian list of goddesses, furies, muses, and nymphs. In 1879 alone, chemists have claimed the discovery of six; Scandium, Norwegium, and Barcenium, by their sensible properties; and Holmium, Thulium and Samarium, by their spectral probabilities. These, with Philippium, Mosandrium, Ytterbium, Neptunium, Lavoisium, Davyum, Gallium, and the older Ilmenium, if they all gain a footing, will serve mainly to increase the census of the metals to seventy-seven, and to burden our memories with symbols of things remote from daily life. None of them have such novelty of character as to require the remoulding of chemical hand-books. A more general, a more practical interest attaches to the fuller determination of the properties of long known and not uncommon elements,— to the production of useful alloys,— and to great improvements in the working of the older metals.

It is noticeable that, after trying for a long time to set aside the Berzelian idea of glucina, chemists are now coming round again to the adoption of $Be_2 O_3$ instead of BeO. Having had, years ago, something to say about a new point of resemblance between this earth and alumina, I am personally pleased to see a return to what seems to be the right view.

Of new alloys, manganesian copper seems worthy of regard, since it may in some measure play the part for copper that spiegel iron does for steel; being valuable for what it removes rather than for what it adds. The one word which best recalls our progress in civilization and the useful arts is "iron;" and it is spiegel iron which has rendered successful the grand iron and steel making inventions of Bessemer and Martin. Indeed it is not unlikely that manganese has always been essential for the production of the so-called natural steel. But even manganese is powerless to exorcise

the evil spirit of the Bessemer converter. Many men have sought for the philosophers' stone which shall eliminate phosphorus and ennoble a cold short metal, and at last there is hope that it will prove to be magnesian lime.

As for the noble metals themselves, silver is becoming so ignobly cheap, that we could give but a cold welcome to inventions which may increase the facility of its production, so long as it continues to be used for money. There is room to consider whether the inimitably light metal aluminium might not be used for coin.

The past decade has been a period of centennial and semicentennial commemorations; and the people whom this Association are now favoring with their presence have taken a notion to celebrate the quartomillennial of the settlement of this *quondam* peninsula of Shawmut. Next month Bostonians will be dwelling in thought and speech on Puritan ideas and Puritan deeds. Possibly, we all, as free men and as chemists too, have some interest in that olden time. The men who settled these shores were representatives of iconoclastic progress. The leaven which was working in men's minds in those days was the Puritan idea of the right to question human authority in church and state and in the domain of learning, and the duty of yielding obedience to the divine, the higher law. Not Puritans alone, but men of various names, were feeling the impulses which led to the founding of new states, and new sciences. The relation of those times to religion and government is a theme for the orator and the poet. But what can be said of science in the first half of the seventeenth century?

In 1620, Francis Bacon published the second part of his "Instauratio Magna" or Restoration of Learning, otherwise called "Novum Organon Scientiarum," wherein he pointed out the way to appeal to nature by experiment, instead of deriving all science from the teaching of the ancients. He handled without mercy the groundless reasonings of the old philosophers and their syllogistic deductions from uncertain premises. Bacon's spirit gradually spread, as we may see by the very name of the Florentine "Accademia del Cimento," Academy of Experiment, founded in 1651. But Bacon's methods had little immediate influence on the science of the time. He relied on induction, and induction alone only strings together dry bones. That perception of general principles which makes science, that planning of *experimenta crucis*, guide-board experiments, which test conjectures and make firm the

bases, come not altogether from the mere collation of facts. The Organon, besides a regulator, needs a motive power. We need something more than eyes to see. "But there is a spirit in man, and the inspiration of the Almighty giveth them understanding." We should make little progress in the discovery of truth, without those suggestions which come to the groping investigator, he knows not how or whence, like lightning flashes to show him the onward path.

The great chemist of two hundred and fifty years ago was John Baptist van Helmont. He, being himself of noble family, as the record reads, married a noble, rich and pious wife, in 1609, and thereafter devoted himself to chemistry. He distinctly recognized that there are other aëriform fluids than common air, and to him we owe the conception and the generic name of gas. "Halitum illum Gas vocavi, non longe a Chao veterum secretum." The word was not derived from "Geest," as is often said, but from "Chaos," as representing the original form of matter.

At the time when our forefathers were laying the foundations of this nation, alchemy was in its garrulous dotage and decline, and chemistry took its rise in the dim knowledge of the gases. A hundred years ago our country was passing through a political revolution. One hundred years ago chemistry was going through its revolution, and this revolution was brought about by a closer study of the gases. It is not long since American chemists celebrated the centennial of Priestley's completed isolation of oxygen; and they did well to honor the memory of Priestley, for he did great service in bringing out plainly what had been neglected or but darkly known before. But it is only by a gush of enthusiasm that we can call the discovery of "dephlogisticated air" the birth of modern chemistry. The evolution of chemistry as a science is threefold. First came the study of the gases, dating back to Van Helmont; then the study of heat, dating back to Beecher and Stahl; and finally the study of combining weights, starting with Wenzel and Richter. Consider how much of what we now know and do depends on those "airs" that Cavendish, Black, Scheele, and Priestley revealed. The study of combustion, respiration, vegetable growth, organic decay, geological transformations, and hygiene, involves the study of carbon dioxide. Our knowledge of dissociation started from the decomposition of calcium carbonate by heat. Carbon monoxide reduces the metals; it gives us

cement steel; it affords us an economical hot blast; it plays a part in the Bessemer process; it renders possible the Siemen's furnace. The fuel of the future is to be coal resolved into a chaos of carbonic oxide and hydrogen. When hydrogen was first made manifest it was thought that phlogiston itself had become tangible. But phlogiston would not endure even this attenuated materialization, and not long after the yokefellow of hydrogen was found, the Stahlian phlogiston took its flight to the limbo of exploded ideas. "Dephlogisticated air," "vital air," the producer of calces, the product of a calx ignited, "oxygène," the supporter of combustion, is also a factor in organic life and in brain work. "Fire damp" is the evil spirit that haunts the mines still, and still shocks us occasionally with his misdeeds. But we have partially tamed the demon and turned his power to account in boiling salt, puddling iron, and giving light. At the end of the last century Murdock found a use for coal-gas, and latterly, in its lengthening train, came the host of secondary products which have had such a marvellous effect on science and industry. Chlorine is a potent element in modern manufactures. To produce the most intense heat we combine two gases. To produce the most intense cold, we liquefy or solidify a gas and then let it recover its original form.

Heat came into chemical science when Beecher devised an immaterial sulphur to explain combustion, but heat was always concerned in chemistry as an art; and chemistry as an art is almost coëval with mankind. Chemical art began when Prometheus brought down fire from heaven. Man contrived to keep this fire alive and make it serve him; but Zeus was angry and bound Prometheus himself, and, as the Roman poet tells us, shut up the seeds of fire in the veins of flint. So carrying about fire in a hollow reed became a lost art not to be revived till the present century, and long after Brandt had discovered phosphorus. Man is the chemical animal, for he alone uses fire. Other creatures can spin, weave, sew, and exercise the plastic art; can build houses, mine the earth and execute skilful works of hydraulic engineering. But man alone dyes tissues, bakes pottery, cooks food, and excavates with explosives. Men made glass three thousand years ago, about as they do now. Vulcan of old made as good iron as the blacksmith requires to-day, and all our modern improvements have not quite extinguished the bloomary forges of primitive times.

As for quantity, to be sure, Vulcan, with all his sweating Cyclops and the fires of groaning Ætna, could not produce as much in six days as the Cambria Iron Works turn out every six minutes. Could Dante's guide see that Alleghanian Avernus with its miles of underground drifts, its coke ovens, its blast furnaces, its puddling furnaces, its Bessemer converters, its coal-fed Cyclops, instead of leisurely rolling out the rhythmic ring of his old anvil chorus "Olli inter sese magna vi brachia tollunt," he would recall again the humming hive, and exclaim with hurried breath "Fervet opus." The American iron master literally as well as figuratively makes the "work boil."

The chemical arts, shortly after the time of Van Helmont, were represented by Johann Rudolf Glauber who made many useful and lasting inventions. We still make "spirit of nitre" and "spirit of salt" by Glauber's method, and the ammonia process is yet far from superseding the production of soda by means of Glauber's salt cake. The chemical arts remained as he left them, for nearly a hundred years, till Roebuck, in 1746, gave a new start by devising leaden chambers for the manufacture of sulphuric acid. Glauber with all his good sense in practical things had a lingering belief in the absurdities of the alchemists, for he wrote "de Medicina Universali sive Auro potabili Vero;" he wrote also "Concerning the Rise and Origin of all Metals and Minerals, how indeed they are produced by the stars, take to themselves a body from water and earth and are fashioned in divers forms." From this little book we learn, what was the common belief of that day, that the rays of the sun and stars bury themselves in the earth and finally become silver and gold. But perhaps these old writers were prophets speaking by symbols which they understood not. Now we know that metallurgy does depend on the sun's rays. The sunshine of the carboniferous period has been materialized into coal-beds and now attains perfection in a metal of more real value than gold.

As to heat in its more strictly scientific relations, we may say that it began to form a part of chemistry when Beecher propounded the rudiments of the theory which Stahl afterwards worked up into a consistent scheme of chemical philosophy. For almost a century this system satisfied the moderate demands of science; and then, in the revolutionary age, Lavoisier dethroned Stahl's phlogiston and set up another one in its place, one of more posi-

tive character, called "caloric," or "the matter of heat." Priestley's simple, "dephlogisticated air" became a compound of oxygène and caloric. Before another hundred years could run their course another phlogiston has set aside Lavoisier's and oxygen has become a mass of molecules swimming in a sea of this "ether." But whatever be the nature of heat, its phenomena are capable of quantitative investigation. And through the labors, first of Lavoisier and Laplace in 1780, and since of Despretz, Regnault, Favre and Silbermann, Andrews, Berthelot, and Thomsen, thermochemistry has been placed on a solid and definite basis. Berthelot's late work on Thermochimie shows culminating progress, and is worthy of him who, years ago, almost created organic synthesis. In 1798, Rumford published the *experimentum crucis* which showed that heat is not matter. But men were slow in getting back to the prephlogistic idea, and it was fifty years before the world came to see the full force of Rumford's conversion of mass motion into molecular motion, or work into heat. And now who can devise an *experimentum crucis* to show whether heat is motion of ordinary or of extraordinary matter? Is this idea of an extraordinary, unatomic, continuous matter a necessity of science; or may a special luminiferous, coloriferous, actiniferous, caloriferous, electriferous, magnetiferous ether be dispensed with? Why should we not add soniferous and nervomotoriferous, to these multifarious capabilities? If nature still abhors a vacuum, as it used to, and the interspaces of ordinary molecules must be filled up by something, as that something is not molecular, it must be incompressible and inelastic. If it is inelastic it is incapable *per se* of oscillatory, vibratory, undulatory, or pulsatory motion. If, on the other hand, ether is molecular, why should not ordinary matter in a hypergaseous state answer the requirements? Chemists do not venture to put down universal ether in the table of elementary substances. In fact, it seems to be incompatible with the very convenient atomic or molecular theory, and either the atomic theory or the ether theory should be thrown overboard. But if you admit the physical universe to be made up of atoms, and intramolecular and intermolecular motion, and an ethereal *tertium quid* through which motion is propagated, you need something more to explain how or why one kind of motion is transmuted into the other, how heat becomes or produces light and how resisted mass motion produces heat. When men attempt to explore the universe,

thought becomes fatiguing and they substitute mathematics for thought. Were Bacon living at the present day, he would add to the idols of the tribe, idols of the den, idols of the market, and idols of the theatre, still another kind which have led men astray, —the idols of disembodiment. There is a strong tendency to interchange the material and the mathematical, to talk of particles as though they were points, to speak of their describing lines when they can only describe cylinders or prisms. The old corpuscular theory was a physical absurdity because it failed to consider that corpuscles must have size, and a fixed size. Corpuscles could fill all space only by expanding as they go and forming hexagonal pyramids. The undulatory theory may be more consistent, and yet it is exceedingly hard to comprehend; at any rate the language used respecting it does not indicate very precise views. Isolated particles may *oscillate*, strings may *vibrate*, material surfaces may *undulate*, but no one has ever devised a word to indicate those quaquaversal, centrifugal pulsations which some suppose to constitute heat. If heat is the motion of atoms, it is oscillation. If it is the pulsation of ether, it is not exactly undulation. Instead of conceiving of light or heat in spherical space, the physicist differentiates it and reasons about rays, and seems to consider rays as disembodied mathematical lines. It needs the habit of experiments to keep one from soaring away into the vague empyrean. The mathematician may arrive at wonderful results by speculating on matter deprived of materiality, but nature brings his devices to naught by friction. The engineer calculates the right size of his iron, but omits the supervision of the work, and the bridge, that according to mathematics must stand, falls by reason of a clumsy weld. It is only incorporeal abstractions which can be proved by mathematics. The physical must be proved by appeals to the senses. Experiment and observation show us that the absolutely exact is unattainable by us. Thus men with great skill, labor and expense have attempted to find a unit of measure which may be referred to a natural standard, and have after all settled down to a really arbitrary, material rod, of which copies shall be made and handed down to the coming generations. And it is now a problem for chemists to find for such rods a suitable substance that will undergo the least secular variation in length. We have come to see that there is no absolute, unchanging solid. The differences between the solid, liquid, and gaseous states of

matter would seem to be those of degree rather than of kind. Andrews showed ten years ago that a liquid may become a gas without change of bulk. It is found that there is no need of the ingenious hypothesis of regelation to account for the closing up of the path which a wire cuts through ice. Ice is really plastic like pitch; and a new word is not called for to express the viscosity of ice. According to the molecular theories of the present time there should be no perfect stability, motion being the universal law. There is a tendency to indefinite expansion and when different substances are brought in contact, their molecules tend to interpenetrate. Dalton discovered this with regard to gases, early in this century. Graham afterwards investigated further the diffusion of gases and found that liquids also slowly intermingle. A chapter still remains to be written on the diffusion of solids. When we put together ice and salt there is a mutual reaction and a liquid is formed, no matter how cold the weather is. It is strange that no one has ever attempted to explain this well-known fact. The phenomenon becomes more striking when we mix two salts, one of which contains water of crystallization. It was found, some years ago, that solid ferric nitrate would react strongly on bicarbonate of ammonium; but here the stronger acid might have an attraction for the stronger base. In another experiment ferric nitrate and sodium sulphate, both hydrated, were found to liquefy when they were brought together. Strong affinities could not be concerned here, though both acids and bases were different and there might be double decomposition. Later experiments, never yet made known to the world, have shown that chloride of sodium and sal soda make a self-liquefying mixture. It still remains to be seen whether any wholly anhydrous bodies can be found which will melt each other. Of course the mere word *diffusion* does not explain the action of such freezing mixtures; but it may serve provisionally to point out in what direction the explanation is to be sought.

There is another form of diffusion in which a solid septum co-operates and yet is not itself changed. Osmose is well known as one of the chief agencies or phenomena of organic life. And this leads to the consideration of biological chemistry, a branch which has yet received but little development. Osmose is thought to account for many vital operations, but osmose is not precisely the

same in the living as in the dead cell. Some chemists, thinking that atomic oscillations will account for everything, have hooted at the idea of any peculiarity in vital action. They would make out that things grow merely by the joint action of the correlated molecular motions under favoring conditions; but there is just the same heat, light, electricity, moisture, and oxygen working on the dead as on the living. Life itself is the chief condition. There is something which transforms light and heat into the right kind of chemical force or motion. Cells grow no doubt by chemical action, but what limits their growth, what makes them divide and thus multiply? What makes one cell produce another unlike itself? Whence come the partitioning of functions and the assignment of these functions to particular cells? Why do cells extend in certain directions, as if according to a predetermined plan? Why is the leaf of the oak sinuated and the leaf of the willow entire? Why does the stem grow week after week and produce nothing but ordinary leaves and branches, and then, as if at an appointed time, concentrate all its forces to form the floral whorls? Why does the matured seed show no sign of life till spring, though you plant it, water it, and warm it in early winter? Is the little protoplasmic mass of a primal germ cell in possession of free will and foreknowledge absolute? Oh, no! its immense possibilities and potencies are said to come from heredity. Is heredity a compacted molecular force? There must be something besides matter and motion, something which gives direction and directs according to a plan foreordained. The director is life. Life is not a force, but it controls and directs the forces. Whence comes the plan by which it works? There is a point of progress in thought, at which "tendency to variation," "struggle for existence," "natural selection" become empty words.

But with the essence, the ontology of the hyperphysical and metaphysical chemistry is not concerned. With the sensible phenomena of life, however, chemists have much to do. Suppose we need acetic acid. We plant seeds that have life. We make sure that the soil shall contain what vigorous life calls for. The living germs run their course and give being to other seeds, "some forty, some sixty, and some an hundred fold." We induce the new germs to sprout a little and they transform the starch into glucose. We call in the aid of other living cells to break up the glucose

and give alcohol. And, finally, still another living thing constrains the alcohol to oxidize and furnish the desired product. Again we boast much of the synthesis of a valuable organic dyestuff, as though life had nothing to do with it. But when we trace all the steps and sum up, it comes to "these are the spores that formed the ferns that made the coal that produced the tar that yielded the anthracene that gave the bibromanthraquinone that turned into the alizarine that Graebe and Liebermann built." Let us not be too proud of our power of dispensing with life. Furthermore, let us not be too proud of our power to reason out and then build up organic compounds. Graebe and Liebermann's was surely a grand discovery with which accident had nothing to do. Was it indeed? We now know that alizarine has at last seven isomers, and therefore it was by a rare stroke of good luck that the German chemists hit upon the one chance in eight of getting what they were seeking for.

Physiological or biological chemistry is yet in its infancy and but comparatively few tenderly and assiduously care for it. It has much to do with indefinite, unstable, evasive colloids, and most chemists prefer to take easier subjects and study the definite and crystallizable. The interest in biological study is, however, increasing, and there is much patient work done and progress made in the chemical part of the subject. The field is large and there is room for many courageous, persevering laborers. Proximate organic analysis still remains undeveloped. The world does not comprehend the light that we already have. Thus the books still call the ultraviolet of the spectrum "the chemical rays," though Draper, thirty years ago, showed that it is yellow light which is most efficient in reducing carbonic acid with the help of chlorophyll. Surely the assimilation of carbon in all the forests and prairies and cultivated fields is of more importance than the reduction of silver salts in a few photographic studios.

In fermentation, putrefaction, nitrification, and in zymotic diseases vegetative life may intervene, but how much do we yet know as to what is cause and what is merely concomitant? When the *Merulius lacrymans* makes its troublesome ravages, does its mycelium ramify infinitely into and around the wood cells and devour their strength with some corrosive secretion or some oxidizing exhalation? Or does it merely afford just the right supply of moisture

to favor atmospheric oxidation? One of our laboratories here has lately furnished most excellent specimens of this unwelcome intruder, which appear to countenance this latter view. Certainly the hydraulic arrangements of the fungus are well calculated to distribute more water than is needed for its own growth and yet allow a free access of air to the dampened support.

It is possible that biologists at present tend towards extremes in referring so many things as they do to the presence of fungi, which may be the consequence rather than the cause of certain conditions. We should always remember that a problem is far from being solved when we have simply worked out the value of y in terms of the still unknown, and perhaps unknowable x.

It may be pertinent to inquire whether chemistry tends, as many assume that all physical science now tends, to materialism. I believe no true science tends that way: it is rather the lack of really liberal cultivation that causes such dimness of vision. Surely materialism is no more prevalent now than it was among the ancient Athenians, who had no physical science. We hear much said of the learning and culture of that polished people,—and some of their modern imitators; just as though æsthetics were the only science, and floriculture the only culture!

Doubtless heredity influences personal belief, and the blood of those seventeenth century colonizers still flows in human veins; blood that is thicker than vapid, all attenuating water. But there is much in the training of the chemist to foster a wholesome skepticism and a just intolerance; intolerance of human pride and human dicta; skepticism of the airy theories of pseudo-science. We would by all means welcome free thought; but let it not be the reproduction of other men's opinions; let it not be mere ciphering with algebraic symbols; let it not be ridicule and confident assertion, the glittering weapons of fools; let it be thought and not the spurious semblance of thought. There is a higher standard than ranting declaimers go by. "Ἡ ἀλήθεια ἐλευθερωσε ὑμας." "Ye shall know the truth, and the truth shall make you free."

In chemical practice, the constant appeal to sensible tests and to the precision of the balance checks reliance on hasty assumptions. The stern rule that the sum of the parts must equal the whole, that percentages must count up one hundred, is rigidly en-

forced and the discipline is severe but strengthening. The chemist soon learns that exact truthfulness in others and rigid honesty in himself lie at the very foundation of science and real knowledge, and he cannot help looking on laxity in experiment or statement as the unpardonable sin.

Again, the history of chemistry gives little encouragement to conservatism, for perhaps no other subject is so well calculated to impress one with the idea that theories are but the changeable dress of science. "Is not the life more than meat, and the body than raiment." The phlogistic, dualistic, caloric, electrochemical, atomic, nucleus, radical, mechanical, catalytic, and biological theories have all had their uses for a time, they have all aided the progress of science, but in the chemistry of to-day we find only well trimmed shreds of most of them. The review of the enthusiastic but unstable past shows how often men have remained satisfied with explanations which were not reasons. We all wonder what will have become of the atomic theory itself, when the centennial of its promulgation comes round, twenty-seven years hence.

But when we rise above the reactions of brute matter and the play of insensate forces, and are insensibly led on to look into vegetative life, irritability, sensation, perception, thought, the transmission of thought, the self-analysis of thought, and the prime origin of thought, all theories, actual or possible, fail to satisfy. It requires something more than lenses and chemical changes of the retina to see. A noise may ring the tympanum and not be heard. We cannot trace the connection between phosphorus and sparkling wit. Physical science can explain only the tools of thought and cannot explain all there is of them. There is no peculiarity in the matter of the printed page. Its supposed atomic motions give no clew to its marvellous power. As we turn the leaves of the oldest scripture in existence, to the eye alone there is only paper and carbon; yet the alphabetic symbols convey something which is not of them, but beneath, beyond, and above them. Through them we see a grand mirror of human trial. To the ear there is no sound, yet this æonian telephone brings down to us the voice of Zophar the Naamathite uttering the interrogative negation, "Canst thou by searching find out God?" and we hear the yearning cry of Job, "O that I knew where I might find him." Then by "the true Light which lighteth every

man that cometh into the world," we perceive that the revelation of Him as something more than the vague mainspring of creation, as a personal being who still lives and cares for every one of us, comes not by induction, not by syllogistic reasoning, not by algebraic analysis, not by synthetic philosophy, not in acts of terror, not in works of power. It comes in the still, small voice which he who will may hear.

PAPERS READ.

Laws governing the Decomposition of Equivalent Solutions of Iodides under the influence of Actinism. By Albert R. Leeds, of Hoboken, N. J.

In a paper published in the Philosophical Magazine for June, 1879, I have given a brief review of the controversy as to whether potassium iodide, in very dilute solution, is decomposable by sulphuric acid. I likewise pointed out that the explanation of the opposite views, entertained by experimenters upon this question, was due to their having overlooked the essential part played by air or oxygen in this reaction. This last was brought to view by Baumert[1], in the course of experiments by which he showed that Andrews[2], in the famous investigation undertaken to prove that Baumert's hypothesis that electrolytic ozone is a teroxide of hydrogen[3] was false, had himself fallen into an error. For Baumert showed that when a stream of electrolytic ozone has been deprived of all of its active oxygen, by passage through a *neutral* solution of iodide of potassium, it may bring about a liberation of iodine in an *acidified* solution, placed later in the series, many times greater (from four to ten in the experiments tried) than that effected by the ozone itself in the first instance. So the curious fact remains, that while Andrews' main conclusion is true, all the results by which he succeeded in establishing it are affected by a constant error and are in excess of their true values. The triumph of Andrews' opinion (1856), that ozone contains no hydrogen whatsoever, but in its substance-matter is identical with the matter of ordinary oxygen, probably explains why the permanently valuable part of Baumert's work has been generally lost sight of; and why the erroneous method, of titrating ozone with an acidified solution of potassium iodide, has been persisted in even down to the present day. Ten years after the facts above stated were made known by

[1] Pogg., Ann., XCIX, p. 88.
[2] R. Soc. Proc., VII, p. 475; Pogg., Ann., XCVIII, p. 435.
[3] Phil. Mag., VI, p. 51; Pogg. Ann., LXXXIX, p. 38.

Baumert, they were rediscovered by Payen [4], who extended their application to the action of nitric, acetic, oxalic and other acids upon dilute solutions of potassium iodide, in and out of contact with air.

In former papers [5], a summary of which is given in the Phil. Mag. (*loc. cit.*), I have shown that the presence of oxygen not merely facilitates, but is absolutely essential to, the occurrence of the reactions in all cases, and whether the reaction occurs in open or closed vessels, in the heat or cold, in darkness or in light. The only exception to this law is in the case of an acid like nitric, which, under the influence of light (the action of heat alone in the absence of light and oxygen is being investigated), spontaneously breaks up, and supplies by internal change the essential oxygen. The experiments made to determine the rates of decomposition, when various iodides in the presence of different acids were submitted to the influence of the magnesium, electric and solar rays, were made with solutions of known though not of chemically equivalent strength. To supply this defect a new series of experiments were performed immediately after the publication of those cited above, and it is to make known certain remarkable laws of actino-chemical change deduced from these latter experiments, that the present article is written.

The solutions were of such strength that 1 cc. of each of the acids employed was chemically equivalent to 12.6 cc. of a normal caustic soda solution; the iodides were each exactly equivalent to the 20 per cent. solution of the potassium iodide. 1 cc. of the iodide and 1 cc. of the acid were used in each trial, the volume of the test being made up to 100 cc. by the addition of distilled water. The tests were contained in "comparison-tubes" made of thin, perfectly colorless glass of uniform bore and dimensions, which were supported on frames in such a manner that each tube should be normal to the incident rays, and in the case of the magnesium and electric lamps, six inches from the focus of the light.

The first set of trials was made to determine, whether the addition of starch as an indicator facilitated the decomposition under the influence of light, as had been originally supposed, or whether, as later on, there had been reason to think, it retarded the reaction.

[4] Compt. Rend., LXII, p. 254.

[5] Proc. Amer. Chem. Soc., 1878, II, No. 4; Jour. Amer. Chem. Soc., 1879, I. p. 18; *ibid.*, p. 65.

In this, as in succeeding experiments, the amounts of iodine liberated are given in milligrammes.

Effect of Starch upon the Rate of Change (March 18, 1879).

Brilliant Sun. Reagents.	1.30–2.30 P. M. 5 cc. starch-water.	2.30–3.30. 30 cc. starch.	3.30–4.30 No starch.
H_2SO_4 + KI	0.48	0.51	1.65
HCl "	1.18	1.46	3.50
HNO_3 "	3.10	2.57	3.50
H_2SO_4 + CdI_2	0.49	0.82	1.65
HCl "	1.90	1.11	3.55
HNO_3 "	2.80	2.83	3.40
H_2SO_4 + LiI	0.34	0.40	0.30
HCl "	0.59	0.72	1.75
HNO_3 "	2.70	3.86	2.75
H_2SO_4 + KI	0.48		
March 19, 12 M.–1 P. M. Feeble sunlight.			
H_2SO_4 + KI	0.26		1.8
HCl "	0.40		2.4
H_2SO_4 + CdI_2	0.22		1.6
HCl "	0.40		2.2
H_2SO_4 + LiI	0.20		1.2

The experiments of March 18th, performed as they were with a sun approaching the horizon, having been less decisive than could have been desired, they were repeated upon the following day, but with the disadvantage of feeble sunlight, with the result of showing that six times more iodine was set free in the absence than in the presence of starch. The suspended precipitate of starch iodide cut off the light except upon the superficies of the solution. Henceforth the employment of starch was abandoned.

The next set of trials was instituted in order to observe the influence upon the rate of change, of larger access of oxygen than that derivable from the air already dissolved, or in contact with the solution at its upper surface. To this end tests were prepared in duplicate, and through one pair of these duplicate solutions, the comparison-tubes being connected together in the manner of wash-bottles, a slow current of oxygen was passed.

Influence of increased supply of Oxygen.

March 19, 1879.	11 A. M.–12. With Oxygen.	Feeble sunlight. Without.
H_2SO_4 + KI	7.15	2.9
HCl "	10.60	4.3
Also with Nitric Acid.	1 P. M.–2.	Snowing at time.
H_2SO_4 + KI	5.25	1.5
HNO_3 "	6.75	2.5

The absorption of oxygen, when sulphuric and hydrochloric acids are employed, is expressed by the equations: —

(1) $4\,KI + 2\,H_2SO_4 + O_2 = 2\,K_2SO_4 + 2\,H_2O + 2\,I_2$, and
(2) $4\,KI + 4\,HCl + O_2 = 4\,KCl + 2\,H_2O + 2\,I_2$.

And since 100 cc. of water, when saturated with air under the ordinary circumstances of temperature and pressure, would hold in solution only about 1 mgrm. of oxygen, the maximum amount of iodine which could be liberated during one of these tests, in case no fresh absorption of oxygen took place from the upper surface, would be 16 mgrms. The influence of these conditions upon the accuracy of the estimations made with the Iodo-acid Actinometers, is being submitted to further investigation.

In the case of nitric acid, the reactions become much more complicated. For in the first place, of the three mineral acids, nitric is the only one which spontaneously decomposes, when subjected to sunlight in closed vessels. This is true both of the concentrated acids, and when diluted with 500 times their volume of water. Moreover, in the latter case, the presence of starch had no influence except in the nitric acid, in which it nearly doubled the rate of decomposition. The same effect of starch (whether it is true of organic matter in general has not been determined) is to be noted in the above table of decompositions for March 18th, in which it will be seen that, in the trials where starch was present, the amounts of iodine liberated by nitric acid were largely in excess of those set free by equivalent amounts of the other acids. In fine, while nitric acid conforms to the general law of actinic change as expressed in the equation, $4\,KI + 4\,HNO_3 + O_2 = 4\,KNO_3 + 2\,H_2O + 2\,I_2$, it is likewise subject to the special decomposition, $4\,HNO_3$ (in sunlight) $= 2\,N_2O_4 + 2\,H_2O + O_2$. For these reasons, at an early stage of the inquiry, the actinometric use of nitric acid was discontinued, until the exact influence of temperature, actinism, oxygen and organic matter, upon its special rate of change, had been established, and the nature of the accompanying reactions.[6]

In order to study the influence of organic acids, trials were made as above, the solutions being of such strength that 1 cc. of

[6] Gay-Lussac states (Ann. de Chem. et de Phys., 1816, p. 317), that no decomposition of dilute acid takes place in the light except in the presence of a certain quantity of concentrated sulphuric acid. Also that the decomposition is into nitrous acid.

each was chemically equivalent to the same amount of the mineral acid.

Effect of Organic Acids.

March 20, 10.10 A. M.–1.10 P. M. Good sunlight.

Oxalic	Acid +	Potassium Iodide	= 6.5	mgrms.	I.
"	"	Cadmium	"	= 6.0	"	"
"	"	Lithium	"	= 5.75	"	"
Tartaric	Acid +	Potassium	"	= 0.10	"	"
"	"	Cadmium	"	= 0.00	"	"
"	"	Lithium	"	= 0.00	"	"

Nor was there any change with acetic acid during this three hour interval. To determine more precisely the rate of change effected by the organic, as compared with the mineral acids, it will be necessary to make simultaneous estimations, but even with oxalic acid, the most active of them all, it is evident that the amount of decomposition was relatively small.

An actinometric measurement of the solar ray was made with the equivalent solutions of various iodides and acids, the principal object being to note the effect upon the rate of decomposition of different acids in the presence of the same base, and of different basic radicals of the iodides in the presence of the same acid. The sunlight, which was good during the early part of the day, declined towards noon, and shortly after the sky became overcast.

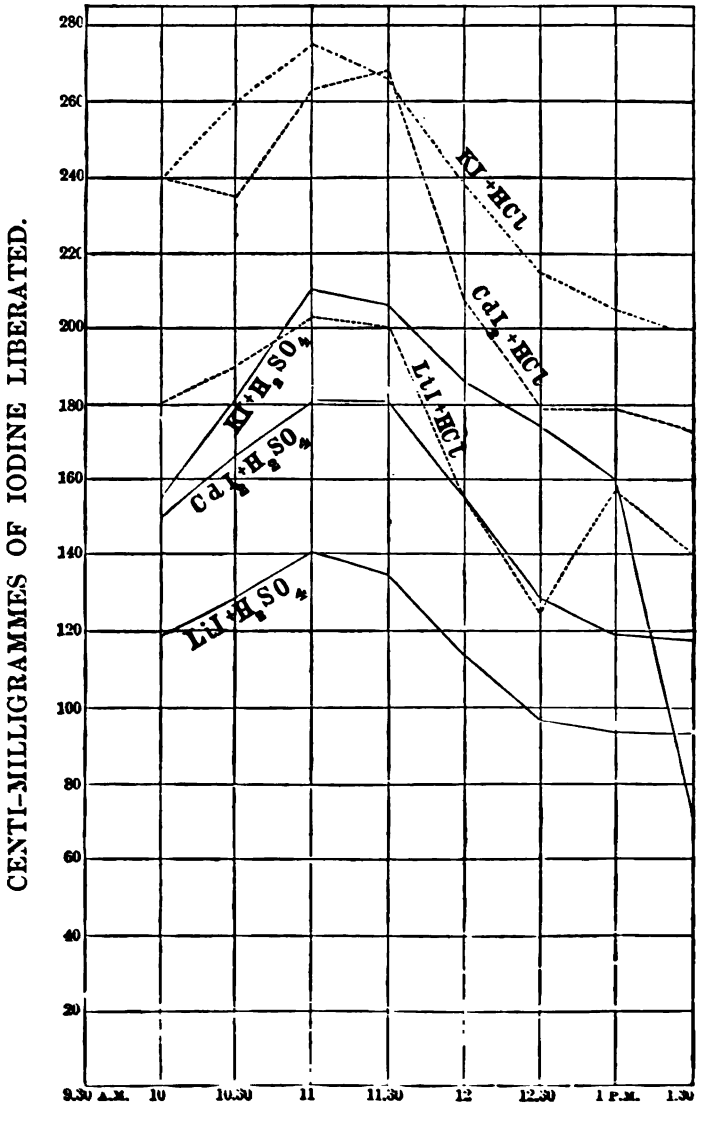
ACTINOMETRIC MEASUREMENT OF THE SOLAR RAY, March 26, 1879.

	9.30–10 A.M.	10–10.30	10.30–11	11–11.30	11.30–12	12–12.30	12.30–1	1–1.30	Means.	Ratios.
H_2SO_4 + KI	1.55	1.81	2.1	2.06	1.87	1.7	1.6	0.75	1.68	H_2SO_4: HCl
HCl "	2.4	2.6	2.75	2.66	2.39	2.15	2.05	2.0	2.50	1: 1.48
H_2SO_4 + CdI_2	1.5	1.62	1.81	1.81	1.56	1.29	1.19	1.18	1.49	H_2SO_4: HCl
HCl "	2.4	2.35	2.63	2.68	2.09	1.79	1.79	1.75	2.18	1: 1.46
H_2SO_4 + LiI	1.18	1.28	1.41	1.35	1.13	0.96	0.93	0.93	1.15	H_2SO_4: HCl
HCl "	1.83	1.9	2.03	2.0	1.56	1.25	1.58	1.4	1.69	1: 1.47

DIAGRAM I.

ACTINOMETRIC MEASUREMENT OF THE SOLAR RAY,

March 26, 1879.



On comparison of the arithmetical means of the results obtained in fifty-four trials made during the course of the same day, the striking fact is brought out that the amounts of iodine, liberated by the two acids in the presence of the same base, stand in a constant ratio to one another. The law of actinic force herein indicated may provisionally be expressed by the formula — the chemism of the chlorine radical is to that of the $\frac{1}{2}$ SO_4 radical (measured by the relative amounts of iodine liberated by each respectively in solutions of the metallic iodides exposed to sunlight), as 1.47 : 1. There is reason for inferring from the experiments, that a similar definite ratio exists between the amounts of iodine liberated from different soluble iodides in the presence of the same acid.

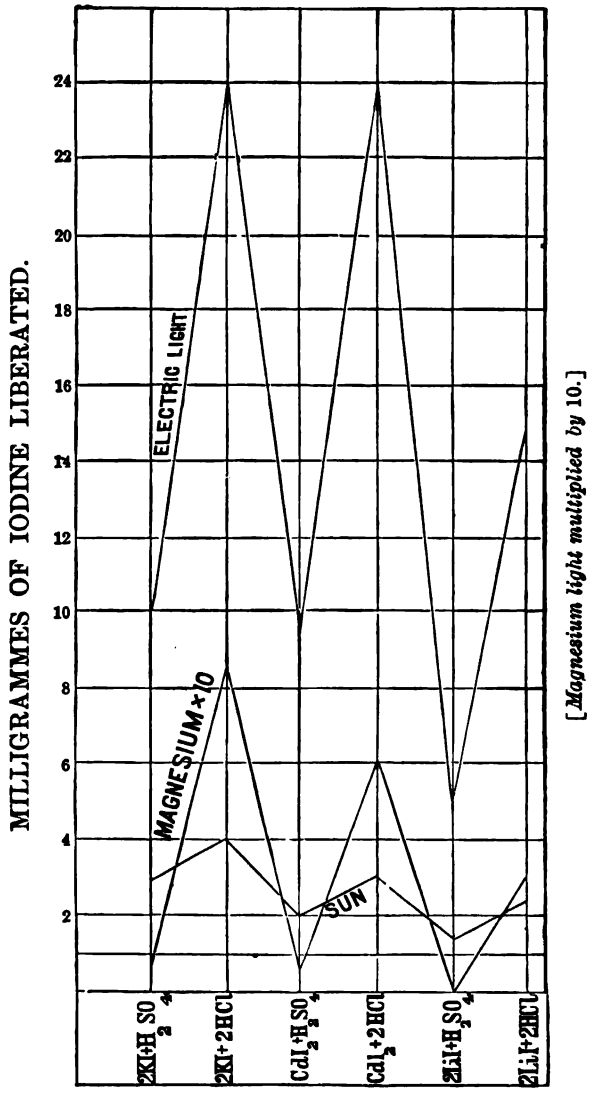
Comparison of the Actinic Intensities of the Solar, Electric and Magnesium Light.

In making this comparison, the amounts of iodine liberated at the hour of maximum actinic intensity (1–1.30 P. M.) were taken in the case of the sun. The electric light was that emanating from a lamp of 7000 candle power, falling upon the solutions at a distance of six inches; the magnesium light that derived from the burning of a single ribbon in the ordinary lamp, placed at the same distance. The time of exposure to the electric light was ten minutes: to the magnesium light twenty-five minutes: to the sun one-half hour: in the table, all are calculated to one hour.

	Solar.	Electric.	Magnesium.
$H_2SO_4 + KI$	2.70	10.0	0.084
HCl "	4.00	24.0	0.87
$H_2SO_4 + CdI_2$	2.04	9.5	0.072
HCl "	3.00	24.0	0.6
$H_2SO_4 + LiI$	1.44	5.0	none
HCl "	2.40	15.0	0.30

DIAGRAM II.

COMPARISON OF THE ACTINIC INTENSITIES OF THE SOLAR, ELECTRIC, AND MAGNESIUM LIGHT.



Calculated to One Hour.

On examining this table, and still more readily, the graphic illustration accompanying it, two phenomena become forcibly manifest:—1st. The very much greater actinic intensity of the electric as compared with the solar ray, when compared in the manner indicated, and the very much less intensity of the magnesium.[7] 2nd. Instead of the relative chemism of the chlorine atom, as estimated by its iodine-liberating power under these conditions, being 1.5 in all three cases, it is 2.4 to 3 with the electric, and 10 with the magnesium light. The examination of these differences, as related to the actinic forces of different sources of light and to different acid and basic radicals, is being further investigated.

Finally, the influence of absorbing media upon the invisible rays of the sun and electric light was determined, the comparison-tubes being surrounded with a thickness of 3 c.m. of ammonio-sulphate of copper, neutral potassium chromate and fuchsine, each solution being brought to the same apparent degree of translucency, for the blue, yellow and red respectively. The exposure to the sun was from 9.30 A.M. to 5.30 P.M.; to the electric light 20 min.; but both are reduced to the interval of 1 hour.

		Sun.	Electric Light.
Blue.	H_2SO_4 + KI	0.74	1.80
	HCl "	1.12	6.75
Yellow.	H_2SO_4 + KI	0.11	0.00
	HCl "	0.25	0.125
Red.	H_2SO_4 + KI	0.28	0.60
	HCl "	0.56	2.25

These figures show that the selective action of absorbing media upon the invisible rays of different illuminants varies greatly, and suggests the employment of this method for the mapping out of absorption spectra for the actinic portion: Stevens Institute of Technology, June, 1880.

[7] In order to bring the magnesium curve into the same diagram as the others, the numbers in the magnesium column in the table were all multiplied by ten.

The Rotatory Power of Commercial Glucose and Grape Sugar. A Method of Determining the Amount of Reducing Substance present by the Polariscope.

By H. W. Wiley, of Lafayette, Ind.

In "the trade" the name "Grape Sugar" is applied only to the solid product obtained from corn starch. On the other hand, the term "Glucose" is given to the thick syrup made from the same material. I shall use these words in their commercial sense.

Instruments Employed.

I used in the following investigations two polariscopes, made by Franz Schmidt and Hænsch, Berlin.

The readings of these instruments, after correction for displacement, agreed well together.

The one was the instrument ordinarily used, in which the purple ray is employed and the quartz half moons give blue and red tints.

The other was the Laurent "half shadow."

Both of these instruments are graduated to read 100 divisions each equal to 1 per cent. sugar with a solution containing 26.048 grams. pure cane sugar in 100 cc.

In addition to this scale the "half shadow" has another which gives the actual angular rotation. This is especially convenient when the specific rotatory power of a substance is to be determined.

The angular rotation however can be calculated for the former instrument.

For if we take the specific rotatory power of pure cane sugar at 73°.8 we have the following equation:

$$73°.8 = \frac{a \times 100}{2 \times 26.048}, \text{ whence } a = 38°.45$$

Each division on the cane sugar scale is therefore equal to 0°.3845 angular measure.

This quantity corresponds to the transition tint. It is different for the differently colored rays.

In the "half shadow" polariscope, an instrument particularly adapted to persons afflicted with any degree of color blindness, the mono-chromatic light, coming from the sodium-Bunsen lamp, passes through a crystal of acid potassium chromate.

The ray thus produced is less rotatable than the "transition tint." When the instrument gives 100 divisions on the sugar scale, it shows an angular rotation of only 34° 42′ = 34°.7.

One division therefore of the sugar scale is equal to 0°.347 angular measure.

To determine the specific rotatory power of cane sugar for the sodium-acid potassium chromate ray, we use the following equation.

$$\text{Sp. rot. pr.} = \frac{34°.7 \times 100}{2 \times 26.048} = 66°.6$$

To determine the specific rotatory power for any other substance which has been determined for the transition tint, we multiply by the factor .9024.

Thus, if we take the specific rotatory power of dextrin for the transition tint at 139°, for the "half shadow" tint it will be 139° × .9024 = 125°.4.

These data rest upon the accepted formulæ;

$$(1)\quad \theta = \frac{a}{\epsilon . \delta . \lambda .}$$

$$(2)\quad \theta = \frac{a \times V}{\lambda \times \omega .}$$

Here a = angular rotation.
" θ = specific rotatory power.
" ϵ = amount of substance in one gramme of the solution.
" δ = specific gravity of solution.
" λ = length of observation tube.
" ω = weight of substance in grammes.

Material.

The glucoses studied in the following examinations were made by the Peoria Grape Sugar Company. I am under obligations to Mr. Wm. Allen, the superintendent, for many favors in connection with my work.

The grape sugars were made in Buffalo.

Rotatory Power.

The average value of θ for the "half shadow" ray is nearly 85°. For the purple ray it is nearly 94°. It however varies extremely in different samples.

The following table will show the range of these variations:—

Showing Variations of θ in different Specimens of Glucose and Grape Sugar, together with the Specific Gravities of the same.

TABLE I.

No.	θ	Sp. gr.	No.	θ	Sp. gr.
1	91.50	1.406	11	89.26	1.416
2	91.50	1.407	12	87.73	1.422
3	98.10	1.440	13	89.77	1.417
4	79.93	1.414	14	70.84	1.463
5	75.47	1.414	15	69.40	1.463
6	83.97	1.417	16	87.07	1.412
7	82.75	1.416	17	109.99	1.427
8	86.41	1.415	18	93.17	1.431
9	84.11	1.416	19	89.75	1.409
10	87.19	1.417	20	91.31	1.421

From a study of this table it is seen that within small limits *θ* is independent of the specific gravity of the solution.

Nos. 14 and 15 were grape sugar and the specific gravity is much higher here than in the glucoses, while the value of *θ* is much less.

Where the increase in density however is considerable, as in 3 and 17, there is also a marked increase in the value of *θ*, although this increase is not proportional to the increment of specific gravity.

In masses of homogeneous nature and structure we should expect *a priori* that *θ* would always be proportional to the density of the body, *i. e.*, to the amount of optically active matter in a unit number of grammes.

It is thus seen without further argument that commercial glucoses are not optically homogeneous, even when made in the same factory and by processes which do not sensibly vary. A further study of these optical reactions convinced me that the rotatory power of commercial glucose increased, as the percentage of reducing substance diminished. The following table shows the value of *θ* and the corresponding percentage of reducing matter as obtained by Fehling's solution.

TABLE II.

No.	θ	Per cent. reducing matter.	No.	θ	Per cent. reducing matter.
1	91.50	53.20	11	89.36	53.50
2	91.50	52.36	12	87.73	56.49
3	98.10	54.60	13	89.77	52.36
4	79.93	61.73	14	70.84	69.93
5	75.47	62.50	15	69.40	69.30
6	83.97	59.35	16	87.07	56.34
7	82.75	61.40	17	109.99	39.22
8	86.41	58.80	18	93.17	57.14
9	84.11	58.55	19	89.75	54.37
10	87.19	55.60	20	91.31	56.81

It will be seen by the above table that, as the per cent. of reducing matter increases, the value of θ diminishes, and *vice versa*. Nos. 14 and 17 show extreme cases of this law.

Nos. 3, 18 and 20, because of their high specific gravities should not be included in the above generalization.

Having thus established the law that the per cent. of reducing matter is in general inversely as the value of θ, it is next proposed to investigate the relation between these two quantities and determine whether it is constant or variable. From Nos. 1 and 2 of table II, it is seen that for a value of $\theta = 91.50$, the percentage of reducing matter is 52.78. Let us say for convenience in calculation that $\theta = 91.50$ corresponds to reducing substance $= 53$ percentage. Let us consider next some of the cases in which the value of θ differs widely from 91.50. No. 17 of above table affords an example.

The difference is $109.99 - 91.50 = 18.49$. The difference in the percentage of reducing substance is $53 - 39.22 = 13.78$. Thus an excess of the value of θ of 18.49 corresponds to a deficit of 13.78 in the percentage of reducing matter.

Therefore a variation of each degree in the value of θ is equivalent to .745 in the percentage of reducing matter. By similar calculations with the other data furnished by the table I have found, not including Nos. 3, 18 and 20, marked by the high specific gravity, that this number lies between .75 and .78.

I will give the calculations for the first of these numbers and compare them with the numbers obtained by analysis.

TABLE III.

No.	Variation of θ.	Per cent. reducing matter by .75 factor.	Per cent. reducing matter by Fehling's solution.	No.	Variation of θ.	Per cent. reducing matter by .75 factor.	Per cent. reducing matter by Fehling's solution.
1	0	53.	53.20	11	— 3.77	55.82	56.49
2	0	53.	53.36	12	— 1.73	54.29	56.18
3	—12.08	63.06	61.78	13	—20.66	68.32	69.93
4	—10.03	65.03	63.50	14	—22.10	69.56	69.30
5	— 7.53	58.64	59.35	15	— 3.83	55.88	56.09
6	— 8.75	59.57	61.40	16	+18.49	39.14	39.22
7	— 5.07	56.81	58.80	17	— 1.75	54.32	54.05
8	— 7.44	58.58	58.55	18	— .19	53.14	56.81
9	— 4.30	56.21	56.60	19	+ 6.60	47.15	54.60
10	— 2.14	54.62	53.50	20	+ 1.66	51.75	57.14

In the above table, Nos. 18, 19 and 20 are the three specimens with high specific gravities; we will, therefore, exclude them from the discussion.

In the other numbers the percentage of reducing matter, as calculated from the readings of the polariscope, falls short of the amount obtained by the alkaline copper test ten times, and by an average of 1.018. It exceeds that amount seven times, with an average of .75.

We thus see that the polariscope will enable us to compute the reducing matter present in a glucose with a probable error of less than one per cent. This is quite accurate enough for practical purposes.

Perhaps a larger number of determinations should be made before constructing a formula for determining the amount of reducing substance in a "straight" glucose.

The following formulæ, however, are given provisionally, subject to some slight correction derived from more extended data.

We may have three cases:

1st. The value of $\theta = 91.50$.
2nd. " " " " < 91.50.
3rd. " " " " > 91.50.

In the first case, the percentage of reducing substance in a glucose of not far from 1.410 specific gravity will be nearly 53.

In the second case, the required percentage may be found by the following formula, in which a = difference between the value of θ and 91.50 and g = percentage reducing substance required.

$$g = 53 + .75a \text{ or } .78a$$

In the third case we have

$$g = 53 - .75a \text{ or } .78a$$

In order to make this principle of more general application, I have modified the calculations so as to apply the formulæ directly to the cane sugar scale of the instrument. To this end, for instruments using 26.048 grammes for 100 divisions, it is convenient to use only 10 grammes of the glucose. If 26.048 grammes are employed, the neutral point is thrown entirely beyond the limit of the scale. Ten grammes is the quantity which has been employed in the following table. The average reading of the sugar scale for ten grammes is about 50. In Table IV (p. 314), will be found the results of my experiments.

The calculations (Table IV, p. 214) were made from the following data.

Starting with Nos. 1 and 2 and discarding small fractions, we find that 53 divisions of the cane sugar scale correspond to 53 per cent. of reducing matter nearly.

By a method of calculation entirely similar to that employed in determining the reducing matter from the fluctuations of θ, I have found that a variation of one degree in the sugar scale corresponds to an inverse variation of nearly 1.25 per cent. of reducing substance.

We may have, as in the previous calculation, three cases.

1st.	The	reading	of	the	scale	$= 53$
2nd.	"	"	"	"	"	> 53
3rd.	"	"	"	"	"	< 53

In the first case $53 = 53$ per cent. nearly.

In the second case placing a for the reading of the scale we have

$$g = 53 - (a - 53)\ 1.25$$

In the third case we have

$$g = 53 + (53 - a)\ 1.25$$

TABLE IV.

No.	Scale.	Per cent. reducing substance by calculation.	Per cent. same by copper test.	Difference.
1	52.65	53.43	53.20	+ .23
2	52.65	53.43	52.36	— 1.07
3	46.07	61.66	61.73	— .07
4	43.05	64.90	62.50	+ 2.40
5	48.04	58.75	59.35	— .60
6	47.70	59.63	61.40	— 1.77
7	49.80	57.00	58.80	— 1.80
8	48.45	58.56	58.55	+ .01
9	50.26	56.45	55.60	+ .85
10	51.50	54.88	53.50	+ 1.30
11	50.57	56.04	56.49	— .45
12	51.74	54.58	56.18	— 1.60
13	40.83	68.21	69.93	— 1.72
14	40.00	69.25	69.30	— .05
15	50.53	56.09	56.34	— .27
16	63.80	39.50	39.22	+ .28
17	51.73	54.37	54.05	+ .32
18	52.63	53.46	56.81	
19	56.53	48.59	54.60	
20	53.70	52.10	57.14	

In seven of the first seventeen cases the percentage of reducing substance, calculated by the above formulæ, exceeded that given by the copper test, and by a mean amount of .539.

In ten of them it fell short and by a mean amount of .938. This method, therefore, can be relied upon to give results which vary from the copper test only by a small amount.

Not much more in the way of accuracy can be claimed for the copper test itself.

In Nos. 18, 19 and 20 we have again the cases where the high specific gravities vitiate the results of the calculation.

Correction for Specific Gravity.

I next proceeded to investigate a method for correcting the reading of the polariscope for variations caused by changes in the

specific gravity of the specimens. First, I determined the percentage of water in glucoses of different specific gravities.

Following are the results :—

I.

Specific gravity = 1.440
Weight taken = 5.515
Loss = .350 at 170°, 2 hours.
Per cent. H_2O = .85 + 5.515 = 6.37

II.

Specific gravity = 1.431
Weight taken = 5.86
Loss = .53, 170°, 2 hours.
Per cent. H_2O = .53 + 5.86 = 9.05

III.

Specific gravity = 1.409
Weight taken = 4.038
Loss = .622, 170°, 3 hours.
Per cent. H_2O = 15.40

IV.

Specific gravity = 1.416
Weight taken = 4.425
Loss = .525, 170°, 2 hours.
Per cent. H_2O = 11.93

V.

Specific gravity = 1.417
Weight taken = 8.639
Loss = 1.031, 170°, 3 hours.
Per cent. H_2O = 12.70

VI.

Solid Grape Sugar.

Specific gravity = 1.463
Weight taken = 7.215
Loss = .61, 170°, 3 hours.
Per cent. H_2O = 9.29

These data are scarcely sufficient to establish a rule for correction for variations in specific gravity; but it appears from them that the formulæ will not vary much from the following.

The rule, 53 divisions = 53 percentage, seems applicable to samples in which the percentage of H_2O is 12 to 14 and of which the specific gravity is from 1.409 to 1.414.

For each variation of .001 in the specific gravity, the percentage of H_2O varies about .3.

Thus, if we take the two extreme cases, viz., 6.37 and 15.40 percentage of H_2O, we find the corresponding specific gravities to be 1.440 and 1.409, a difference of .031.

The difference in the percentage of water is 9.03. The quotient of $.0903 \div .031 = 3$ nearly.

Let us apply these data to the correction of Nos. 18, 19 and 20 in table IV.

I give below these numbers and also their corrections.

TABLE V.

No.	Scale.	Reducing substance by calculation.	Same corrected.	Same by copper solution.
18	52.63	53.46	55.83	53.81
19	56.53	48.59	55.17	54.60
20	53.70	52.10	56.55	57.14

The above corrections were based on the supposition that 53 divisions of the scale correspond to 53 percentage reducing matter when the specific gravity = 1.409 and the percentage water 15.

We may therefore construct the following provisional formulæ, for estimating the correction to be applied to the reading of the scale when the specific gravity of the specimen varies much from 1.409.

Let a = reading of scale;
" a = corrected reading;
" s = sp. gr. of the sample.

Then $a = a - 3\,a\,(s - 1.409)$ when the specific gravity is greater than 1.409; and $a = a + 3\,a\,(1.409 - s)$ when s is less than 1.409.

I next propose to undertake some investigations to show the nature and number of the optically active principles present in glucose.

THE EFFECT OF HEATING WITH DILUTE ACIDS, AND TREATING WITH ANIMAL CHARCOAL ON THE ROTATORY POWER OF GLUCOSE; WITH NOTES ON THE ESTIMATION OF CANE SUGAR AND GLUCOSE IN MIXTURES.

By H. W. WILEY, of Lafayette, Ind.

SHAKING dilute solutions of glucose or grape sugar with animal charcoal produces a slimy precipitate.

I use the words *glucose* and *grape sugar* in their commercial sense. By *glucose* I mean the thick syrup made from corn starch, and by grape sugar, the solid product made from the same substance.

By *pure glucose* I mean the substances present capable of reducing the alkaline copper solution. I will not take time here to discuss the propriety of these names, nor the exact nature of the substances present.

I have made some experiments to determine the effect of animal charcoal on the rotatory power of the glucose of commerce. The following are some of the results obtained.

In each case 10 grammes of the substance were taken and made up to 100 cc. The observation tube was 200 mm. in length.

EXPERIMENTS.

I.

Reading before addition of coal	52°
After shaking with 20 grammes coal	47.20
Loss	4.80

II.

Reading before	52°.63
After addition 10 grammes coal	50 .28
Loss	2.35

III.

Reading before	52°.63
After addition 4 grammes ivory black	52 .38
Loss	.25

IV.

Reading before	52°.20
After addition 4 grammes ivory black	51 .13
Loss	1 .07

V.

Reading before	52°.20
After addition 10 grammes ivory black	48 .13
Loss	4 .07

From these figures it is seen that a glucose may lose nearly 10 per cent. of its rotating power when shaken with animal charcoal.

This is a matter of great importance when attempts are made to examine commercial syrups with the polariscope.

These syrups are usually highly colored, and require a great deal of bone black and lead acetate to make them fit for polariscopic examination. I have not yet tried the action of lead acetate on the rotatory power of glucose.

Heating with Dilute Acids.

The following readings as well as those in the preceding part of this paper are divisions on the cane sugar scale.

I.

A glucose gave direct reading	53°.7
Heated to 68° with 10 per cent. of its volume of strong sulphuric acid. Reading	52 .96
Loss	.74

II.

Heated same glucose for 50 minutes at 68°. Reading	50°.76
Loss	2 .94

III.

Direct reading another glucose	43°.36
Heated for 10 min. at 68° with 10 per cent. of its volume strong HCl. Reading	43°.02
Loss	.34

IV.

Heated same glucose 20 min. at 68°. Reading	42°.36
Loss	1 .00

V.

Heated same for 30 min. at 68°. Reading	40°.16
Loss	3 .20

VI.

Heated same for one hour at 68°. Reading	39°.26
Loss	4 .10
The per cent. of reducing substance in the above sample before heating with the acid was	62°.50
After	65 .58
Increase	3 .08

Thus a loss of 4.10 on the cane sugar scale corresponded to a gain of 3.08 per cent. in reducing power.

VII.

A grape sugar gave direct reading	40°.83
Heated with $\frac{1}{5}$ its volume strong HCl for 20 min. at 68°. Reading	31 .70
Loss	9.13

VIII.

A grape sugar gave direct reading	40°.00
After heating with $\frac{1}{5}$ vol. HCl. for 15 min. at 68°. Reading	32 .52
Loss	7 .48
The per cent. of reducing substance in the above was, before heating	69°.30
After heating	71°.50
Gain	2 .20

Thus a loss of 7.48 divisions in rotating power corresponded only to an increase of 2.20 per cent. in reducing power.

It will be seen, by inspection of the foregoing numbers, that glucose and grape sugar undergo quite a degree of change when subjected to the process of inversion as it is practised on cane sugar.

This change is sufficiently great to introduce an appreciable error into the process of estimating cane sugar and glucose in mixtures.

Conversion of the whole of the Optically Active Substances in Glucose into pure Mono-rotatory Glucose.

I next made an attempt to convert the whole of the optically active substances, present in glucose, into pure glucose, by prolonged heating with dilute sulphuric acid.

The conversions were made in a flask fitted with a glass tube about one metre in length.

By this device, vapors arising are condensed and flow back into the flask. Thus no loss of volume takes place.

The flask was at first heated in a water bath at 100°.

The conversion, however, took place so slowly by this method, that I afterwards added enough salt to the bath to raise the temperature to 104°.

The sulphuric acid employed was of 1.25 sp. gr., and was used in the proportion of 10 per cent. of the volume of the glucose solution. The readings were made in a tube longer by one-tenth than half of the length of the 200 mm., and the results multiplied by 2.

The following table contains the results of my work.

Nos. 1. 2. and 3 were heated at 100°. No. 1 for six hours, No. 2 for four hours, and No. 3 for three hours.

Nos. 4, 5, and 6 were heated for three hours at 102°.

Nos. 7, 8, 9, 10, 11, and 12 were heated for three hours at 104°.

TABLE I.

No.	Reading before heating.	Per cent. reducing matter before heating.	Reading after heating.	Per cent. reducing matter after heating.
1	52.65	53.20	25.92	81.30
2	48.40	59.35	27.14	80.00
3	48.45	58.55	26.10	80.00
4	43.50	62.50	24.73	79.00
5	50.26	55.60	24.18	82.00
6	51.50	53.50	25.38	81.14
7	52.65	52.36	23.89	83.33
8	56.53	54.00	27.36	90.10
9	51.74	52.33	26.19	82.00
10	47.70	61.40	25.36	82.04
11	49.80	58.80	26.02	84.00
12	40.83	69.93	26.07	84.80

In all cases the samples assumed a decidedly yellowish tint before the completion of the operation; interfering somewhat with the delicacy of the final readings.

No. 8 was a sample of glucose made for confectioners' use. If we exclude it, we get the following general results.

1st. The per cent. of reducing substance obtained is nearly 82.00.

2nd. The average reading of the cane sugar scale is nearly 25.5 divisions.

3d. If the pure glucose present is mono-rotatory, the specific rotatory power 0 would be $0 = \frac{8°.85 \times 100}{2 \times 8.2} = 54$ nearly. 8°.85 is the angular rotation (half shadow) corresponding to 25.5 divisions of the scale. But the specific rotatory power of pure glucose for the half shadow in the polariscope is *supposed to be* nearly 50. We have then here an excess of 0 equal to 4.

4th. This excess is due to the presence of optically active matter of a higher specific rotatory power than pure glucose, which has not been changed even by prolonged boiling with dilute acid.

I conclude, therefore, that it is quite difficult to convert the whole of the optically active matter into pure mono-rotatory glucose.

Examination of Mixtures.

In the examination of mixtures of cane sugar and grape sugar, it is necessary, first, to obtain the direct reading. Afterwards, the cane sugar is to be inverted, using at the most not more than ten minutes for this, and keeping the temperature at 68°. It is simply necessary to heat to 68° and then quickly cool.

After inversion, the temperature is carefully noted and another reading taken. Then, by Clerget's table, the per cent. of cane sugar is calculated. This, however, will only be approximately correct. We next calculate the whole amount of rotation produced by the approximate amount of grape sugar present. Having determined this, the reading after inversion is to be corrected, for the effect of inversion on the rotatory power of the grape sugar present. The average correction, when the heating has been carried just to 68°, will be about two per cent. of the whole rotation due to the grape sugar. If the process of inversion is carried on for an hour, as is the practice with some, this correction may amount to 10 per cent.

If the amount of glucose present is very small, this correction can be neglected. But if the amount is large, a failure to correct will introduce an appreciable error into the result. If animal charcoal has been used, a correction must be made also for it, depending, as has been shown, upon the amount employed, and also upon some other conditions which are not yet clearly made out.

It is easy to see into how grave an error we might fall, should we attempt to estimate directly the per cent. of reducing substance present in such mixtures from its rotatory power. I have shown in a previous paper, that the per cent. of such reducing substance is not *directly* but *inversely* as its rotatory power.

The error would be equally as grave, if the amount of the grape sugar present should be computed by using a factor supposed to represent the amount of such substance for each division of the scale. Such a method is based upon the assumption that

the rotatory power of grape sugar is constant and proportional to the reducing substance present. My paper, already mentioned, shows that this assumption is false.

Syrups.

When we come to the syrups of commerce, the case becomes still more difficult. These syrups are glucose adulterated with so called cane syrups, *i. e.*, with the drippings and refuse of the sugar refineries.

Among these syrups which are commonly used, I may mention the Revere and the Continental.

A Revere syrup, which I examined, had a specific gravity of 1.425, and contained 34.5 per cent. cane sugar.

A Continental syrup had a specific gravity of 1.415, and 35.7 per cent. cane sugar. It also contained 32.64 per cent. of inverted sugar.

It is easy to see how the admixture of such syrups with glucose tends to demoralize the results of the alkaline copper test, and mystify the readings of the polariscope.

No wonder that, heretofore, such egregious blunders have been committed in these examinations. It is only when examinations are made with a full knowledge of the optical peculiarities of the commercial glucose itself, and of the composition of the cane syrups employed, while at the same time attention is paid to the changes produced by inversion and clarification, that they can be relied upon as correct.

Observation Tubes.

In the polariscopic observation of inverted sugars, I use a copper tube, silver plated inside and out. I find the most convenient length to be 110 mm. This tube enables the readings to be made without the trouble of neutralizing the acid.

The silver plating of the tube is not necessary, but it makes the working with it a little cleaner. Pure copper can be used without any danger whatever of the acid acting on it.

I use a tube of thin copper and of small diameter. A few cc. will fill one of these tubes. Such a tube, when filled, quickly takes the temperature of the surrounding air. I have found by experiments, that when such a tube is filled with a liquid several degrees above or below the temperature of the air, in twenty minutes it will differ by less than half a degree from that temperature.

If an ordinary glass tube is employed, it is quite difficult to say how much time will be required to restore the equilibrium of temperature. My experiments show that it will require, at least, from two to three times as long.

I also provide this tube with a metallic jacket, very conveniently made of a piece of zinc tubing. This jacket is a little less in diameter than the metallic caps of the tube, and is of such a length that when the caps are screwed on, the jacket fits against a shoulder turned on them. The joint is made water-tight by using a rubber packing.

This jacket is furnished with three openings, fitted with short tubes. One in the centre carries a thermometer. One at each end serves as a supply or exit tube for any liquid, which we may wish to pass through the jacket.

By simply drilling a hole in the trough of the polariscope for the exit tube, the whole apparatus can be placed at once in position.

If now you wish to make a reading at a temperature, say of 88°, it is done in the following way:

In a large flask or tank, placed at one side and above the polariscope, and holding several litres, water is heated to a temperature of about 92°. By a rubber tube, this is connected on the principle of a siphon with the observation tube and its jacket. By means of a pinch cock attached to the exit tube, the hot water is allowed to flow through the jacket until the thermometer shows the temperature desired. It is hardly necessary to say that the observation tube must be filled at a temperature near that at which the reading is to be made to prevent expansion.

If we desire to make a reading at 0° corresponding to 44° to the left Clerget, we replace the water by alcohol, which is cooled by a salt and ice bath to —3° or —4°. The apparatus is then used as before.

We have thus a means of making readings at any desired temperature without having to mutilate the polariscope, as is done by fitting it with a water bath. I will say, however, that a judicious use of Clerget's principle renders readings at a given temperature unnecessary. They are, however, useful in certain complex mixtures by making certain calculations unnecessary.

I propose next to extend my investigations to candies and other confections in which glucose enters as an ingredient.

SALT SOLUTIONS AND THE ABSORPTION OF GASES. By E. L. NICHOLS, of Baltimore, Md.

THE experiments described in a paper presented by Mr. Wheeler and myself, on the Coëfficient of Expansion of Gas-solutions, give new evidence of an analogy, which I had for some time suspected to exist between the processes of salt-solution and the absorption of gases by liquids. The phenomena, by reference to which this analogy can be best shown, are too familiar to need description. It is, however, necessary to consider the explanation of them, afforded by the kinetic gas theory; it being by means of this theory that the relations in question can be most simply expressed.

When a liquid and a confined portion of gas have been for some time in contact, a constant condition is reached. In common parlance, the liquid is then saturated with the gas, and the vapor in the atmosphere above the liquid is at its maximum tension for that temperature and pressure. Now, in the terms of the kinetic theory of gases, the state of things is as follows: There are two fluid mixtures having one surface in common, across which surface a double interchange of molecules is continually taking place. The upper mixture consists of two kinds of molecules moving through long, free paths; the lower of two kinds of molecules (chemically identical with the first two varieties) moving through short free paths. The rate at which the molecules cross the common surface differs according to their kind, but the number of each kind crossing in one direction, in a unit of time, is precisely equal to the number of the same kind crossing in the opposite direction. Upon this last fact depends the maintenance of the constant condition in the two halves, and it is to this fact we refer when we speak of the saturation of the liquid, and of the maximum tension of the vapor.

Suppose, on the other hand, that a liquid and some solid which is soluble in it are in contact. After a time, as in the former case, a constant state is reached, which state we also express by the term saturation. We are not able to tell with the same certainty, what takes place between solid and liquid, because the kinetic gas theory has not been extended to cover these cases; but from the theory as at present understood, we may safely

assume that the liquid, after saturation, consists of an aggregation of independently moving molecules, making very short excursions. It is probably composed of two sets of molecules; molecules of the original liquid, and molecules of the dissolved salt. By molecules are meant, simply, such particles as in a gas or liquid move independently. I shall presently give reasons for assuming the solution to be a mixture of this kind rather than a collection of complex molecules, such as might be formed by any combination of the salt and liquid molecules.

To account for saturation, we must assume one of two things: that the rate at which particles leave the solid mass, during the process of solution and begin a course of independent excursions throughout the solution, grows slower from time to time, and that this process finally ceases entirely, or that a reverse process sets in which, after a time, counterbalances the first movement. If we adopt the hypothesis that there is a return of salt-molecules to the solid, the rate of which return grows more and more rapid as the solution becomes stronger, until finally the number of particles returning in a unit of time precisely equals the number torn loose by the action of the liquid, we obtain an explanation of the saturation of salt-solutions similar to the generally accepted explanation of the saturation of liquids by gases. *The process of solution can then be treated as a special case of the general process known as fluid diffusion.*

The other hypothesis leads to no such simple explanation. To account for the cessation of the process of solution, it becomes necessary to reject the supposition just made concerning the constitution of the solution, and to suppose some combination of the salt and liquid into complex molecules incapable of further action upon the solid salt. The point of saturation can be thus accounted for, but many secondary phenomena, which follow directly under the first hypothesis, remain unexplained.

There are other reasons for regarding salt-solution as a process of fluid diffusion. When a body passes from the liquid into the gaseous or solid states, it experiences under certain conditions what Prof. James Thomson has called a "*difficulty in beginning the change of state.*" It is supposed that the same difficulty would exist, when vapor is cooled to the condensing point, if it were feasible to fulfil the necessary condition. This condition is the entire absence of the substance in question in the state into which

the rest of the substance is ready to pass. Thus in the cooling of water below the freezing point, a bit of ice causes the immediate congelation of the entire mass; in the superheating of water, the presence of the smallest globule of saturated vapor below the surface of the liquid, brings about explosive boiling. Knowing this, it is easy to surmise the nature of this difficulty of beginning the change of state; the change of state is a process of *fluid diffusion* involving the presence of the substance in both states, and the reluctance to begin the change occurs whenever the substance is present in one state only. In the super-saturation of salt-solutions this same difficulty of beginning the change of state occurs; and the difficulty disappears when a particle of the substance in the solid state is introduced into the solution; just as in the other cases it vanished so soon as the substance was present in the two states. This indicates that the process is the same in the two cases; in a word, that, as we had concluded from other considerations, *solution* is a process of fluid diffusion.

The other phenomena connected with the change of state admit of equally simple explanations, if we adopt one more hypothesis concerning the nature of liquids. I feel the more confidence in making use of this hypothesis, since Professor Clausius has suggested and defended it in a recent paper in Poggendorff's Annalen. According to this view, the liquid molecule (meaning again by molecule that portion of the liquid which moves independently) is an aggregation of the gas-molecules of the substance; and its *mass* is a function of the temperature—decreasing as the liquid is heated. Andrews' critical point is then for each liquid, the temperature at which the liquid and vapor molecule are identical. At lower temperatures, the ability of liquid and vapor to exist, side by side, exhibiting such different properties, is explained by the difference in the size of their molecules. Under this hypothesis, the total energy necessary to the vaporization of a liquid is divided into two portions: that involved in raising the mean velocity of the molecules, and that consumed in breaking up the larger molecules into smaller ones. This latter portion is in common parlance, the *latent heat of vaporization;* it decreases with rise of temperature, and at the critical point becomes equal to zero.

Under this hypothesis, finally, we can account for the high rate of evaporation of gases in solution and the low rate of salts in so-

lution. During evaporation it is chiefly the molecules of highest velocity which escape — hence the lowering of the temperature of the liquid from the evaporation. If the liquid molecules of a gas solution are of much greater mass and of much smaller mean velocity than the gas molecules, the absorption of the latter will raise the mean velocity of the mixture, and cause the well-known rise of temperature due to absorption, but the small gas particles will retain their own high velocity instead of imparting them to the large liquid molecules.

Were gas molecules and liquid molecules of nearly equal mass, a distribution of velocities would occur, since by a single collision the entire velocity of a gas molecule could be imparted to some liquid molecule; but it is evident that since no gas molecule impinging upon a very much larger liquid molecule can sensibly change the velocity of the latter, and since in the long run the sum of the velocities imparted to such a liquid molecule by such collisions is equal to zero; there can no such distribution of velocities take place. The gas molecules in the mixture will therefore retain their high velocity and will evaporate more rapidly than the liquid molecules. In the same way, if the molecules of a salt in solution are vastly larger than the liquid molecules the liquid molecules will maintain their higher velocities and the liquid component of the solution will evaporate most rapidly.

This fact, that in salt and gas solutions the different components evaporate separately and independently and at very different rates, implies their existence in the solution in independently moving molecules. The hypothesis of a complex molecule offers no such simple explanation of the phenomena connected with solution; therefore is not tenable. From the other hypothesis the classification of the process of solution together with the other processes by which matter changes state, under the head of fluid diffusion, follows almost as a matter of course.

NOTES ON WATER ANALYSIS. By A. A. BRENEMAN, of Ithaca, N. Y.

1. *Significance of the chlorine test.*—Notwithstanding all that has been said upon the subject, it seems to me worthy of remark, that the determination of chlorine in well waters is of the greatest importance, under certain precautions, and in many cases should alone condemn the water. My own experience in more than two hundred determinations of chlorine, in the waters of the gravel district of Central New York, determinations made together with the ordinary analysis by Wanklyn's method, sustains this view.

2. *Change of Organic Matter by Aeration and Light.*—A sample of peaty water, deep brownish in color, and yielding 0.44 per million of albuminoid ammonia, gave after flowing one and a half miles, with slight fall, .25, and at the end of three miles .11, the color passing from deep brown to almost nothing, at the same time.

The aeration of suspended matter in streams should in general lead to a more liberal interpretation of its influence, than in the case of the same kind of matter in well waters, which are deprived of aeration. Too little importance is attached to influence of animal life and strong light as purifying agents, in running streams.

3. *On the Volume of Water to be used in Water Analysis.*—The use of 100 cc. for each distillation by Wanklyn's method is preferable, because of the greater convenience and quickness of the operation, and also because of increased delicacy in Nesslerizing. Increased delicacy results from the use of smaller tubes, which give a length of column greater in proportion to volume of distillate used in Nesslerizing.

4. *An Improved Nessler pipette-stirrer.*—The pipette used for the Nessler test, as here shown, is distinguished from the ordinary bulb-stirrer by the very short tip below the cord or bead, which latter fits closely to the tube in which the test is made. The wide opening prevents spirting of liquid from the tube as the stirrer is raised and lowered rapidly. The short tip permits agitation of the lowest portion of liquid in the tube.

5. *On the use of Common Yellow Glass as a Color Standard in Nesslerizing.*—The advantage of colored glass is its permanence and convenience of application. The glass in ordinary use is

suited only to the determination of high colors. Thin glass, of the same composition, might be made to satisfy all necessities of the case. The standard slip of glass is marked with a diamond, and the simultaneous use of a bundle of slips, of different values, permits the accumulation of any desired degree of intensity in color.

On Colors for Salt-glazed Pottery. By A. A. Breneman, of Ithaca, N. Y.

In continuation of some experiments, reported at the last meeting of this Association, I have succeeded in obtaining in the ordinary stone-ware kiln, all of the colors shown upon the experimental tile here shown with many other tints and shades, not heretofore produced, to my knowledge, on this kind of ware. I have little doubt that a complete palette of colors may be obtained for the decorator in this field. The number of experiments made is so great, and the results are so various, that I shall not attempt a description of them at this time, confining myself for the present to a statement of a theory of the action of the salt-glaze in its relation to mineral colors. This I believe, to depend upon a circulation by convection currents in the thin layer of sodium silicate resting upon the ware, after salt has been thrown into the kiln. As chlorine can have little affinity for the silicious matter of the glaze, while it is capable of volatilizing many of the metals which, in one form or another, are used as pigments, it would seem that the destruction of color, well known in the case of ordinary pigments, may be prevented by giving such consistency to the color-holding medium, that convection currents shall be impossible, at the temperature of the kiln; while at the same time this medium shall have mobility sufficient to permit of that migration of molecules, necessary to a uniform distribution of the color. Upon this theory my results have been obtained.

On the Deficiencies of Meteorological Work in data of value to Agriculture, and Means for supplying them.

By Wm. McMurtrie, of Washington, D. C.

What I mean by deficiencies of meteorological work in this connection is the want of data in the published records, concerning many of those conditions recognized by agriculturists and vegetable physiologists, to have an important influence upon the normal development of animal and vegetable life; and that the observations of the present time are not such as will supply all the data required in the study of meteorology, in its relations to agriculture, and especially to the condition of the crops. The want of such data, I felt most keenly in my work of last year, a portion of the results of which I had the honor to present to the association, at the Saratoga meeting. In that work, the object was to determine the limits to the conditions to which the crop in question might be subject, and reach such development as would give a profitable return to producer and consumer; and while the results obtained were interesting to me at least, it was difficult to make them so thoroughly marked as would have been possible, if more complete data, such as I will mention later on, had been obtainable.

The records published give detailed data concerning barometric pressures, temperatures observed in the shade, rainfall, relative humidity, direction and force of the wind and conditions of the sky, expressed in percentages of cloudiness, and these are valuable in their way, and for the end to which the observations were taken: but they by no means cover all that must be recognized in scientific and practical agriculture. Radiated as well as diffused heat, diurnal and monthly distribution of rainfall, evaporation of moisture, prevalence of dews, fogs and frosts, and tension of atmospheric electricity are also important and necessary. Light, that most patent factor in vegetable development, has been almost wholly neglected, except in the record of the condition of the sky, as expressed in percentage of cloudiness, which, in this connection, is as much a matter of accident as design.

Practical agriculturists have almost completely ignored the science of meteorology in the prosecution of their labors, repeatedly making attempts at the introduction of new crops into sections wholly unsuited to them on account of climate; and on the other hand, in the work of meteorologists, although agriculture

has been thought of and mentioned, the facts they have recorded have been collected more for the benefit of commerce than agriculture, and in the latter connection, they are, as we have said, sadly deficient.

Count de Gasparin seems to have been the first to call attention to the relations we have mentioned, and he has had at least two active followers, Quetelet in Belgium, and Marié-Davy in France. The sentiments of the latter in this particular are ably embodied in the prelude to an article in the Journal d'Agriculture Pratique of last year, in which he says, "Agriculture and its products have thus far received comparatively little profit from the labors of meteorologists, because the latter have been more preoccupied with questions of climatology in general, than with agricultural climatology, and the data they discuss, look to the first and not to the second." This difficulty belongs to all countries alike—Davy himself is making an excellent start in the right direction, at the Observatory of Montsouris, and he has already secured results of great scientific and practical value; but in order that his plan of work may be attended with the most fruitful results, he should have more extended coöperation, and the observations he makes and records should be made at a large number of stations, to render accurate generalization possible.—He observes and records all the conditions we have named. In this country, we have nothing comparable with it.

Our Signal Bureau, probably the most perfect of its kind, for the purposes for which it was designed, renders excellent service to agriculture and commerce, and we would not question its efficiency; but its work, as it claims, tends to the commercial rather than to the physiological side—to predictions of future conditions which may affect human affairs, rather than to the record and discussion of the effects of conditions which have existed, upon the development of crops. Nor is its organization sufficiently extensive to effect the end we have in view, the whole number of observers engaged under its direction, not being more than 800; while for practical deductions, for agricultural work, 3000 for the United States would not be excessive. But I believe the machinery and organization of the Department of Agriculture could be employed with good effect in this connection, and the desired observations for this country secured, through the 2300 observers and reporters it already employs to make monthly returns concerning

the condition of the crops, and who receive, as compensation for their services, the publications of the Department. If the necessary instruments were supplied them, it would doubtless be found that very few would decline to perform the tasks requested at their hands—making the observations and reporting weekly or monthly records to the Department, where their results could be tabulated and discussed in connection with their simultaneous returns of the condition of the crops.

I have, therefore, taken the liberty to suggest to Gen. LeDuc, the Commissioner of Agriculture, the following plan of work in Agricultural Meteorology for his Department, and I am pleased to say that he fully recognizes its necessity, and is in full sympathy with it, though he would, of course, require congressional support to enable him to carry it out.

The plan is this:—

1. The establishment of a system of observation and record among the reporters to the Department, and others whose coöperation may be secured throughout the United States and Territories, with instruction to observers to keep careful records of the conditions of atmospheric pressure, temperature in its various relations, relative humidity, evaporation of moisture, winds, light, tension of atmospheric electricity, occurrence of dews, fogs, and frosts, and report them at stated intervals of time to the Department for consideration and permanent record.

2. The collection of meteorological records from every part of the world, from which to construct detailed tables, showing the relations of all the conditions named above, and that may influence the growth or health of vegetation.

3. The construction of maps showing the geographical distribution of various crops to be used in connection with the meteorological or climatic data to be collected.

To secure the data from abroad for this work, correspondence should be established with ministries of agriculture of foreign governments, learned societies, institutions of learning and research, and students in meteorology and allied sciences, requesting information concerning the published and manuscript records of each country and sources from which copies of them can be obtained. They should then be secured to facilitate the end in view. Information should also be requested concerning the cultures to which each country is devoted, those which succeed

well and are profitable, and those which have been experimented upon with unprofitable financial results.

In the tables and maps that could be constructed from the information thus gained, there would be found an almost perfect guide as to experiments that may be made in new cultures with prospect of success. Such work would in no way interfere with that of the Signal Bureau, the value of which cannot be overrated — and on account of the very nature of the work, it should be carried on by the Department of Agriculture. It would become an additional source of knowledge concerning that science in which all classes of the people are equally interested, and if the knowledge thus gained be applied to the management of farm crops, it will be difficult to estimate the saving that must result in the expenditure of time and means, and of enterprising and intelligent labor of agriculturists often employed in unfruitful experiments that could be directed to other channels of industry, or other and profitable cultures.

THE ACTION OF SUNLIGHT ON GLASS. By THOMAS GAFFIELD, of Boston, Mass.

WHEN God made the light, He created one of the most ethereal and yet most powerful of the elements and forces of nature.

It will be my pleasure to show how the sun, its great fountain and source, while it paints so beautifully and wonderfully the birds, the insects and the flowers, can in some measure also delicately tint the wares upon our tables, the glass in our houses, and the windows of the storied cathedrals of the world.

My subject is the action of sunlight in changing the color of glass, and I shall give a brief account of my humble experiments, commenced in 1863, and continued to the present day.

There is no mention of the subject in ancient records, for there was little need or use of glass windows in the olden time, and probably little or no glass of composition or color liable to any very perceptible change of tint. Not until after the beginning of the present century was the phenomenon observed in Europe, in the change of some light colored plate glass to a purple and of

another kind to a yellowish hue. In 1823, and the following year, Faraday, Bontemps and Fresnel made a few brief experiments showing that exposure to sunlight effected this change of color.

Other experiments were made by Melloni and Hunt, showing the action of glasses of different colors as media in the transmission of light and heat, but none to my knowledge are recorded up to 1863, with the above exceptions, showing the effect produced on the glass itself. The observations upon the subject by Pelouze (who for many years was the chemist of the French Plate Glass works at St. Gobain) are published in the Comptes Rendus for January 14, 1867.

Without making any pretension to the learning of the schools, or accurate scientific knowledge, I believe that my experiments, suggested by my experience as a glass-dealer and manufacturer, are original in their method and extent, as they cover the whole field of glass-making, including both colored and colorless glass. Those in connection with colored glass are entirely new.

By colorless glass is intended glass like that which we see in our windows, which shows little or no color in looking through its surface; but, observed through the edges, exhibits a great variety of tints, running from the almost colorless white to those of yellow, blue and green. The really colored glasses — made so intentionally by the addition of some metallic oxide, or other coloring constituent — can only be observed through the surface, as their opacity, except in the lightest colored specimens, will not allow an observation through the edges for more than a fraction of an inch. My experiments have been carried on chiefly upon the roof and upper window-sills of my house in Boston, in a position exposed to the full force of the sun's rays during the greater portion of every day, being protected by covers only in the event of snowstorms. Of course, a perfect arrangement could only be made when a flat roof or platform in an open field could be provided, and the sunlight could act with full force during every hour and minute of the day.

But mine was sufficiently near this point to show very interesting results. The change in color in some specimens is developed slowly, while in others it commences after a few hours of exposure in a summer day. In some sensitive kinds, I have witnessed a perceptible change in a single hour of sunlight exposure upon the top of a post in a country garden, at noontime, on a clear and hot day of August.

The glass plates exposed vary from four by two inches (my usual size) to four by eighteen, and from one-sixteenth of an inch to an inch in thickness. I have in my cabinet more than a thousand specimens showing the effect of exposure from a single hour to thirteen years.

I have thus exposed and tested some eighty kinds of colorless glass of American, English, French, German, and Belgian manufacture; embracing rough and polished plate; crown and sheet window glass; flint and crown optical glass; glass ware and glass in the rough metal. I have also exposed ground and opal glass, and some seventy kinds of colored sheet and rolled cathedral glass, including not only the main spectral colors,—red, orange, yellow, green, blue and violet,—but a variety of intermediate tints, such as brown, olive, amethyst, flesh color, etc.

I cannot in these brief moments give you all the privately recorded details of the interesting results of these experiments; but referring you to Silliman's Journal for 1867, for an account of the same up to that date, I shall rather hasten to allow my illustrations to tell their own story, which they will do without any color of exaggeration.

Various are the tints of the original colorless specimens, and equally various are those which are produced by sunlight exposure.

The nomenclature of different observers might also vary. I cannot recapitulate the long list from my journal, but would name as a general classification of the results produced, the following changes of color:

1. From white to yellowish.
2. From greenish to yellowish-green.
3. From brownish-yellow and greenish tints to various tints of purple.
4. From greenish-white to bluish.
5. From bluish and other tints to darker tints of the same colors.

Every specimen of colorless glass exposed for ten years has changed in color or tint, except some white flint glass, such as is used for fine glass-ware and optical glass. The optical glasses exposed—with the exception of two specimens of crown glass, containing no oxide of lead, which became of a yellowish hue—changed only in a very slight degree in tint, even after more than ten years of exposure, and not enough to be witnessed by ordinary

observers, or seriously to interfere with their practical value and use.

In some instances, where lenses seem to be affected, the change may be traced to a discoloration of the Canada balsam, by which the crown and flint glasses of the lens are cemented together. Bontemps thinks that this almost unique character of fine flint glass shows that the presence of oxide of lead, which enters largely into its composition, exercises a protecting influence against the coloring action of the sun's rays. I have proved by actual experiment that un-annealed as well as annealed glass can be changed in color by sunlight exposure.

Diffused light will also color glass, but with a greatly diminished effect, corresponding to its diminished power, as compared with the direct rays of the sun. Different degrees of coloration are sometimes noticed in the panes of the same window, and sometimes in the same pane, corresponding to their different degrees of sensitiveness to sunlight coloration, or differences of exposure to the direct rays of the sun.

That there comes a time when this coloring action of sunlight ceases, I have demonstrated by finding no further change by a second long exposure of glass removed from a building in which it was exposed for thirty-four years. The period of its duration varies with different glasses, the yellow coloration being chiefly developed in a year, increasing slowly afterwards, and almost entirely ceasing in some instances in ten years, while the purple tinted specimens develop less rapidly, and go on increasing in depth of color beyond this period. The continuance of my experiments for another decade or score of years, if my life should be preserved, will enable me to speak more definitely upon this point.

It is a curious fact that while these various glasses, with a few exceptions among the colored specimens, can be submitted before sunlight exposure to great heat in a glass-stainer's kiln without suffering any change, they can, after sunlight exposure, be restored to their original color by being placed in the kiln during a single fire. A second exposure to sunlight will reproduce the same yellow, purple and other colors as before, and this process of coloration by light and decoloration by heat can be carried on indefinitely. I am indebted for this observation to Pelouze, who names it in his essay in 1867, and I have confirmed its accuracy by many experiments since that date. Professor Percy also made

the same observation to the editor of the London Photographic News at about the same date, having ascertained the interesting fact in the laboratory which he occupied at the Museum of Practical Geology in London.

The most tangible illustration of change in colorless glass, as observed in similar windows in London, by Faraday in 1823, can be witnessed in the purple-tinted panes of some of the houses in Beacon street, in Boston, which were built fifty or sixty years ago. Some of the present owners and occupants formerly supposed that they were imported of the same color which we now observe, as an intentional decoration of their pleasant homes, and they were quite skeptical when I first published the statement that they were painted by the magic pencil of the sun. They have generally been convinced by the testimony of experiments which speak for themselves.

In speaking of experiments with colored glass, I should say that some kinds—called pot-metal, because colored in the pot—are colored throughout the body of the glass, while in the flashed, stained and enamelled specimens, the colors are upon the surface only.

In experiments continued for ten years with the main spectral colors, no change was found in any pot-metal specimens, except a slight darkening of the violet.

A change was observed in the colorless body of some of the flashed and stained pieces, a yellowish or purplish color being produced when the colorless side was uppermost, and also when the colored surface first met the sun's rays, and transmitted them in differing degrees to the colorless body below. The sunlight coloration of these flashed specimens is not sufficient to be noticed in an observation through the surface only.

In 1870, I commenced experiments with pot-metals, not of the primary colors, but of the intermediate ones which most nearly approach those produced in colorless glass by sunlight exposure.

In many specimens of the brownish, amber, purple and flesh colors thus exposed, I was surprised to observe the change in color or tint in a short time, a few days in summer sometimes sufficing to show the commencement of the sun's action. I have continued these experiments with colored glasses to the present day, and find the general results as follows:

The main colors, except violet, which is rendered a little darker

by exposure, are not at all affected, but the following changes are notable:

1. From brownish tints to a flesh color.
2. From flesh color to tints of purple or violet.
3. From amber, olive, and purple to darker tints of the same colors.

The result of these experiments led me to believe that they might furnish a key to solve interesting questions concerning the alleged superiority of the old cathedral windows, and I made a trip to Europe in 1872, to visit these interesting edifices, and, if possible, obtain some of the old glass. By removing the protecting putty from the edges of purple-tinted panes in Beacon street, one can observe the original color of the glass. I hoped to do the same with fragments from the old cathedrals. But the protecting edges of the lead divisions were so narrow, being generally not over one-eighth or one-sixteenth of an inch, that they afforded no chance for a similar observation. Obtaining specimens from Strasburg and elsewhere, I applied also the interesting heat test by dividing some of them in two pieces, and exposing one to the heat of a glass-stainer's kiln. But while some ordinary and expected effects were produced, such as the deepening of some red and yellow tints by a second firing, no change was produced in the pot-metal colors by heat which indicated any previous change by sunlight, except in the few small purple and flesh colored specimens. The colorless body of the flashed and enamelled glasses, with one exception of a slight lightening of the tint, was unchanged, being generally of so dark a color originally, that it seems no change could be effected by light or heat. So far as these purple and flesh colors, and other easily changing tints of pot-metal colors, were used in the old windows, and as the same cause must always produce the same effect, we can certainly say that they must have been changed in color or tint by their centuries of exposure, and that we cannot see to-day the glass as it was when it came from the artists' studios and the glass factories of the mediæval ages. And so far we may transfer some of our praise from the old artists in glass to the silent but wonderful pencil of the sun. But we must not make too much of this point, as an examination of many cathedrals in England, and on the continent, convinced me that the proportion of these peculiarly sensitive tints of flesh and light purple colors in most of their windows was small, being confined

chiefly to the faces and limbs of the Madonnas and saints, and many of these being produced in enamelled or flashed glass rather than pot-metal.

We may, perhaps, properly digress for a moment to say that the true cause of the superiority of these old cathedral windows, speaking paradoxically and yet truly, is found in the inferiority of the glass, its richness in the poverty of its constituents, its very perfection in its uneven thickness and the imperfections of its surface and its body, all covered as they are by the accumulating dust of ages and honeycombed by the corroding tooth of time. Like the facets of a diamond or ruby, each little wave and thread and blister becomes, by interference, refraction and reflection of the light which plays upon it, a new source of the gemlike brilliance, harmony, and beauty which distinguish the painted glass of former centuries.

The glassmakers of Berkshire and England aim to reproduce in some measure the perfection of this old glass, by reproducing its imperfections in the antique and rolled cathedral glass which are so extensively used by the artists and architects of to-day.

And now, what the wonderful alchemy of the sunlight is, and what the methods of its operations, are questions on which various opinions have been given by glassmakers and scientific men, but which only a careful consideration and comparison of the observations and theories of many practical and scientific observers can accurately decide. Some have attributed one or another of these colorations to the presence of oxide of iron, some to arsenic, and some to carbon or sulphates in the constituent materials of the glass.

The greater number think oxide of manganese, singular as it may seem, used as a decolorizer, to be the great colorist in all of these changes. In many colored and colorless glasses, and especially in those which assume after exposure any tint of purple or flesh color, it undoubtedly plays a very important part. But in some experiments with glass said to contain no manganese, decided changes of color from greenish to yellow have been produced. Perhaps the question cannot be accurately solved, until some glass manufacturer or lover of science and scientific truth shall make, with great care and for this special purpose, a series of colored and colorless glasses, which shall be exposed for months and years to the influence of sunlight. Knowing the exact con-

stituents of each specimen, a good foundation would be laid for a thorough scientific investigation of the subject. This has never yet been done, and in the absence of such knowledge, we can only theorize upon the results which we witness.

While I may not theorize, I may help others to do so by stating the interesting part which some metallic oxides play in coloring, and the oxide of manganese in decolorizing glass. In almost all kinds of window glass and glass-ware, materials are necessarily used which are not perfectly and chemically pure.

The sand, the carbonate or sulphate of soda, the lime and other constituents, one or all, contain slight impurities, and almost always oxide of iron.

The protoxide of this metal gives glass a bluish or bluish-green tint, the peroxide a yellowish, and a mixture of the two, in which condition it is generally found in glass, produces the almost universal greenish tints which we witness in the glass of commerce. To correct in some measure the coloration by iron, a small proportion of oxide of manganese, called "glassmakers' soap" is put into the "batch," or glass mixture. The natural tint produced by manganese in a high state of oxidation is purple or violet, and we cannot produce these colors without its use. In proportion as it is deprived of oxygen, it loses its coloring power, and when it reaches the state of protoxide, it becomes nearly colorless.

Now in the reaction which takes place in the melting-pot, the manganese gives up a portion of its oxygen and its coloring power to the iron, which is converted into peroxide, which gives a yellowish color to the glass.

This yellow color is complementary to whatever of purple coloring power is left in the manganese, and is therefore neutralized, and the glass comes out of a light color. When the sunlight strikes this glass, the nice balance between the oxygen of the iron and the manganese is disturbed, and there ensues, even in so solid a body as glass, an interesting contest for the possession of the coveted oxygen, and the conquering element is known and announces its victory by a display of its special colors. Every change of color or tint involves the wonderful condition, invisible to human eyes, of a molecular or inter-molecular movement of the atoms of this solid body, and it is probably attended by two circumstances: an interchange or redistribution of the oxygen among the constituents of the glass, and the development, in consequence of this

redistribution, of the special coloring power of the metallic oxide which has the greatest affinity for oxygen, or stands in the greatest proportion to the other constituents.

And so in some specimens the yellow of the iron predominates, in some the purple of the manganese, and in others, the yellow at first appears, and afterwards a yellowish purple and purple fully developed.

While these suggestions may help to explain the sunlight coloration of the so-called colorless glasses, they may apply also to colored glass, inasmuch as all of the sensitive specimens contain a certain proportion of iron, or manganese, or both.

If this coloring power of the sunbeams has not been generally known, it is because it cannot readily be seen by ordinary observers, except in such as the purple-tinted windows of Beacon street, and even then it may sometimes require the background of a light colored curtain.

The discovery of the defect is an annoyance to both consumers and glassmakers. When our Beacon street friends could no longer obtain similar glass to replace broken panes, the insertion of colorless ones gave their windows the interesting checker-board appearance which we witness to-day. When the glassmakers found the defect a matter of scientific observation and experiment, and their results published at home and abroad, they remedied the difficulty in a measure by using purer materials, or reducing the proportion of manganese in their "batch," or giving up its use entirely, preferring to have the glass assume its natural and more permanent color, even if it be a little greenish or bluish, rather than by "doctoring" the mixture,—to adopt the glass-makers' term for the use of manganese,— to have it light colored to-day and easily affected by the sunlight of to-morrow.

This improvement is of especial importance to photographers, who, in all operations requiring short exposures and all the light possible to obtain, would avoid the use of any glass in their skylights which, after a few months or years of exposure, will be robbed of a great proportion of its power to transmit the chemical influence of sunlight by a change to a yellow or purple tint, which in time might cut off almost as much actinic effect as if it had been ground or enamelled on one of its surfaces.

I have made some photographic experiments to show this deteriorating effect, by exposing sensitive paper under glasses of the

original colors, and those of the same kind, changed by sunlight exposure, and witnessing the perceptibly different shades of darkening produced. This action of sunlight must not be confounded with that called "rust," or "stain," which is occasioned in some glasses having an excess of alkali in their composition, by exposure to the atmosphere, and manifests itself in two ways; first, by a disintegration and roughening of the surface, sometimes producing all the effects of ground glass; and secondly, by an efflorescence and apparent formation of an infinitesimal coating of oxide upon the surface, on which the play of the sun's rays produces all the colors of the rainbow, as with the action of light on the infinitesimal grooves of mother-of-pearl. This is simply surface action, whereas the action of sunlight permeates the whole body of the glass wherever the rays directly strike it.

I might refer to many other points in connection with my subject, but my time will not permit and I hasten to exhibit the results of my experiments, asking my hearers, who are so much better versed in scientific studies than myself, to give me their light in ascertaining the causes and exact operations of this interesting power of the sun's rays to paint the products of art, as they do so beautifully and wonderfully the works of nature on the mountain, in the forest and field.

The large number of specimens of colored and colorless glasses here exhibited show the coloring effect of exposure to sunlight.

A tangible illustration of this subject is found in the purple-tinted windows of some of the Beacon-street houses, of which the specimens exhibited show the original color and the changes produced by sunlight exposures varying from one day to fifty years. These examples also show how this action can be turned to interesting account in impressing upon colored and colorless glasses the forms of leaves and ferns, and in printing inscriptions and mottoes. It is a species of photographic work with sensitive glass, instead of sensitive paper, the sun showing itself a most excellent printer and developer, and, indeed, the only true photographer in colors.

The discovery of Oxide of Antimony, in extensive lodes, in Sonora, Mexico. By E. T. Cox, of Oakland, Cal.

Up to the present time, the antimony of commerce has been mostly obtained by the reduction of the sulphuret; and though this ore is widely distributed over the globe, it is, as a rule, associated with a variety of mineral substances that obstruct reduction and add to the cost of purifying the metal. These sulphurets are also found in such sparse quantities, that the metal usually commands from three to four times the price of lead and fully as much as that of tin or copper. At present, the principal supply of sulphuret of antimony, for the English smelters, is obtained from Algeria, Spain and Ceylon. Small quantities of oxidized antimony ores have been found in portions of Europe, and in Ceylon, but at no time in such quantities as to elicit special attention from the smelters. When, therefore, about a year ago, I called the attention of English metallurgists and smelters to the occurrence of vast lodes of almost pure oxide of antimony, in the district of Alta Sonora, Mexico, thirty miles from the Gulf of California, it seemed too marvellous for their belief. A company of gentlemen of Boston, Mass., now have control of these antimony mines, and the ore will soon be in the hands of the smelters.

The geological features of the country, where this ore abounds, is similar to that of southern Arizona. The mountains are in short, narrow ranges, having, for the most part, a northerly and southerly trend. Their crests are either rugged or well-rounded cones, according to the nature of the rocks forming their mass. Between these ranges, we have what is called mesa or table-land; the latter is formed of the débris of the mountains. This material is of so loose and porous a nature, that the small amount of rain which falls sinks through it and leaves the land dry and arid. As far as I have been able to make out the order of the rocks forming these mountain chains, we have first granite, and this is flanked by sub-carboniferous limestone, in most places so crystalline as to obliterate all traces of fossils. Protruding through these and forming the mountain peaks, we have porphyry, quartzites, basalts, diorites and trachites.

The country in the immediate vicinity of the antimony mines is quartzite and limestone. The ore lodes are from four to twenty feet wide, and exploration work, carried to a depth of thirty feet,

shows that the fissures are filled, from wall to wall, with the oxide of antimony, almost pure and remarkably uniform in character. The course of the lodes is nearly north and south; the pitch is 90° to the east. The area of country, over which the ore is found, may be roughly stated to be five or six miles long and half a mile or more wide.

The Boston company controls nine mines, each of which is a full Mexican claim, 800 metres (2624′ 8″) long and 200 metres (656′ 2″) wide. On three of the mines, the crop, which is solid oxide of antimony, like the samples exhibited upon the table, stands up boldly above the general surface and may be traced along the claims for many hundred feet. As stated above, the ore, so far as explorations have exposed it, is almost pure oxide of antimony, the little impurity it contains being silica. The fire assay shows it to contain from 60 to 70 per cent. of pure metal, and I have estimated the entire lode to average 50 per cent. By selection the average may be augmented.

On going down to a greater depth on the lode, it is possible that the oxide may give place to sulphides, but thus far there is not the slightest evidence of any change. This discovery is destined to produce a marked influence upon the production of metallic antimony and greatly to extend its uses.

Note.—Prof. S. P. Sharples, of Boston, said that he had examined many specimens of the oxide of antimony received from Prof. Cox. These varied in color from almost white to a very dark brown. The specific gravity of one of the purest specimens is 5.07 and it contained 5 per cent. of water and 75 per cent. of antimony. This composition and specific gravity approach very closely that of *stibiconite*.

The mineral is only very slightly soluble in hydrochloric or nitric acids or aqua regia. Fusion with bi-sulphate of soda only partially resolves it. It is, however, readily and easily decomposed by fusion in a platinum crucible with carbonate of soda.

This oxide of antimony has, hitherto, been found only as slight coatings in other antimony minerals and it has been very difficult to get specimens of it, even a few grains in weight.

The mineral is not easily reduced before the blow-pipe, but is very easily reduced in a crucible with powdered charcoal or cyanide of potassium; giving, at a single operation, buttons of star antimony.

Remarks on tables for the reduction to zero of the measured volumes of gases. By Edward W. Morley, of Hudson, Ohio.

[ABSTRACT.]

The tables of Bunsen, Sutton, and others, are too bulky, from not adopting the form in which all logarithmic tables are now printed.

Many are also clumsy, giving seven decimal places of the logarithm. Four places give as much accuracy as the manipulation in any but the most refined work; five places more than equal the accuracy of the best work in gas analysis.

All tables for the purpose give a logarithm to be subtracted from the sum of the logarithms of the volume and tension observed. The computer therefore has mentally to take the arithmetical complement of the tabular number, in order then to add the three logarithms in one operation. But such tables, being intended for this one purpose only, ought to give this complement by inspection.

All tables at present published use the coëfficient of the absolute expansion of air. For refined work, a second and even a third correction is then necessary, with a second and third tabular number. But by using the coëfficient of the apparent expansion of air, under the conditions of measurement in analysis, the whole correction can be computed in a single operation, and with but a single tabular number. The same table may be used for ordinary work, for its use involves no more labor than that of any other table.

For the most refined work, there have to be taken into account the cubical expansion of the eudiometer, the linear expansion of the scale on which pressure is measured, and the inequality of the degrees of the mercury-in-glass thermometer. We should therefore use, for the computation of tables for the purpose mentioned, the coëfficient of expansion of air added to that of mercury and diminished by the sum of the cubical expansion of the eudiometer and of the linear expansion of the scale. This gives the coëfficient for a degree of the ideal air thermometer, and must now be multiplied by the factor which will reduce the degrees of the air thermometer to those of the mercury thermometer.

Since the nature of the glass of which a given mercury ther-

mometer is made affects this factor, it is best to determine the joint effect of this factor and of the correction for errors in calibration of the given thermometer by a simple process which gives very satisfactory results. A convenient volume of air is measured at a temperature near freezing point. The same volume is also measured at about ten, twenty, and thirty degrees. The corresponding temperatures are carefully observed, but the apparent volumes are computed without reduction to zero. The table for temperature corrections is now computed with that coëfficient which will reduce the apparent volumes at the three higher temperatures to the volume at the lower temperature. This coëfficient will be different in the different intervals, partly owing to errors of calibration, and partly to the inequalities of a perfect mercurial thermometer. With such a table computed for a given thermometer, reductions may be made, even when another thermometer is used, with more accuracy than when the reductions are made from a table which does not regard the difference between the degrees of the mercury and of the air thermometer, and with far more accuracy than when the reductions are made from a table which regards only the absolute expansion of air.

THE MOST CONVENIENT SCALE FOR A THERMOMETER USED IN GAS ANALYSIS. By EDWARD W. MORLEY, of Hudson, Ohio.

[ABSTRACT.]

IT is impossible to dispense with a thermometer in gas analysis, if accuracy and rapidity of work are both required. No method of keeping the temperature constant, nor of mechanically compensating for its variations, answers this double requirement. My analyses of air are made with a probable error of but the four-hundredth part of one per centum. In them the total reduction of the whole analysis, that is the computing the percentage of oxygen from the readings of scale and thermometer, takes less than two minutes and a half. No system other than that which

determines the temperature, and corrects for its variations, can equal this in rapidity.

If the thermometer is to be used in gas analysis, it is worth while to provide it with that form of scale which is most economical of time. Usually it is necessary to read the centigrade degree on its scale, and then consult a table for a logarithmic factor corresponding; the thermometer ought to be so graduated as to show this logarithm directly, dispensing with the table.

A seeming difficulty lies in the fact that the degrees are of varying length, and hence not so easy to produce on a graduating engine. But since all that is required is that the graduations shall show logarithms accurate in the fifth decimal place, the difficulty can easily be surmounted. We may compute the places on the centigrade scale at which every third logarithmic degree would end, and then divide these intervals of three logarithmic degrees into thirty tenths at one setting of the engine. No error would appear in the fifth decimal place in this way. As the logarithmic degrees mentioned are two-thirds as long as centigrade degrees, the number of settings of the engine, to reach say thirty-five degrees, would not be too troublesome.

The most convenient method of numbering these logarithmic degrees may be described. The freezing point should be considered as corresponding to the logarithm of unity; at about six-tenths of a centigrade degree above this point, will be a logarithm differing from the former by one-thousandth; the space between this and the former is to be divided into ten equal parts which represent units of the fourth place of decimals, and read by estimation to units of the fifth place of decimals. This logarithm differing by one-thousandth from the logarithm of unity should be numbered 99, representing the logarithm 0.99900. At about 1.2 centigrade will stand logarithmic degree 98, representing logarithm 0.99800. The scale is continued upwards in this way, the numbers decreasing upwards. The number taken from this scale, followed by the tenths and hundredths, and preceded by the figure nine, supplied mentally, is the logarithm to be added to the logarithms of tension and observed volume to reduce apparent volumes at other temperatures to the freezing point.

REMARKS ON JOLLY'S APPARATUS FOR DETERMINING THE AMOUNT OF OXYGEN IN AIR. By EDWARD W. MORLEY, of Hudson, Ohio.

[ABSTRACT.]

THIS apparatus resembles Jolly's air-thermometer. The air to be analyzed is contained in the bulb of the apparatus, and is measured by determining its tension at the freezing point. Its oxygen is absorbed by a spiral of copper wire heated by an electric current, and the residual nitrogen is measured at the freezing point. The process is simple, and the apparatus is simple and economical; but it can claim no greater accuracy than has been obtained by a great many experimenters by means of the more rapid process with eudiometric apparatus.

Let us compare its accuracy with the accuracy of the Bunsen long eudiometer, and that of the Frankland and Ward apparatus. We compute the probable error of Jolly's apparatus on the hypothesis that the probable error at the so-called Jolly-point is the hundredth of a millimetre, and that at the other three measurements is the tenth of a millimetre. This probable error of the result may be compared with the probable error computed for the two other apparatus on the hypothesis that the probable error in each reading is the same quantity as that in the corresponding kind of readings in the Jolly apparatus, namely, the tenth of a millimetre.

The error at the Jolly-point is partly one of volume observed, and partly of tension observed. The two have always the same sign, and form but one independent error. The error at the top of the manometric column, and the errors at each end of the barometric column form the other three independent errors to which the measurement is liable. Their total effect makes the probable error of the measurement 0.023 per centum.

The volume of air taken for analysis enters the result as a denominator and as a minuend in the numerator. These two effects have contrary signs, and the residual effect is four-fifths of the probable error of the measurement. The probable error of the other measurement of residual nitrogen enters the result with its full value. Their joint effect makes the probable error of the result 0.029 per centum.

Now in the eudiometric process, the error in the measurement

of air enters the result only as a divisor, with one-fifth its value. The measurements before and after explosion affect the numerator with one-third their value. All the errors of measurement are diminished threefold and fivefold in their effect on the result. The errors probable in the result with the Bunsen long eudiometer and with the Frankland and Ward apparatus are 0.027 and 0.019 per centum, for a probable error of a tenth of a millimetre in each scale reading. Therefore, while the Jolly apparatus can make more accurate single measurements than the other methods, it is not capable of obtaining so accurate final results.

If the apparatus be modified, as the air thermometer has been modified, by closing the end of the manometer tube, and dispensing with the barometer, the apparatus will give as good results as the Frankland and Ward apparatus. The probable error of a measurement becomes 0.014 per centum, and the probable error of the result 0.019 per centum, both on the hypothesis of a probable error of a tenth of a millimetre in each scale reading and of a hundredth at the Jolly-point.

The following table shows the probable comparative errors of the four apparatus mentioned.

PROBABLE ERRORS OF MEASUREMENTS AND OF FINAL RESULTS FOR PROBABLE ERROR OF ONE-TENTH MM. : AND OF ONE-HUNDREDTH MM. FOR JOLLY-POINT.

	Bunsen long Eudiometer.	Frankland & Ward apparatus.	Jolly apparatus.	Jolly apparatus modified.
Probable error 1st measurement.	.045 per ct.	.034 per ct.	.023 per ct.	.014 per ct.
Probable error 2nd measurement.	.059 " "	.042 " "	.023 " "	.014 " "
Probable error 3rd measurement.	.048 " "	.033 " "		
Probable error of result.	.027 " "	.019 " "	.029 " "	.019 " "

SOME POINTS IN THE CONSTRUCTION OF AN APPARATUS FOR THE ACCURATE ANALYSIS OF GASES. By EDWARD W. MORLEY, of Hudson, Ohio.

[ABSTRACT.]

A. WHEN a certain method of determining the top of the pressure column is used, the conditions of most accurate measurement of varying volumes of gas are satisfied by making the column of mercury, which measures the pressure, equal in length to the part of the eudiometer which is filled with the gas. In the usual form of the apparatus, the pressure column has to be longer, which immensely increases the difficulty of securing uniformity of temperature.

B. To define the top of the pressure column, I use a Jolly-point enclosed in the barometric vacuum; this makes the probable error at the top of this column evanescent. The whole error of the tension is therefore produced by the same uncertainty which produces the error in the observed volume. If now the Jolly-point is put at about the level of the top of the eudiometer tube, the probable error of the tension is the same part of the tension as the probable error of the observed volume is of the observed volume. This is the condition of the maximum accuracy.

C. To preserve the barometric vacuum over the pressure column, I shut off this column from communication with the rest of the apparatus except at the instant of a measurement. The screw of the fine adjustment formerly described serves also this purpose. The vacuum is found to be even more permanent than that of a barometer.

D. An auxiliary pressure tube is employed for preliminary adjustment. The bore of this tube is but a millimetre, which secures two important advantages.

E. The eudiometer tube is graduated with fine lines not over the eight-thousandth of an inch wide, in half millimetres. The graduation is affected with no relative errors of the hundredth of a millimetre. A reading microscope is carried on a cylinder so solidly connected with the iron tripod of the apparatus that ten pounds produce a relative flexure of only a tenth of a millimetre. This microscope is brought nearly to the level of the mercury in the eudiometer when the column in the pressure tube reaches just to the Jolly-point. The microscope is made to give distinct vision

of the graduation, and by Grunow's cathetometer fine adjustment, the terminal lines of the eye-piece micrometer are made to coincide with two millimetres of the graduation. The focussing movement now moves the microscope so that distinct vision is had of the meniscus, and the level of the mercury is read to the hundredth of a millimetre on the eye-piece micrometer, whose divisions now represent the divisions of the tube carried forward into its interior. A four inch objective, with an amplification of about sixty diameters, is thus utilized.

F. The probable error of this reading of the level of the meniscus cannot be separated from other probable errors incident to the observation, but the sum total of all the errors whatever is equivalent to no more than a probable error of one-hundredth of a millimetre in this determination: and in the calibration of the apparatus, made before much practice in the use of this reading arrangement, the probable error of a reading was the hundredth of a millimetre.

G. The probable error of a single determination of oxygen in air is less than the four hundredth of one per centum.

H. The method of measurement is adapted to rapid computation. The whole reduction of three measurements in an analysis, so as to get the per centum of oxygen, takes less than two and a half minutes.

NUMERICAL RESULTS FOR THE MEAN RATIO OF OXYGEN TO THE SUM OF OXYGEN AND NITROGEN IN ATMOSPHERIC AIR.

By EDWARD W. MORLEY, of Hudson, Ohio.

[ABSTRACT.]

SINCE the proportion of oxygen to nitrogen in the air varies almost as incessantly as its temperature or pressure, the mean value can be accurately ascertained only by regular and continuous observation. The true value cannot be safely inferred from observations at irregular intervals.

A series of daily analyses in duplicate, continued for six months,

undertaken for a very different purpose, may therefore be of some interest as contributing to the knowledge of this constant.

The result is affected with errors depending on the following causes, the probable magnitude of which is well known.

1. The most serious uncertainty depends on the uncertainty in the calibration of the eudiometer. The probable error of a single determination at any given point on the scale is three milligrammes of water. At each point four determinations were made, so that it is as likely as not that the volume at any given point is known within a milligramme and a half of water. The probable error of the mean result, as far as this error depends on errors of calibration, is 0.0014 per centum.

2. The second source of uncertainty depends on the fact that the composition of the air is variable. Owing to this variation, the result of the number of samples analyzed is as likely as not to differ from the result from an unlimited number of samples by 0.0009 per centum.

3. The third source of uncertainty depends on the accidental errors of analysis. Owing to these errors, the mean result obtained from two analyses of each sample is as likely as not to differ from what would have been obtained by an unlimited number of duplicate analyses of each sample by 0.0002 per centum.

This result is also affected with one source of constant error which has not yet been taken into account. This error is due to the fact that the eudiometer tube, after the explosion, contains a larger volume of water than at the previous measurement. I do not see clearly how to determine the magnitude of this error.

It is at present hoped that other sources of error produce only results which may safely be neglected in comparison with these.

The mean composition of the air, and the magnitude of the errors attending the determination, may then be stated as follows:

Ratio of oxygen to sum of oxygen and nitrogen,	20.949	per ct.
Probable error from imperfect calibration,	0.0014	" "
Probable error from variation in ratio,	0.0009	" "
Probable error from accidental errors,	0.0002	" "
Total probable error,	0.0016	" "

SOME CONCLUSIONS AS TO THE CAUSE OF THE FREQUENT FLUCTUATIONS IN THE RATIO OF OXYGEN TO NITROGEN IN THE AIR AT DIFFERENT TIMES.

By E. W. MORLEY, of Hudson, Ohio.

[ABSTRACT.]

To study the subject above indicated, I have made duplicate analyses of air collected at Hudson, Ohio, on every day from January 1st to June 30th. The results have been graphically compared with the daily observations on temperature and pressure of the atmosphere made by the Signal Service at Cleveland, Ohio, from which Hudson is not far distant. Many of the variations in the amount of oxygen observed are closely connected with variations in the temperature and pressure. For the prediction or explanation of these, local observations of temperature and pressure might be sufficient.

I have also been favored, through the courtesy of General Myer, with the thrice-daily maps of the state of the weather for the period mentioned. From a comparison between these maps and the results of my analyses, which I have only this week begun, I derive some interesting conclusions, serving to confirm the notion that most of the variations in the amount of oxygen are caused by the vertical descent of air from above. I find this notion strikingly confirmed in some cases where it was some time since evident to me that Loomis' suggestion that the cold was caused by the descent could not be accepted as holding good. I find evidence that some depressions of temperature are caused by such descent, and at such times the amount of oxygen falls promptly at the beginning of the cold. But with the times of depression of temperature remarkable for their suddenness and severity, a vertical descent of cold air seems to be the effect and not the cause. The descent follows the cold by a day or two or more, and at the time when the Signal Service maps lead me to suppose the descent has begun, and not till then, the fall in oxygen occurs.

The variations found this year have not been as great as those published by me last year, nor as great as those found by Jolly. I have therefore carefully re-examined those made before this year, and am confirmed in my confidence that they are affected with no unsuspected error.

THE SOIL SUPPLY OF NITROGEN FOR PLANTS. By W. O. ATWATER, of Middletown, Conn.

[ABSTRACT.]

THE nitrogen of the soil, which is the chief source of supply of that material to plants, is subject to more or less loss by the leaching away of nitrates to the lower strata of the soil and thence to rivers and the sea.

The best data at present at hand indicate that, in certain portions of the United States, *e. g.*, in New England, a much smaller portion of the total water which comes to the soil thus escapes by percolation than is the case in England and northern Europe. A larger proportion of the summer rainfall is evaporated from the New England soil, and probably, in our longer and colder winter, the freezing of the ground would do more to prevent downward escape of water here than there.

Accordingly, other things being equal, we might assume a probability of less loss of nitrogen by the leaching away of nitrates from our soils than from those of northern Europe.

If such be the case, our soils, in the regions that have been exhausted by cultivation, might be expected, *ceteris paribus*, to be the richer in nitrogen.

In northern Europe, according to general experience, nitrogenous fertilizers are held to be equally or more important for the increase of crops in ordinary husbandry than phosphatic, potassic, or other mineral manures.

So far as the less definitely measured experience of our agriculture indicates, nitrogen hardly appears to assume a position so important relatively to other ingredients of plant-food as is the case in Europe.

A series of field experiments with chemical fertilizers, executed by several agricultural colleges and private individuals, upon a plan suggested by the writer, have had for their chief end the study of the effects of nitrogenous materials upon the growth of plants in various soils in New England and other states.

The general outcome of these shows a decidedly less relative benefit from nitrogen and more from phosphoric acid, potash, etc., than the teachings of European experience and experiments would lead us to expect.

This fact, though in the nature of the cause less definitely

attested than strict scientific reliability would require, does nevertheless, so far as it goes, corroborate the assumption that our soils are better able to supply our crops with nitrogen than is the case under corresponding circumstances in Europe, and in lack of better explanation we may not be entirely wrong in attributing the difference, in part at least, to the climatic conditions which produce a smaller percentage of percolation of water of atmospheric precipitation here than in Europe.

Our agriculture suffers, in comparison with that of some other regions, through dry weather and more frequent drought.

Nitrogen is of immense importance in agriculture and its lack can be supplied only at immense cost. If the hypothesis above suggested be correct, we have in the fact which it explains a partial compensation for the loss which our lack of atmospheric water-supply entails upon us.

THE DETERMINATION OF PHOSPHORIC ACID BY THE MOLYBDIC METHOD.

By W. O. ATWATER, of Middletown, Conn.

[ABSTRACT.]

I. EFFECT of presence of organic matter upon the precipitation of ammonium phospho-molybdate.

It has been stated (*e. g.*, by *Schumann*, *Fres. Zeit.*, 14, 301) that phosphoric acid may be accurately determined by precipitation with ammonium molybdate, in presence of organic matter. But analyses are adduced showing that the determination may be materially affected by organic matter, even after treatment with strong nitric acid. The organic matters were found, in a number of cases, to partially prevent the precipitation of the phosphoric acid.

II. Precipitation of small quantities of phosphoric acid in the presence of alkaline salts.

It has been shown that the precipitation of phosphoric acid as phospho-molybdate is hindered by certain compounds, as hydrochloric acid, sundry sulphates, etc., and aided by nitric acid, ammonium nitrate, and the ordinary solution of ammonium mo-

lybdate in nitric acid. (See *Fresenius, Fres. Zeitschrift*, 3, 446; *Richter, Dingler's Polyt. Journal*, 199, 183, *etc.*)

Sundry experiments were made to test the influence of sodium nitrate upon the determination of minute quantities of phosphoric acid by the molybdic process. It was found that, up to a certain proportion, the presence of sodium nitrate did not hinder the precipitation of even minute quantities of phosphoric acid. This fact has a direct bearing upon the determination of phosphorus in organic substances, since it shows that, when the latter are fused with sodium carbcnate and nitrate, and the products of fusion dissolved in nitric acid, the phosphorus can be accurately estimated by the molybdic process.

THE DETERMINATION OF SULPHURIC ACID. By W. O. ATWATER, of Middletown, Conn.

[ABSTRACT.]

THIS paper gives results of experiments upon effects of varying quantities of sodium chloride upon the precipitation of sulphuric acid as barium sulphate. The method and results are analogous to those in the previous paper. They bear upon the determination of sulphur in organic compounds in that they confirm results obtained by other experimenters and indicate that sulphur may be accurately estimated by burning the substance in sodium carbonate and nitrate dissolving in hydrochloric acid and precipitating with barium chloride.

These experiments have however less interest than the previous ones — on the determination of phosphoric acid — since they only confirm generally accepted statements.

THE DETERMINATION OF NITROGEN BY THE HYPOBROMITE PROCESS. By W. O. ATWATER, of Middletown, Conn.

[ABSTRACT.]

A number of experiments were given which indicate that the absorption of nitrogen by the hypobromite solution affects the

determination of nitrogen by this process very materially and may account, in part, for the inaccurate results so commonly obtained in determinations of urea by Hüfner's method.

THE QUANTITATIVE DETERMINATION OF FATS. By W. O. ATWATER, of Middletown, Conn.

[ABSTRACT.]

A number of experiments were adduced to show that in the determination of fats in vegetable substances in the ordinary way, the drying, which is sometimes necessary for correct results, may, if conducted in air, lead to serious error. This is probably due to oxidation of the fats by which they are rendered insoluble in ether, carbon disulphide, etc., the re-agents commonly used for extraction. These re-agents are found to extract much more from the same materials dried in hydrogen.

TITLES OF OTHER PAPERS READ IN THE PERMANENT SUBSECTION OF CHEMISTRY.[1]

THE CHEMICAL COMPOSITION AND NUTRITIVE VALUES OF FISH. By W. O. Atwater, of Middletown, Conn.

SOME NEW FORMS OF APPARATUS. By W. O. Atwater, of Middletown, Conn.

CONSTITUTION OF THE TARTRATES OF ANTIMONY. By F. W. Clarke and Helena Stallo, of Cincinnati, Ohio.

DIRECT COMBINATION OF HYDROGEN AND NITROGEN. By W. Colegrove, of Boston, Mass.

THE GENESIS OF CERTAIN IRON ORES. By T. Sterry Hunt, of Montreal, Canada.

[1] Many of these papers were printed in abstract or in full in SCIENCE, soon after the meeting, and some have been printed in the AMERICAN CHEMICAL JOURNAL, Baltimore.

SUBSTITUTION OF CONES MADE FROM PARCHMENT PAPER FOR PLATINUM CONES IN BUNSEN'S PROCESS OF FILTRATION. By L. P. Kennicutt, of Cambridge, Mass.

FOREIGN SUBSTANCES IN IRON. By J. L. Kleinschmidt, of St. Louis, Mo.

ACTION OF SUNLIGHT IN THE PRODUCTION OF CHLORINATED ADDITION-PRODUCTS OF BENZENE AND NAPHTHALENE, WITH DESCRIPTIONS OF TWO NEW CHLORINE DERIVATIVES OF NAPHTHALENE. By Albert N. Leeds, of Hoboken, N. J.

ACTION OF HYPONITRIC ANHYDRIDE UPON ORGANIC SUBSTANCES, WITH DESCRIPTIONS OF THREE NEW OXYGENATED DERIVATIVES OF THE AROMATIC GROUP—MONOXYBENZENE, TETROXYNAPHTHALENE AND NAPHTHODIQUINENE. By Albert N. Leeds, of Hoboken, N. J.

ON THE ILLUMINATING GAS OF NEW YORK CITY. By E. G. Love, of New York, N. Y.[2]

THE SUBSTITUTED ACRYLIC ACIDS. By G. F. Mabery and Mrs. R. Lloyd, of Cambridge, Mass.

ON THE ACTION OF VEGETABLE ACIDS ON TIN. By C. E. Munroe, of Annapolis, Md.

A MODIFICATION OF BERTHIER'S PROCESS FOR THE VALUATION OF COAL. By C. E. Munroe, of Annapolis, Md.

INCRUSTATIONS FORMED IN PIPES USED IN GAS WELLS. By H. B. Nason, of Troy, N. Y.

OBSERVATIONS ON THE TEMPERATURE AND CHEMICAL CHARACTER OF THE WATER OF MYSTIC LAKE, MASS., AT DIFFERENT TIMES AND AT DIFFERENT DEPTHS. By Wm. Ripley Nichols, of Boston, Mass.

THE VALUATION OF INDIGO. By L. M. Norton, of Manchester, N. H.

FORMATION OF SULPHOTEREPHTHALIC ACID BY DIRECT OXIDATION. By Ira Remsen, of Baltimore, Md.

COMMERCIAL TESTING OF SUGAR, ILLUSTRATED BY SAMPLES OF SUGAR AND INSTRUMENTS USED. By S. P. Sharples, of Boston, Mass.

ON A SOLUTION OF FERRIC-GALLATE AND FERRIC-OXALATE AS A REAGENT FOR QUANTITATIVE ANALYSIS OF AMMONIA. By N. B. Webster, of Norfolk, Va.

[2] Published in full in THE AMERICAN GAS LIGHT JOURNAL, New York, for Oct. 2, 1880.

PERMANENT

SUBSECTION OF MICROSCOPY.

PAPERS READ.

MICROSCOPICAL COLLECTIONS IN FLORIDA. By C. C. MERRIMAN, of Rochester, N. Y.

IT has been my fortune during the past two winters to spend a few weeks in the regions of central Florida. Lake Harris is the most southern and the most beautiful of the cluster of lakes which forms the source of that exceedingly picturesque river, the Oklawaha. With high banks and surrounded by a belt of hummock land, as rich as any that Florida affords, this lake is becoming settled upon, and the lands are fast being taken up by enterprising southerners for orange groves and pine-apple plantations. The sojourner will find the society of this lake settlement intelligent and hospitable beyond anything that would be suspected in so new and pioneer a country. The vegetation of this almost tropical region is so full of interest to the microscopist, and the causes conducing thereto so peculiar, that I have thought them deserving of especial mention and illustration.

The absence, or at least the rarity of frosts, injurious to vegetation in these lake districts, gives the longest possible season for the growth and maturity of such organs as are best or especially adapted to the exigencies of Florida plants. There is a period of rest, usually comprising about the three winter months, after which vegetation takes up and continues its growth again as if there had been no period of interruption; so that practically there is a continuous development of plant life, whether annual or perennial, from birth to death.

The soil of Florida, as of all the south Atlantic seaboard, is sandy and naturally barren. No polar glaciers have ground up for these regions, as for the northern states, a rich and abundant alluvium, sufficient in itself for the production of a rapid and vigorous vegetation. *The south* has apparently only the siftings of our northern soil, carried down to the ocean by rivers, and then washed up by the sea-waves to form their interminable sandy plains. But to compensate for this natural infertility of soil the atmosphere, especially of southern Florida, abounds in all the

elements of plant growth. The winds which come up from the Gulf on the one side, or the Atlantic on the other, are charged with moisture, and bear also minute quantities of nitric acid and saline compounds; while the exhalations from the swamps and marshes furnish in abundance the salts of ammonia and carbonic acid. Now to utilize these precious products from the air it is necessary for plants to have peculiar organs, such as absorbing glands, glandular hairs, stellate hairs, protecting scales, and a variety of other special appendages. All these have been developed by time and necessity in remarkable profusion and perfection on the vegetation of southern Florida. Although the meagre soil produces no nutritious grasses, and scarcely enough of an honest vegetation to keep an herbivorous animal from starving, yet there is ar abundant flora, such as it is — air plants, parasitic growths, insectivorous plants, and strange herbs seeking a livelihood in any other way than the good old honest one of growing from their roots. It is this fact which makes the microscopical interest of botanical researches in central Florida. One can scarcely examine, with a two-thirds objective, the flowers, leaves or stems of any plant growing there, without discovering some beautiful or striking modification of plant hairs, or scales, or glands, or other absorbing or secreting organs.

We will notice first the Onosmodium as found in Florida — *O. Virginianum*. It grows from Virginia south, but is more glandular, I think, in Florida than anywhere else. It will be almost the first plant one will stop to observe on entering the piny woods — a dark green, narrow-leaved, biennial herb; its straight stem of the second year's growth, about a foot high, bearing a raceme-like cluster of flowers, coiled at the end, and straightening out as the flowers expand. The leaves of this plant are thickly studded on both sides with stiff transparent hairs, lying nearly flat on the surface, and all pointing towards the tip end of the leaf. At the base of each hair is a cluster of glandular cells, amounting sometimes to fifty or more, and arranged in beautiful geometrical forms. When pressed and dried in the herbarium, the body of the leaf turns to a very dark green, almost black, and on this background, with a half-inch objective, the hairs stand out like sculptured glass, and the glands like mosaics of purest pearls. I think it is the most attractive opaque object that can be shown under the microscope.

That these glandular cells, covering, as they do, nearly half the surface of the leaves, especially the upper surface, and differing from all other vegetable cells, subserve an important purpose in the sustenance of the plant, there cannot be any doubt. But just what that purpose is, or what is the mode of operation, I think, has never been ascertained.

In the same locality will very likely be found the most beautiful of all the Croton plants, the *C. argyranthemum*. Unlike the other Crotons, which are bushes, this is an herb growing only about a foot high, with a milky sap which exudes when the stem is broken. The leaves are silvery, verging in some cases to a bronze color, and are thickly covered on the upper side with most remarkable and beautiful stellate scales. The flower buds and stems when pressed make much more beautiful opaque objects than the leaves.

The object of these scales is without doubt to prevent the too rapid evaporation of the moisture stored up in the plant. They are the exquisitely woven blankets which preserve the precious juices so laboriously gathered. The same kind of covering is spread over the leaves and stems of all the air-plants of Florida, and doubtless for the same purpose. The well-known Florida moss, although not a moss, but a member of the Pineapple family (*Tillandsia usneoides*), is an exceedingly beautiful object under the microscope. Each hanging stem is overlaid with filmy white scales, every one of which is fastened in its place by what would seem to be the stamp of some miniature seal on golden-tinted wax. This plant, as ordinarily seen on the live-oaks near cities, is a dirty-looking and unattractive object, and goes by the name of "black moss." But in out-of-the-way places, removed from the dust and smoke of settled localities, it is pearly white and exceedingly beautiful, both to the naked eye and under any power of magnification. Florida moss should be preserved with only very slight pressure, just enough to make the threads lie straight. After it is dried in this way, small cuttings may be mounted in the ordinary cells for opaque mounting.

On the high banks of the lake, and in the adjoining fields, may be found the large leaved and vigorous growing Calicarpa (*C. Americana*), sometimes called the French mulberry, a bush growing some five or six feet in height. The under side of the leaves of this plant is nearly covered with little round yellow sessile glands, flattened on top and marked off into eight or ten sections

by ribs, like those on a melon. They are in immense numbers—something like thirty thousand to the square inch—over half a million on a good sized leaf. Under a light net-work of branching glandular hairs, viewed with a two-thirds objective, these polished amber-colored disks glisten like a spangle of golden beads. The same kind of glands are found on the leaves of many other shrubs in Florida—the sweet myrtle (*Myrica cerifera*), the low ground blueberry (*Vaccinium tenellum*), a certain bush or dwarf hickory (*Carya glabra*), and some others. These glands have been variously called resin dots, resin glands, and odoriferous glands. So far as I can judge, however, they are not connected with any resinous or odoriferous secretions. From their almost perfect resemblance to the terminal bulb of the mushroom glands of the Pinguicula and Drosera, which are known to be absorbing glands, the probability is that these also serve to absorb moisture and ammonia from the atmosphere and from rains; although I am free to acknowledge that the position of the glands, being for the most part on the under side of the leaves, militates somewhat against this view of their purpose.

Great care will have to be taken in pressing and drying vegetable specimens in the moist climate of Florida. The little threads of the mould fungus will be sure to creep over the surface of the leaves, spoiling them for microscopical material, if they are not quickly and effectually dried. For this purpose it is well to have a good supply of the bibulous botanical paper, and to change the specimens every day to fresh sheets for at least four or five days. The sheets after being once used should be spread out in the sun to dry. A weight of about thirty pounds may be used as the pressure.

The objects heretofore mentioned are all for opaque mounting. Almost every preparer of slides has his own favorite method for this kind of work. I myself prefer the use of the transparent shell-lac cells. Clarified shell-lac is dissolved in alcohol, and filtered through cotton wool under a bell-glass and with the application of heat. The solution is evaporated down until it is so thick that it will only just run, almost like a jelly. In this condition it can be put on a slide with a camel's-hair brush on the turntable, and very quickly worked up into a ring with the point of a knife, used first on the inside to make the cell of the size wanted, and then on the outside to turn the cement up into a compact ring.

Two or three applications of the cement, with intervals of a day or two after each, will make cells of sufficient depth for all ordinary specimens. These cells dry quite slowly, and if artificial heat is used, it must be increased only very gradually, otherwise vapor of alcohol bubbles will make their appearance in them. A small ring of Brunswick black may be made in the inside of the cell, to which, when thoroughly dry, the object may be fastened with a very little liquid marine glue. In this case both sides of the leaf can be seen, which is often desirable. In all opaque mountings a minute aperture should in some way be left open into the inside of the cell, so that it shall not be hermetically sealed up. This little precaution will save an innumerable number of failures.

The collector in Florida will not fail to secure a supply of the leaf stems of the castor-oil plant (*Ricinus communis*). In regions beyond the influence of frosts this plant grows continuously from year to year, and becomes quite a tree. It is only in such a growth that the spiral tissue of the fibro-vascular bundles is fully perfected. The castor-oil plants grown in our climate, during one short season, will furnish very little spiral tissue, mostly spotted ducts, and scalariform cells. There is no more beautiful object for multiple staining than thin longitudinal sections through the woody fibre, the vascular tissues, and the pith cells of well matured leaf stems of the castor-oil plant.

I will briefly describe my process of making these stainings. After being decolorized in chlorinated soda, the sections may be left for half a day or more in a solution of carmine in water containing a few drops of aqua ammonia; then for half an hour in a rather weak solution of extract of logwood in alum water, and finally ten to fifteen minutes in a weak solution of aniline violet or blue in alcohol. From this they can be carried through absolute alcohol into turpentine, and mounted in balsam at any time thereafter. If successful in this staining you will have the pith cells in red, the spiral tissue in blue, the wood cells in purple, and the stellate crystals in green or yellow.

But the chief objects of interest to the microscopist in the vegetation of Florida are the insectivorous plants. Not only are they more abundant, and as I think more perfectly developed in the central lake regions of Florida, but some varieties are found there differing, it seems to me, from any found elsewhere. I desire particularly to mention one which I discovered, and which

perhaps might be entitled to rank as a new species. In a lagoon-like basin at the side of a small lake near Lake Harris, in water from two to three feet deep, I found numerous specimens of the insectivorous plant, known as the Drosera or sun-dew, growing thriftily and floating about among the scattered water weeds, without any attachment whatever, indeed with very little root of any kind, the dead leaves that hung down in the water seeming both to buoy it up and to hold it upright. This plant differs from all the described species of Drosera, so far as I have been able to ascertain, in having an upright leaf-bearing stem, four to five inches long, in floating free on the water, and in having unusually long, vigorous, and numerous leaves. As I never found this floating Drosera in any other location, and as there was an abundance of the ordinary *Drosera longifolia* growing on the adjoining shore, I could not resist the suspicion that, at this very spot in some past time, a plant of the *longifolia* had by accident become uprooted, and floated out on the water — that finding it could capture insects even better on the water than crowded among shore plants, it adapted itself permanently to its new location and modes of growth. It appeared to me quite within the bounds of probability that here was an instance of the evolution of a species *in loco*.

The Drosera or "sun-dew" is found on the margins of nearly all small ponds and permanently wet places throughout the south. It is a small red plant growing close to the ground, and glistening in the sunlight. Its little whorl of expanded leaves forms a circlet as beautiful as any flower, and often so very small that I have frequently mounted whole plants with flower stalk and buds on one slide. Each leaf of the Drosera has, spread out on its upper surface and edges, from two to three hundred arms, called tentacles, because endowed with the power of motion, and of such varying lengths that when naturally incurved their ends just meet at the centre of the leaf. Each tentacle has, at its extremity, a pad, like an extended palm, with a ridge raised lengthwise upon it; and in this palm is a bundle of spiral vessels connected with the same tissues in the leaf. Now all these tentacles secrete and exude from the glands at their ends a little drop of a very adhesive fluid; and the glistening of these drops in the sunlight on their usually bright red back-ground gives to the plant its beauty and its name of the "sun-dew." An insect attracted to and alighting

on these leaves is inevitably held fast. The tentacles by which it is held very soon begin to bend towards the centre of the leaf, carrying the fly with them. Then in some mysterious way intelligence is communicated to the other tentacles, and they too begin to turn towards the centre of the leaf, in the course of an hour or two completely covering the captured prey. If the insect is caught entirely on one side of the leaf then only the tentacles of that side inflect. The glands, after envelopment, exude a gastric fluid which dissolves the nitrogenous matter in the body; after which, by another change of function, they absorb and carry down into the plant all this nutritious little feast. In the course of three or four days the tentacles again expand and prepare themselves for another capture.

There are several reasons which lead me to believe that these unique and most wonderful organs of the Droseræ are a direct and special development from the common simple mushroom glands, which are found on many plants and which have for their primary function to absorb moisture and ammonia from the atmosphere and from rains. I found on the calyx and flower stem of the Drosera an abundance of these mushroom glands. Indeed, the flower stem with its buds furnishes by reason of them an exceedingly beautiful object for the microscope, both in a natural state, and when prepared by double staining. I have found it quite a general rule as regards plants, that whatever organs, such as stellate hairs or glands, the leaves may possess, the calyx and stem of the flower will show them in far greater luxuriance and beauty. The stellate hairs of the Deutzia, the Crotons, and the Shepherdias are far more numerous and striking on the flower buds than on the leaves. The mushroom glands which are found on the leaves of the Saxifrage and Pinguicula are multiplied many-fold in number and attractiveness on the calyx and flower stems of these plants. So I regard that this was once the case with the Drosera; and that the mushroom glands, which are now found on the flower, were then common to the leaves. A process of evolution has transformed them on the leaves into those wonderful motile arms adapted to the capture of insects, but has left them unchanged on the flower, where that function would be of no use to the plant. I occasionally find in my preparations a solitary mushroom gland among the tentacles of the leaf—a remnant of a race that has been supplanted. There is found in Portugal a

of the utricles and the captures contained in them. Specimens of new growth, showing the just forming utricles and the peculiar circinate mode of growth, should be included on the slide. The mounting should be in carbolated water.

The Pinguicula, another of the insectivorous plants, is found abundantly on the more open plains, and not far from wet places. It is a compact rosette of very light green leaves, growing close to the ground, from the centre of which rises a single flower stalk eight or ten inches high. The leaves have their edges turned up forming a shallow trough, and on the upper surface are mushroom glands which exude a viscid secretion. Insects are caught and held by this sticky substance until they die. The nutritious matter is then dissolved out by an acid secretion, and this is ultimately absorbed into the substance of the plant by the glands on the leaf. The edge of a leaf when excited by a capture will bend over upon it for a short time,—merely for the purpose, I think, of more effectually securing it, and of bathing it in the secretions.

The calyx and flower stalk, as I have already mentioned, are thickly covered with the same mushroom glands that are found more sparingly on the leaves. I have never seen any evidence that the flower appendages took any part in the digestion of insects. They seem to be rather in the nature of an ornamentation, than of anything useful. For exhibition, therefore, or for double staining, the calyx and flower stem will be found by far the most attractive part of the plant. The best way to preserve them, as well as all such small material, until wanted for use, is to put them green into a common morphia vial with a few drops each of alcohol and water, and then to cork and seal them up tight with melted beeswax. To prepare them for the slide, these objects may be treated precisely as recommended for sections of castor-oil plant, but should be mounted in a weak solution of glycerine in camphorated water.

If cells are made of rings punched out of the thin sheets of colored wax used by artificial flower makers, and then coated with either liquid marine glue, or a mixture in equal parts of gold size and gum damar dissolved in benzole, this method of liquid mounting may be as easily and safely performed as mounting in balsam. In very many cases simple water, made antiseptic in any manner, will be found far preferable to any other media, both for retaining the full and distended forms of minute organs, and for bringing

plant very similar to the Drosera, the Drosophyllum, which has still only the mushroom glands on its leaves, and catches insects in great quantity by loading them down with the viscid secretion which these glands abundantly pour forth.

To exhibit the very delicate structure of the leaf and tentacles of the Drosera, it is necessary to color them but slightly. The danger will be in overstaining. Therefore, after decolorizing and immersion for a few hours in the carmine solution, the specimens should be exposed to only a very weak fresh solution of logwood for fifteen or twenty minutes. If the aniline blue is resorted to at all, it must be in a very weak solution. A mounting of a leaf and a stem with flower buds in one cell, in camphorated or carbolated water, makes a very pretty and complete slide for the Drosera.

The Utricularia is a floating carnivorous plant which grows in the shallow water of quiet ponds. On the surface of the water from five to seven leaves are spread out like the spokes of a wheel, and from the centre of these leaves the plant sends upward its flower stalk and downward its root-like branches, floating freely in the water. Among the thickly branching fibres of these long submerged stems are perched innumerable little bladders or utricles, not much larger than the head of a pin, each provided with a mouth, at the bottom of a sort of funnel of bristles, closed with a cunning little trap-lid which opens inward, ingulfing and imprisoning whatever minute creatures or substances may happen to be resting on it. In these sacks during the growing season we will find numerous microscopic water fleas, mites and beetles, with grains of pine pollen and other floating particles. The organic bodies will be found in all stages of digestion, showing that the plant derives nourishment from such captured prey; and apparently its only means of livelihood is trapping.

When taken from the water and dried under slight pressure, the submerged portions of the Utricularia will be found literally covered with diatoms; and many very interesting chrysalids of water insects will be found attached to them. These will all be washed off if the plant is bleached in chlorinated soda. To preserve them it will be necessary to remove the color in alcohol, and besides to handle very carefully. The staining can only be single, and I have found a weak solution of aniline eosine in water to be the best material for coloring, showing at the same time the structure

of the utricles and the captures contained in them. Specimens of new growth, showing the just forming utricles and the peculiar circinate mode of growth, should be included on the slide. The mounting should be in carbolated water.

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out the delicate markings of vegetable structure which the highly refractive balsam would entirely obliterate.

There is only one other insectivorous plant found in Florida—the pitcher plant, *Sarracenia variolaris*, a species growing only in the South Atlantic States. It is found in low and wet places among the open pine barrens, but is not as abundant as the others which have been mentioned. The leaf is a hollow, conical, or trumpet-shaped tube, with a flange or wing running up one side, and a hood which arches over the orifice of the tube. During the growing season this tube is usually more than half filled with water, which we must suppose secreted by the plant itself, because the hood effectually sheds all rain water from it. Crowded into the bottom of the tubes of mature leaves we shall almost invariably find a mass of the hard and indigestible parts of insects. These creatures have been in some way attracted into that suspicious looking receptacle, and once in have been unable to get out again. A mere partially covered tube, however, with a little water in it, is by no means a fly-trap. Not one insect in a hundred would fall into that well and drown, if there were not some special device absolutely preventing it from crawling upward. Now a microscopical examination of the inside of the hood and tube of the pitcher plant reveals the most skilful contrivances for securing insect prey that could possibly be imagined. In the first place, there are in the upper part of the receptacle, and about the mouth, great numbers of sessile glands which secrete abundantly a sweet fluid very attractive to ants and flies. Further, there is, on the inner surface of the hood and mouth, a formidable array of comparatively long, pike-pointed spines, all pointing backward and downward. These grade off into shorter, more blunt, but still exceedingly sharp-pointed spines which overlap each other like tiles on the roof of a house. This kind of coating lines the tube for a third of the way down, the spines growing finer until at last they grade off into regular hairs which line all the lower part of the tube; spines and hairs all pointing downward. An insect, attempting to retrace its steps after its ambrosial feast, would find nothing which it could penetrate or grasp with the hooklets of its feet; and the wetness of the spines, from the constantly overflowing glands, would probably prevent it from making use of any other device that insects may have for climbing glazed surfaces.

As a matter of fact, no creature comes out of that prison house; unless it be with the single exception of one cunning spider, which in some way finds a safe and rich retreat under the hood of its great vegetable rival.

The bodies of the captured prey fall into the fluid in the tube, and are macerated or decomposed, but without any signs of putrescence. Therefore the plant must at once absorb the animal matter, for otherwise this would cause the infusorial life which is called putrefaction.

In order to show the internal structure of the pitcher plant leaf, it will be necessary to separate the cuticle which bears the spines and glands from the rest of the leaf. To do this pieces cut from the leaf, and preferably those showing the transition from one kind of spines into another, after being soaked in water, may be put into common nitric acid, and this brought up to the boiling point over an alcohol lamp. They should then be immediately washed in several waters, when it will probably be found that the cuticle, both the inner and the outer, has already separated from the parenchyma. The specimens will need no further bleaching, and may be stained either in eosine, the aniline orange color, dissolved in water, or in aniline blue in alcohol. As there is only one kind of tissue to be stained, it will be impossible to get more than one color in them. They should be mounted, or kept in water very slightly acidulated with carbolic acid.

I cannot but regard the pitcher plant as the most highly developed, the most specialized in its organization of any of the insectivorous plants. It differs more widely from ordinary vegetation, and has more special and adapted contrivances about it than any of the others. Now, as I believe that the truth of the modern evolutionary theory will be eventually brought to the test by well studied monographs, made by microscopists, on some such highly differentiated organic structures as the pitcher plant, I do not deem it a digression to present here briefly some inferences which seem to me to arise from the developmental history of this particular plant.

Of course, if the pitcher plant was developed from other and ordinary plants, it had at one time the simple plain leaves of common herbs. It must have early commenced, in some way, to appropriate insect food on these leaves, because every essential change was for the betterment of the plant in this respect. The

stem of the leaves soon began to put out flanges or wings on each side — the phyllodia of the botanists, which are not uncommon among plants. And these outspread wings must have assisted in the absorption of insect food that was washed down upon them. Then the edges of the wings turned up, and curved around towards each other, until finally they met and grew together, forming a tube and a much more complete receptacle for decomposing animal bodies. A South American genus, the *Heliamphora*, is just in this condition at the present time. Then from some unknown cause, and in a way exceedingly difficult to explain, our Sarracenia changed entirely its manner of capturing insects. The leaf bent over the orifice of the tube, forming the hood, and those remarkable spines and tiled plates were developed on the inside of the hood and tube, growing backwards contrary to the order of nature. When all this was accomplished and fully completed, but not before, our plant could commence its career as the most successful trappist of either the vegetable or the animal kingdom.

Now, according to the Darwinian theory, all these transformations were the result of innumerable slight and accidental variations, each one of which happened to be so beneficial to the particular plant concerned, that it got the start of all the others and every time run them all out of existence. One cannot tell how many million times this extinction and reproduction must have occurred before our marvellously perfect little flytrap was finally produced. Excuse me, if I confess that all the canonical books of Darwin are not sufficient to make me put faith in the miracles of accidental evolution. I believe in the fact of the gradual development of the organic kingdoms; for all science teaches it. But I believe it was governed and guided by forces more potent than accident and chance. The Being, or the first cause, if you will, that originated the simple elements of matter, and endowed them with the power and the tendency to aggregate into developing worlds, might equally as well have endowed certain of them with the power and the tendency to aggregate into ever-advancing organisms. There is no chance in the myriad forms of crystalline and chemical substances; then why should there be in the scarcely more varied colloid forms of living matter? In a world that unfolds from chaos in one steady line of progress, that shows only design at every advancing stage, I must logically place somewhere at its commencement the Almighty fiat of a Designer.

Sulphuric-acid treatment of Seeds and its value to Cotton Planting and Culture. By Thomas Taylor, of Washington, D. C.

While making some microscopical examinations about fifteen months ago, to determine the position of the oil-cells in Indian corn, I found that the germ of this seed was remarkably well protected from accidental injury. It is surrounded by a series of oil-cells, outside of which are many layers of starch-cells, filled with granules of starch. Bounding these cells is a second series of oil-cells in close contact with, and under the external cellulose coating of the seed.

As one of the results of this investigation, it occurred to me that seeds of this structure might with entire safety be subjected to chemical action, with a view to effect certain chemical changes, in those outer portions which serve as nourishment to the germ, thus accelerating the process of germination. For the purpose of experiment on this point, I selected sulphuric-acid for the reason, that, like diastase, this agent has the property of converting starch and dextrine into grape sugar, substances used as plant food in the germination of the embryo.

Indian corn is a type, in its internal structure, of a large class of seeds. The cotton-seed is a conspicuous example of another class.

In the cotton-seed the entire space inside the pericarp is occupied by the embryo or miniature plantlet. This plantlet, if removed and subjected to chemical treatment, will be seen, by the aid of the microscope, to be charged with starch, oil and albumen, thus showing that the plantlet contains within itself the food necessary for its proper germination.

Owing to such differences of structure, the action of sulphuric-acid is not alike on all seeds. In one case the acid may be employed to act directly on the proximate principles of the seed, while in others, as in the case of the cotton-seed, its action may be confined to the process of reducing the non-conducting substance of the hull, so that heat and moisture from without may have more ready access to the germ or plantlet.

In the cotton-seed, the benefits of the acid treatment are conspicuously manifest. The cotton-seed of commerce is covered with matted short fibres of cotton, firmly adhering to the compact woody shell or pericarp. As a result of experiment, I have found

that two ounces of strong sulphuric-acid poured over sixteen ounces of cotton-seed, as now used for planting, completely dissolves the cotton-fibre on its surface, changing it into a gummy substance soluble in water. If sufficient time is allowed for the acid to act on the hull, the latter is reduced in thickness and rendered more porous. On washing the seeds thus treated with plenty of water, the gummy substance is removed, and the seeds appear clean and smooth, resembling in color a ripe apple-seed.

When the seed is thus cleaned there is no mechanical obstacle to the use of machinery in planting it, whereas it is now planted by hand throughout all the cotton-growing regions of the world; but before this advantage can be utilized by the planter, it is essential to show that the germ of the seed is uninjured by the use of the acid. If a cotton-seed, treated as above described, is immersed in water for a period of say twenty-four hours, the hull may be removed by the careful use of a knife and the leaves and plumule of the plantlet thus exposed may be examined by a low power of the microscope.

If sufficient mechanical care has been observed in the removal of the hull, the plantlet will appear perfect in all respects, showing that no chemical injury has been sustained on its surface by means of the action of acid on the external surface of the hull or pericarp. From these and other facts thus gained by the use of the microscope, I became firmly impressed with the conviction that cotton-seeds thus treated were not only uninjured, but would germinate earlier than seeds planted in the usual manner. But to leave no doubt upon this point, I determined to test such seeds by having them planted under conditions favorable for germination; and to this end I placed them in the hands of Mr. Wm. Saunders, Superintendent of Gardens and Grounds of the Agricultural Department at Washington. I visited daily the hot-house in which these seed-experiments took place and took notes of their daily relative growth. The experiments were conducted in the following manner:—

In every experiment two samples of seed were used and placed in separate pots side by side and under the same general conditions. One lot was treated with sulphuric-acid, the other planted in the usual manner without chemical treatment. In this way we could judge of the acid treatment by comparison of growth. The first cotton-seeds planted were subjected to an average temperature of about 67° Fah. It was during the winter. The seeds

treated with acid germinated in about eighty-two hours, the untreated in about one hundred and sixty-four hours.

When similar experiments were made under an average temperature of 85° Fah., the treated seeds germinated in about forty hours; the untreated in about ninety hours. One sample was treated with full strength sulphuric-acid of commerce for a period of one hour. The seeds were next removed from the acid, placed on a glass-plate and allowed to remain in that condition unwashed for a period of twenty-four hours, after which they were washed in water and planted. After a lapse of thirty hours these seeds germinated, and they have produced thrifty plants which are now in bloom and partly bolled. Hundreds of cotton-plants have been successfully grown from the seeds used in these experiments; and in every case, where comparison was made, seeds treated with strong sulphuric-acid germinated earlier than those not so treated. Several persons in the city of Washingon, D. C., have planted, in open ground, treated cotton-seeds, furnished by me. All of them have germinated and produced healthy plants some of which are already bolled. My own garden furnishes several examples.

In all these experiments the seeds were planted, according to hot-house custom, near the surface of the soil. The cotton-seeds were planted about one-fourth of an inch below the surface. When planted in the field, they have a covering of about three inches of earth. Treated seeds, planted at that depth, would during high temperature germinate much earlier relatively than seed untreated; because the cotton covering and thick hull offer more resistance to transmitted heat at a depth of several inches, than they will when nearer to the direct rays of the sun.

Theoretically and, I believe, practically the planter will gain several days' time by planting treated seeds. In the process of washing, the light, inferior and wormy seeds, which float on the surface of the water, may be at once removed. The heavy and plump seeds fall to the bottom.

The washings should be saved and thrown on the home-made manure pile. The sulphuric-acid they contain will absorb the escaping ammonia and thus preserve from waste a valuable fertilizing agent.

During the past year I have made many experiments with other seeds, including cranberry, tobacco and sorghum seeds, wheat, oats, peas, beans, Australian tree-seeds and palm. All seeds,

treated by soakage in full strength sulphuric acid of commerce, germinated without failure.

Two sets of old and hard Australian tree-seeds were planted, one set having been severely treated with acid, full strength.

There was a marked difference in the time of germination between the treated and untreated seeds. All of the treated seeds germinated within one month, while the non-treated did not germinate until the expiration of three months after planting.

Beans were so severely treated that their hulls separated and shrivelled up, yet all germinated and grew vigorously.

A palm-seed measuring 2½ inches long by 1½ inches across, and as hard as vegetable ivory, was soaked in a mixture of sulphuric-acid, diluted with fifty per cent. of water for a period of twenty-four hours, and to render the soakage the more complete, the shell of the nut at the base end was removed to the extent of half an inch in diameter superficial measurement.

This seed germinated three weeks after planting, although the usual period of germination of the seed under hot-house culture is sometimes several years. Seeds which ordinarily germinate in a short period of time are not improved by the acid treatment, as far as my experiments have gone. Its utility has been most conspicuous in the case of cotton-seeds and hard and old tree-seeds, including palm-seeds.

It is my present purpose to continue my experiments with seeds, and any useful results that may be attained will be made known to the public in the Reports of the Department of Agriculture or other publications. In the meantime, it is my intention to have treated cotton-seeds tested in the open fields in the coming season on a scale sufficiently large to ascertain practically the benefits of the sulphuric-acid treatment.

The following extract from a letter from a practical cotton planter, formerly of North Carolina, sets forth clearly the possible advantages to be derived by the planter from cotton-seeds thus treated:—

WASHINGTON, D. C.
24th July, 1880.

MR. THOMAS TAYLOR,
Microscopist, Department of Agriculture.

DEAR SIR:—

I know something about the raising of cotton, having spent the greater part of my life in Cabarrus County, North Carolina, where cotton is our money crop.

Your discovery of removing the lint from cotton-seed without injuring the germ is of great value.

In the first place: as you prepare them, the seed can now be drilled at regular intervals apart, as easily as grains of corn, and this will, to a great extent, if not entirely, do away with the labor of thinning out the weed known among planters as "chopping cotton." This regular drilling was not possible by any machine while the seed was coated with lint, for the lint held the mass together, so that the individual grains could not be separated.

In the second place: the seed, as you prepare them, come up much earlier than what we may now call "lint-seed."

This makes them doubly valuable to the planters of the Northern belt of the cotton region, where the frost so often catches the crop, cutting it down, not unfrequently one-third. The earlier start of the prepared seed will much reduce the damage now done by the frost.

Yours truly,

WM. M. COLEMAN.

PERMANENT MICROSCOPIC PREPARATIONS OF PLASMODIUM. By SIMON H. GAGE, of Ithaca, N. Y.

THE previously published methods of making permanent preparations of the motile or naked protoplasmic stage of the Myxomycetes are but two, so far as I know; and no method of getting the Plasmodium in a desired position has to my knowledge been published.

The old method was to dry the extended Plasmodium, the new is to harden it with osmic acid. Both these methods are defective, for osmic acid changes the color of the protoplasm, and drying causes it to shrink as well as to change color.

The following is a simple and efficient method of extension and preservation: Small pieces of the rotten wood, on which the Plasmodium is found, should be placed on moistened microscope slides with some of the Plasmodium touching the slides. These should be on a piece of window or plate glass, and over the whole should be placed a bell-jar, or other cover, to prevent evaporation. After an hour or more, the glass on which the slides rest should be lifted up to see whether the protoplasm has crawled out upon any of the slides. If any of the slides are satisfactory, lift off the bell-jar and remove the pieces of wood from the slide. The

Plasmodium will remain. The slide should then be put very gently into a mixture of equal parts of a saturated aqueous solution of picric acid and 95 per cent. alcohol; it should be removed in 15 or 20 minutes, and placed, for about the same length of time, in 95 per cent. alcohol; it may then be mounted in Canada balsam in the usual way, but without previous clearing.

The picric acid stiffens the protoplasm almost instantly, but does not shrink it, the alcohol removes the water and allows of Canada balsam mounting.

The above method is especially good for the yellow Plasmodium, as the color is precisely that of the picric acid solution. If white Plasmodium is to be mounted it should be soaked in 25 per cent. alcohol to remove the yellow color of the picric acid, before anhydrating it with strong alcohol.

Experiments have not been tried with Plasmodium of purple and other colors to determine successful methods of preservation, but some slight modification of the above is confidently expected to succeed.

SUMMARY. A.—The Plasmodium will crawl from rotten wood and extend itself on a moistened glass surface.

B.—The extended Plasmodium may be fixed in position by immersing the slide on which it is extended in a solution of picric acid.

C.—The slide may be placed in 95 per cent. alcohol to anhydrate the Plasmodium, after which it may be mounted in Canada balsam.

D.—The yellow Plasmodium retains its natural color if treated in this way.

PERMANENT MICROSCOPIC PREPARATIONS OF AMPHIBIAN BLOOD CORPUSCLES. By SIMON H. GAGE, of Ithaca, N. Y.

THE very excellent method of drying the corpuscles of mammalian blood, on the microscopic slide, is not applicable to the much more bulky corpuscles of Amphibia. The corpuscles of the latter are sure to be distorted and seamed in drying; hence various methods of preserving the corpuscles moist have been tried with varying success.

The following very great modification of the method proposed by Ranvier, in his treatise on histology,[1] has been in use for some time in the Anatomical Laboratory of Cornell University, and has given uniformly excellent results. Preparations made three years ago are quite as good as at first.

Three or four drops of fresh blood are allowed to fall into 10 cc. of normal salt solution (common salt 750 milligrammes, water 100 cc.), preferably contained in a high narrow vessel like a graduate glass or beaker. The mixture of blood and salt solution should be well agitated and then 100 cc. of a saturated aqueous solution of picric acid added with constant stirring. After the corpuscles have settled, as much of the supernatant liquid as possible is poured off, and in its place is put about an equal volume of normal salt solution. The corpuscles are allowed to settle, the liquid poured off and another volume of salt solution added. This is continued until the salt solution acquires only a faint yellow tinge.

The use of the salt solution is, first, to dilute the blood in order to avoid distortion of the corpuscles, and second, to wash away the picric acid so that the subsequent staining will be more satisfactory.

After pouring off the last salt solution, there is put in its place 10 cc. of a mixture of five parts of Frey's carmine and ninety-five parts of picrocarmine. The corpuscles will stain in from one to fifteen hours. A drop of the agitated mixture should be examined occasionally to ascertain when the staining is sufficient. The nucleus should be deep red, and the body of the corpuscle yellow or pinkish.

When the staining is completed, as much stainer as possible should be poured off, and replaced by 10 or 15 cc. of acid glycerine (glycerine 100 cc., acetic or formic acid 1 cc.). This mixture of corpuscles and glycerine may be placed in a bottle and used at any time, it being simply necessary to agitate the mixture slightly or to take up some of the sediment with a pipette and mount it precisely as any other glycerine preparation.

Summary.—1. The fresh blood is first diluted with about fifty times its volume of normal salt solution.

2. To this diluted blood is added ten times as great a volume of a saturated aqueous solution of picric acid.

[1] Traité technique de Histologie, p. 195.

3. The picric acid is washed away with normal salt solution.

4. The corpuscles are stained with picrocarmine, or a mixture of this and Frey's carmine.

5. They are preserved in acid glycerine, and may be mounted for the microscope at any time.

Method of Preparing and Mounting Wings of Microlepidoptera. By C. H. Fernald, of Orono, Me.

For a long time I have been seeking some method, by means of which, the wings of the microlepidoptera could be prepared, so that the venation could be studied under the compound microscope, in a manner that would leave no doubt of the presence, or absence, of the faintest vein in the whole wing-structure.

The removal of the scales by mechanical means, as resorted to by some, was to me quite unsatisfactory, and, in some cases, even impracticable. I therefore tried some of the methods recommended for bleaching the wings of small moths. That described by Chambers, in the Canadian Entomologist, was not a success in all cases, but whether not properly tried, I cannot say.

I next tried the method for bleaching the wings, published by Dimmock, and while this seemed to be a success so far as the bleaching was concerned, the final mounting did not always give satisfactory results; for when mounted dry, the scales, although bleached, were not sufficiently transparent to show clearly the more obscure parts of the structure, and when mounted in Canada balsam, the entire wing was rendered so transparent that only the larger veins were visible, and I found it extremely difficult to get rid of the air bubbles, which so readily gathered under the concave portions of certain minute wings.

I next tried mounting in cold glycerine. After having been bleached by Dimmock's method, the wings were transferred to the slide direct from the water in which they were washed, then allowed to dry, which was sometimes hastened by holding the slide over the flame of a lamp. When quite dry, a drop of glycerine was added and the cover at once put on, and where the

glycerine penetrated around the edges so as completely to saturate portions of the wing, the scales at once became transparent and the structure was clearly apparent.

The difficulty still remained to replace the air under the concave portions of the wing with the glycerine, so that it could come to, and completely saturate, every portion of the wing, to the exclusion of all the air. Recourse was now had to heat, and by holding the slide over the lamp till ebullition took place, the glycerine was found to replace the air without any injury to the wing structure, and even in those refractory cases where the glycerine was allowed to boil for a considerable length of time, no injury whatever was done to the wing-membrane.

The Microscopical Investigations of the Havana Yellow Fever Commission.

By Geo. M. Sternberg,[1] of Washington, D. C.

The purpose of the present paper is to give a general idea of the nature and objects of the investigations in which the writer has been engaged during the past year, under the auspices of the National Board of Health, and, more especially, to bring to notice the results obtained in photographing minute organisms, and to call attention to the great value of this method in conducting similar investigations.

A photographic memorandum of an object which we have seen under the microscope, while it may not be so sharply defined as a drawing, has the advantage of being quickly made, and is an unimpeachable record of what has been seen. A record, it is true, which conveys but little meaning to the untrained eye, just as a dingy painting, by one of the old masters, fails to tell its story to the eye of an untutored savage; but which, nevertheless, is full of meaning and of interest for the skilled microscopist. The photograph also saves great time and labor in the measurement of objects, and makes it easy to compare objects seen at different times and places by different observers. It may also be duplicated indefinitely; and when everything is working well, fifteen or

[1] Surg. U. S. A., Secretary of the Commission.

twenty negatives may be made, with the assistance of a skilled photographer, in the time which would be required by an accomplished draughtsman to make a single copy of any one of them. Many of my photographs have been made from extemporaneous preparations, in which a delay of half an hour or less, for the purpose of making drawings and measurements, would have resulted in the destruction of the specimen by drying of the liquid in which it was immersed.

All microscopists in the habit of working with high powers know how many accidents may happen to interesting extemporaneous preparations, by which the object, which has been found after a protracted search, is lost from view before the drawings and measurements desired have been made. This is obviated to a great extent by the promptness with which a photograph may be made, and with low powers we may even take instantaneous views of objects in motion. I do not claim, however, that all is plain and easy sailing and that the art of making photo-micrographs of minute objects can be acquired without effort. On the contrary, there are many difficulties with which to contend, and the chances are that the beginner will make many poor pictures and will have his temper and patience sorely tried before he attains any considerable degree of success. The labors of my friend, Surg. J. J. Woodward of the Army, have done much towards reducing these difficulties and establishing a satisfactory technique for this art, and I take pleasure in embracing the present opportunity to acknowledge my own indebtedness to him for my first lessons in photo-micrography.

As I intend to project some of these photo-micrographs for your inspection and to make a few remarks upon each, while it is displayed before you on the screen, I will not, at present, dwell upon this portion of my subject, but will proceed briefly to review the objects and results of the microscopical investigations in which I have been engaged.

The Havana Yellow Fever Commission received the following instructions from the National Board of Health.

"*First.* To ascertain the actual sanitary condition of the principal ports of Cuba from which shipments are made to the United States." * * *

"*Second.* To increase existing knowledge as to the pathology of yellow fever." * * *

"*Third.* To obtain as much information as possible with regard to the so-called endemicity of yellow fever in Cuba, and the conditions which may be supposed to determine this endemicity." * * *

"The three points above referred to are believed to be those which will most certainly yield results to scientific investigation; and which, therefore, should receive the special attention of the Commission."

"But, in addition to these, the National Board desires that the Commission shall consider certain problems relating to this disease; problems which may be entirely insoluble, but which nevertheless, are of such importance that an effort should be made to decide whether the National Board of Health will be justified in undertaking the labor and expense which will probably be required to obtain anything like a complete solution of them, if such solution is at all possible. *These problems relate to the nature and natural history of the cause of yellow fever.*" * * *

In the division of labor made by the members of the Commission, the duty of conducting the investigations under the first and third of the above headings was assigned to the chairman of the Commission, Dr. S. E. Chaillé; and that of conducting the investigations under the second heading to Dr. J. Guiteras; while my attention was chiefly given to investigations contemplated in the additional instructions:

This, then, was the task set before me: to determine the nature of the yellow fever poison; to answer the question which is uppermost in our minds whenever this disease is mentioned, and which has heretofore baffled all research. Is it an emanation from the human body? Is it a gas generated external to the body? Is it a bioplast? Is it a germ?

I might well have hesitated before undertaking this unpromising investigation, if the language of our instructions had implied that the National Board considered this an easy task, and one which they expected would be accomplished during our brief stay in Havana. But you will observe that no such result was anticipated, that the difficulty of the undertaking was fully appreciated; and that the work of the Havana Commission in this direction was looked upon as pioneer work, "to decide whether the National Board will be justified in undertaking the labor and expense which will probably be required to obtain anything like a complete solution of them, if such solution is at all possible."

Now, I have great faith that science will eventually solve these problems, but had little expectation that my feeble efforts for three months in Havana, would do more than to clear away some of the difficulties and make the solution easier for future investigators. I therefore had no hesitation in undertaking to do my best to fulfil the expectations of the National Board, without troubling myself as to whether a failure to find the germ would be considered a serious neglect of duty by the expectant public, or would be seized upon as a proof that there is no germ, by the wise men who, having, without any investigation at all, pronounced positively against its existence, are naturally anxious to have a chance to say, "I told you so." The truth is that we are yet a long way from settling this question. But I think I may safely say as the result of my investigations in Havana, *that there is no gross and conspicuous germ or organism, either in the blood of yellow fever patients or in the air of infected localities*, which, by its peculiar appearance or abundant presence, might arrest the attention of a microscopist and cause suspicion that it is the veritable germ of yellow fever. By gross and conspicuous germ, I mean an organism, the morphological peculiarities of which are readily made out with a first class modern objective, such as the Zeiss 1–18, which I used in Havana. The *Bacillus anthracis* of splenic fever; the *Spirillum Obermeieri* of relapsing fever; and the bacillus of pig typhoid, or infectious pneumo-enteritis, all come under this head. Yellow fever, so far as the germ theory is concerned, may therefore be classed with cholera, typhoid fever and the malarial fevers, which, by a process of reasoning, similar to that commonly used for yellow fever, are very generally believed to be germ diseases, but in which no organism has yet been demonstrated.

It is true that the recent investigations of Klebs and Tomassi, in the vicinity of Rome, have resulted in the discovery of what they believe to be the veritable germ of malarial fever; but these observations require verification, and I may say here that I am instructed by the National Board to repeat their experiments, and to seek their *Bacillus malariæ* in the vicinity of the swamps about New Orleans. I have already made some preliminary observations in this direction, but thus far without any positive results.

Both for yellow fever and for the malarial fevers, the microscopic examination of infected atmospheres would seem to offer the best

promise of success in the discovery of the organism, if there is an organism, which causes these diseases. We may suppose that in non-contagious diseases this organism is itself destroyed in producing its noxious effects and does not multiply in the human body; or that it exists in the blood or other fluids, in such small numbers as to escape attention, or that it accumulates in certain undiscovered foci, in the tissues; or that it is too minute to be discovered by the highest powers of the microscope. All of these hypotheses must be disproved before the germ theory, which, at present, so far as these diseases are concerned, is based upon a deductive process of reasoning and not upon direct experimental proof, can be supposed to have fallen before the attacks of science. On the other hand, in the efforts of science to discover the hypothetical germs of these diseases, it would seem that a study of infected atmospheres should give the best results; as we have here, beyond doubt, an abundance of these subtle poisons, which as yet are known to us only by their effects.

The *particulate* nature of these poisons which is predicated, especially for yellow fever, upon various facts relating to portability, preservation in goods, clothing, etc., makes it seem especially the province of the microscopist to seek for them. It is to be hoped, however, that the resources of chemistry will also be brought to bear upon the problem.

As already stated, a pretty thorough search in Havana has failed to disclose any organism peculiar to infected atmospheres, which may be supposed to be the germ of yellow fever. But the difficulties of such an investigation, the brief stay of the Commission in Cuba, the innumerable harmless organisms which abound everywhere in the atmosphere and which must be excluded, one by one, by the novice in this field of inquiry, and, finally, the imperfection of our optical apparatus, all combine to make this negative result of little value, and it is only after a protracted search by specially trained experts that a negative result can be supposed to carry any great weight. Even then, we shall only have disproved the presence of a germ visible with the optical apparatus now available, and the microscope of the future may show our successors that which at present is ultramicroscopic. We have positive evidence of the constant presence of ultra-microscopic germs in the atmosphere in the development of a multitude of micro-organisms in organic liquids, exposed for a short time to the air.

For these organisms, although quite within reach of our modern objectives, are not found in the atmosphere by the most careful search, except, occasionally in the immediate vicinity of sewers, etc., and, as evidence of spontaneous generation is wanting, we are obliged to assume that they are developed from germs which escape our researches.

While I have nothing but negative results to bring before you, so far as the main object of my investigations is concerned, I have been enabled through the liberal policy of the National Board of Health to study and photograph many of the common organisms found in the atmosphere of New Orleans during the absence of any epidemic disease. As I consider a thorough knowledge of these every-day organisms an essential prerequisite to an enlightened study of an infected atmosphere, I have no hesitation in bringing before you the results of my labors. This I can best do by projecting my photo-micrographs for your inspection, and for this purpose will here terminate the written portion of my address.

TITLES OF OTHER PAPERS READ IN THE PERMANENT SUBSECTION OF MICROSCOPY.

CONTRIBUTION TO THE HISTOLOGICAL NATURE OF THE MEMBRANE IN CROUP. By Ephraim Cutter, of Boston, Mass.

ABUNDANCE OF MICROSCOPIC FORMS OF LIFE IN THE CENTRAL AND LATERAL SURFACES OF LAKES AND PONDS. By Ephraim Cutter, of Boston, Mass.

ON THE LIMITS OF VISIBILITY WITH THE MICROSCOPE. By A. E. Dolbear, of College Hill, Mass.

A NEW FREEZING MICROTOME. By Wm. Hailes, of Albany, N. Y.

A METHOD OF MOUNTING IN GLYCERINE AND CERTAIN DIFFERENTIATIONS OF STRUCTURE PRODUCED BY IT ON INSECTS. By W. H. Seaman, of Washington, D. C.

MINUTE ANATOMY OF THE HUMAN LARYNX. By Carl Seiler, of Philadelphia, Pa.

SOME OF THE INFUSORIA FOUND IN FRESH POND, CAMBRIDGE. By S. P. Sharples, of Boston, Mass.

APPARATUS USED IN PHOTOGRAPHING MICROSCOPICAL OBJECTS. By Samuel Wells, of Boston, Mass.

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ADDRESS

BY

ALEXANDER AGASSIZ,

VICE-PRESIDENT, SECTION B.

PALEONTOLOGICAL AND EMBRYOLOGICAL DEVELOPMENT.

MEMBERS OF THE ASSOCIATION: —

SINCE the publication of the "Poissons Fossiles" by Agassiz and of the "Embryologie des Salmonidées" by Vogt, the similarity, traced by the former between certain stages in the growth of young fishes and the fossil representatives of extinct members of the group, has also been observed in nearly every class of the animal kingdom, and the fact has become a most convenient axiom in the study of paleontological and embryological development. This parallelism, which has been on the one side a strong argument in favor of design in the plan of creation, is now, with slight emendations, doing duty on the other as a newly discovered article of faith in the new biology.

But while in a general way we accept the truth of the proposition that there is a remarkable parallelism between the embryonic development of a group and its paleontological history, yet no one has attempted to demonstrate this, or rather to show how far the parallelism extends. We have up to the present time been satisfied with tracing the general coincidence, or with striking individual cases.

The resemblance between the pupa stage of some Insects and of adult Crustacea, the earlier existence of the latter, and the subse-

quent appearance of the former in paleontological history, furnished one of the first and most natural illustrations of this parallelism; while theoretically the necessary development of the higher tracheate insects from their early branchiate aquatic ancestors seemed to form an additional link in the chain, and point to the Worms, the representatives of the larval condition of Insects, as a still earlier embryonic stage of the Articulates.

Indeed, there is not a single group of the animal kingdom in which embryology has not played a most important part in demonstrating affinities little suspected before. The development of our frogs, our salamanders, has given us the key to much that was unexplained in the history of Reptiles and Batrachians. The little that has been done in the embryology of Birds has revolutionized our ideas of a class which at the beginning of the century seemed to be the most naturally circumscribed of all. Embryology and paleontology combined have led to the recognition of a natural classification uniting Birds and Reptiles on the one side and Batrachians and Fishes on the other. It is to embryology that we owe the explanation of the affinities of the old Fishes in which Agassiz first recognized the similarity to the embryo of Fishes now living, and by its aid we may hope to understand the relationship of the oldest representatives of the class. It has given us the only explanation of the early appearance of the Cartilaginous Fishes, and of the probable formation of the earliest vertebrate limb from the lateral embryonic fold, still to be traced in the young of the Osseous Fishes of to-day.

Embryology has helped us to understand the changes aquatic animals must gradually undergo in order to become capable of living upon dry land. It has given us pictures of swimming-bladders existing as rudimentary lungs in Fishes with a branchial system; in Batrachians it has shown us the persistence of a branchial system side by side with a veritable lung. We find among the earliest terrestrial Vertebrates, types having manifest affinities with the Fishes on one side and Batrachians on the other, and we call these types Reptiles; but we should nevertheless do so with a reservation, looking to embryology for the true meaning of these half-fledged Reptiles, which lived at the period of transition between an aquatic and a terrestrial life, and must therefore always retain an unusual importance in the study of the development of animal life.

When we come to the embryology of the marine Invertebrates,

the history of the development of the barnacles is too familiar to be dwelt upon, and I need only allude to the well-known transformations of the Echinoderms, of the Acalephs, Polyps, in fact of every single class of Invertebrates, and perhaps in none more than in the Brachiopods, to show how far-reaching has been the influence of embryology in guiding us to a correct reading of the relations between the fossils of successive formations. There is scarcely an embryological monograph now published dealing with any of the later stages of growth which does not speak of their resemblance to some type of the group long ago extinct. It has therefore been most natural to combine with the attempts constantly made to establish the genetic sequence between the genera of successive formations, an effort to establish also a correspondence between their paleontological sequence and that of the embryonic stages of development of the same, thus extending the mere similarity first observed between certain stages to a far broader generalization.

It would carry me too far to sketch out, except in a most general way, even for a single class, the agreement known to exist in certain groups between their embryonic development and their paleontological history. It is hinted at in the succession of animal life of any period we may take up, and perhaps cannot be better expressed than by comparing the fauna of any period as a whole with that of following epochs; — a zoölogical system of the Jura, for instance, compared with one made up for the Cretaceous; next, one for the Tertiary, compared with the fauna of the present day. In no case could we find any class of the animal kingdom bearing the same definitions or characterized in the same manner. But apply to this comparison the data obtained from the embryological development of our present fauna, and what a flood of light is thrown upon the meaning of the succession of these apparently disconnected animal kingdoms, belonging to different geological periods, especially in connection with the study of the few ancient types which have survived to the present day from the earliest times in the history of our earth!

Although there is hardly a class of the animal kingdom in which some most interesting parallelism could not be drawn, and while the material for an examination of this parallelism is partially available for the Fishes, Mollusks, Crustacea, Corals, and Crinoids, yet for the illustration and critical examination of this parallel-

ism I have been led to choose to-day a very limited group, that of Sea-urchins, both on account of the nature of the material and of my own familiarity with their development and with the living and extinct species of Echini. The number of living species is not very great, — less than three hundred, — and the number of fossil species thus far known is not, according to Zittel, more than about two thousand. It is therefore possible for a specialist to know of his own knowledge the greater part of the species of the group. It has been my good fortune to examine all but a few of the species now known to exist, and the collections to which I have had access contain representatives of the majority of the fossil species. Sea-urchins are found in the oldest fossiliferous rocks; they have continued to exist without interruption in all the strata up to the present time. While it is true that our knowledge of the Sea-urchins occurring before the Jurassic period is not very satisfactory, it is yet complete enough for the purposes of the present essay, as it will enable me, starting from the Jurassic period, to call your attention to the paleontological history of the group, and to compare the succession of its members with the embryological development of the types now living in our seas. Ample material for making this comparison is fortunately at hand; it is material of a peculiar kind, not easily obtained, and which thus far has not greatly attracted the attention of zoölogists.

Interesting and important as are the earliest stages of embryonic development in the different classes of the animal kingdom, as bearing upon the history of the first appearance of any organ and its subsequent modifications, they throw but little light on the subject before us. What we need for our comparisons are the various stages of growth through which the young Sea-urchins of different families pass from the time they have practically become Sea-urchins until they have attained the stage which we now dignify with the name of species. Few embryologists have carried their investigations into the more extended field of the changes the embryo undergoes when it begins to be recognized as belonging to a special class, and when the knowledge of the specialist is absolutely needed to trace the bearing of the changes undergone, and to understand their full meaning. Fortunately the growth of the young Echini has been traced in a sufficient number of families to enable me to draw the parallelism between these various stages of growth and the paleontological stages in a very different man-

ner from what is possible in other groups of the animal kingdom, where we are overwhelmed with the number of species, as in the Insects or Mollusks, or where the paleontological or the embryological terms of comparison are wanting or very imperfect.

Beginning with the paleontological history of the regular Sea-urchins at the time of the Trias, when they constituted an unimportant group as compared with the Crinoids, we find the Echini of that time limited to representatives of two families. One of these, the genus Cidaris, has continued to exist, with slight modifications, up to the present time, and not less than one tenth of all the known species of fossil Echini belong to this important genus, which in our tropical seas is still a prominent one. It is interesting here to note that in the Cidaridæ the modifications of the test are not striking, and the fossil genera appearing in the successive formations are distinguished by characters which often leave us in doubt as to the genus to which many species should be referred. In the genus Rhabdocidaris, which appears in the lower Jura, and which is mainly characterized by the extraordinary development of the radioles, we find the extreme of the variations of the spines in this family. From that time to the present day, the most striking differences have existed in the shape of the spines, not only of closely allied genera, but even in specimens of the same species; differences which in some of the species of to-day are as great as in older geological periods. The oldest Cidaridæ are remarkable for their narrow poriferous zones. It is only in the Jura that they widen somewhat; subsequently the pores become conjugated, and only later, during the Cretaceous period, do we find the first traces of any ornamentation of the test (Temnocidaris) so marked at the present day in the genus Goniocidaris. As far, then, as the Cidaridæ are concerned, the modifications which take place from their earliest appearance are restricted to slight changes in the poriferous zone and in the ornamentation of the test, accompanied with great variability in the shape of the primary radioles. We must except from this statement the genera Diplocidaris and Tetracidaris, to which I shall refer again. The representatives of the other Triassic family become extinct in the lower tertiaries. The oldest genus, Hemicidaris, undoubtedly represents the earliest deviations from the true Cidaris type; modifications which affect not only the poriferous zone, but the test, the actinal and the abactinal systems, while from the extent of these minor changes we can trace out the

gradual development of some of the characteristics in families of the regular Echini now living. The genus Hemicidaris may be considered as a Cidaris in which the poriferous zone is narrow and undulating, in which the granules of the ambulacral system have become minute tubercles in the upper portion of the zone and small primary tubercles in its actinal region, in which many of the interambulacral granules become small secondaries, in which the plates of the actinal system have become reduced in number, and the apical system has become a narrow ring, and finally in which the primary radioles no longer assume the fantastic shapes so common among the Cidaridæ.

We can trace in this genus the origin of the modifications of the poriferous zone, leading us, on the one side, through genera with merely undulating lines of pores to more or less distinct confluent arcs of pores, formed round the primary ambulacral tubercles, and, on the other, to the formation of open arcs of three or more pairs of pores. The first type culminates at the present day with the Arbaciadæ, the other with the Diadematidæ, Triplechinidæ, and Echinometradæ. This specialization very early takes place, for already in the lower Jura Stomechinus has assumed the principal characteristics of the Triplechinidæ of to-day.

Although in Hemicidaris the number of the coronal plates has increased as compared with the Cidaridæ, and while we find that in many genera, even of those of the present day, the number of the coronal plates is still comparatively small, yet, as a general rule, the more recent formations contain genera in which the increase in number of the interambulacral plates is accompanied by a corresponding decrease in the number of plates of the ambulacral area so characteristic thus far of the Cidaridæ and Hemicidaridæ, a change also affecting the size of the primary ambulacral tubercles. This increase in the number of the coronal plates is likewise accompanied by the development of irregular secondary and miliary tubercles, and the disappearance in this group of the granular tuberculation, so important a character in the Cidaridæ. With the increase in the number of the interambulacral coronal plates, the Pseudodiadematidæ still retain prominent primary tubercles, recalling the earlier Hemicidaridæ and Cidaridæ, and, as in the Cidaridæ proper, the test is frequently ornamented by deep pits or by ridges formed by the junction of adjoining tubercles. The genital ring becomes narrower, and the tendency to the special-

ization of one of its plates, the madreporite, more and more marked.

With the appearance of Stomechinus, the Echinidæ proper already assume in the Jura the open arcs of pores, the large number of coronal interambulacral plates, the specialization of the secondary tubercles, and the large number of primary tubercles in each plate. With the appearance of Sphærechinus in the early Tertiary come in all the elements for the greater multiplication of the pairs of pores in the arcs of the poriferous zones, while the gigantic primary spines of some of the genera (Heterocentrotus), and the small number of primary tubercles are structural features which had completely disappeared in the group preceding the Echinometradæ, to which they appear most closely allied.

Going back again to the Hemicidaridæ, it requires but slight changes to pass from them to Acrosalenia and to the Saleniæ proper; the latter have continued to the present day, and have, like the Cidaridæ, retained almost unchanged the characters of the genera which preceded them, combined, however, with a few Cidaridian and Echinid features which date back to the Triassic period. We can thus trace the modifications which have taken place in the poriferous zone, the apical and actinal systems, the coronal plates, the ambulacral and interambulacral tubercles, as well as in the radioles, and in the most direct manner possible indicate the origin of the peculiar combination of structural features which we find at any geological horizon. On taking in succession the modifications undergone by the different parts of the test, we can trace each one singly, without the endless complication of combinations which any attempt to trace the whole of any special generic combination would imply.

Leaving out of the question for the moment the Palæchinidæ, we find no difficulty in tracing the history of the characters of the genera of the regular Echini which have existed from the time of the Trias and are now living, provided we take up each character independently. Nothing can be more direct than the gradual modification of the simple, barely undulating poriferous zone, made up of numerous ambulacral plates covered by granules, such as we find it among the Cidaridæ of the Trias, first into the slightly undulating poriferous zone of the Hemicidaridæ, next into the indistinct arcs of pores of the Pseudodiadematidæ, then into the arcs with a limited number of pores of the Triplechinidæ, and finally to the

polyporous arcs of the Echinometradæ. What can be more direct than the gradual modification to be traced in the development of the primary ambulacral tubercles, such as are characteristic of the Echinidæ of the present day, from their first appearance at the oral extremity of the ambulacral system of the Hemicidaridæ, and the increase in the number of primary interambulacral tubercles, accompanied by the growth of secondaries and miliaries, which we can trace in Hemicidaris, Acrosalenia, and Stomechinus, — the increase in number of the primary and secondary tubercles being accompanied by a reduction in the size of the radioles and a greater uniformity in their size and shape?

But while these modifications take place, the original structural feature may be retained in an allied group. Thus the Cidaridæ retain unchanged from the earliest time to the present day the few primary tubercles, the secondary granules, the simple poriferous zone, the imbricating actinal system, and the few coronal plates, with the large apical system and many-shaped radioles; while in the Salenidæ the primary interambulacral tubercles, the secondary granules, the radioles, the genital ring, are recognized features of the Cidaridæ, associated, however, with an Echinid actinal and anal system, Hemicidarid primary ambulacral tubercles, and an Echinid poriferous zone. In the same way in the Diadematidæ, the large primary interambulacral tubercles are Cidaridian features, while the structure of the ambulacral tubercles is Hemicidaridian. The existence of two kinds of spines is another Cidaridian feature, while the apical and actinal systems have become modified in the same direction as that of the Echinidæ. The more recent the genus, the greater is the difficulty of tracing in a direct manner the origin of any one structural feature, owing to the difficulty of disassociating structural elements characteristic of genera which may be derived from totally different sources. This is particularly the case with genera having a great geological age. Many of them, especially among the Spatangoids, show affinities with genera following them in time, to be explained at present only on the supposition that, when a structural feature has once made its appearance, it may reappear subsequently, apparently as a new creation, while in reality it is only its peculiar combination with structural features with which it had not before been associated (a new genus), which conceals in that instance the fact of its previous existence. A careful analysis, not only of the genera of the order, but sometimes of other

orders which have preceded this combination in time, may often reveal the elements from which have been produced apparently unintelligible modifications.

There is, however, not one of the simple structural features in the few types of the Triassic and Liassic Echini from which we can so easily trace the origin of the structural features of all the subsequent Echinid genera, which is not also itself continued to the present day in some generic type of the present epoch, fully as well characterized as it was at the beginning. In fact, the very existence to-day of these early structural features seem to be as positive a proof of the unbroken systematic affinity between the Echini of our seas and those of the Trias, as the uninterrupted existence of the genus Pygaster or Cidaris from the Lias down to the present epoch, or of the connection of many of the genera of the Chalk with those of our epoch (Salenia, Cyphosoma, Psammechinus, etc.).

Passing to the Clypeastridæ, we find there as among the Desmosticha that the earliest type, Pygaster, has existed from the Trias to the present time; and that, while we can readily reconstruct, on embryological grounds, the modifications the earliest Desmosticha-like Echini should undergo in order to assume the structural features of Pygaster, yet the early periods in which the precursors of the Echinoconidæ and Clypeastridæ are found have thus far not produced the genera in which these modifications actually take place. But, starting from Pygaster, we naturally pass to Holectypus, to Discoidea, to Conoclypus, on the one side, while on the other, from Holectypus to Echinocyamus, Sismondia, Fibularia, and Mortonia, we have the natural sequence of the characters of the existing Echinanthidæ, Laganidæ, and Scutellidæ, the greater number of which are characteristic of the present epoch. If we were to take in turn the changes undergone in the arrangement of the plates of the test, as we pass from Pygaster to Holectypus, to Echinocyamus, and the Echinanthidæ, we should have in the genera which follow each other in the paleontological record an unbroken series showing exactly what these modifications have been. In the same way, the modifications of the abactinal and anal systems, and those of the poriferous zone, can equally well be followed to Echinocyamus, and thence to the Clypeastridæ; while a similar sequence in the modifications of these structural features can be followed from Mortonia to the Scutellidæ of the present period.

Passing finally to the Petalosticha, we find no difficulty in tracing theoretically the modifications which our early Echinoconidæ of the Lias should primarily undergo previous to the appearance of Galeropygus. The similarity of the early Cassiduloid and Echinoneoid types points to the same systematic affinity, and perhaps even to a direct and not very distant relationship with the Palæchinidæ. For if we analyze the Echinothuriæ of the present day, we find in genera like Phormosoma many structural features, such as the shape of the test, the character of the spines, the structure of the apical system, that of the poriferous zone, indicative of possible modifications in the direction of Pygaster or of Galeropygus, which have as yet not been taken into account.

Adopting for the Petalosticha the same method of tracing the modifications of single structural features in their paleontological succession, we trace the comparatively little modified paleontological history of the Echinoneidæ of the present day from the Pyrina of the lower Jura. This, in its turn, has been preceded by Hyboclypus and Galeropygus, while the Echinolampadæ of the present day date back, with but trifling modifications, to the Echinobrissus of the Jura, itself preceded by Clypeus; and they have been subject only to slight generic changes since that time, Echinobrissus being still extant, while such closely allied genera as Catopygus and Cassidulus of the earlier Cretaceous are still represented at the present day; the modifications taking place in the actinal system, in the ambulacal zones of the Echinoconidæ and of the Echinolampadæ showing the closest possible systematic affinity in these families. Starting again from Hyboclypus, with its elongate apical system, we naturally pass to Collyrites and the strange Dysasteridæ forms which in their turn are closely allied to the Holasteridæ. From Holaster on the one side and from Toxaster on the other, we find an unbroken sequence of structural characters uniting the successive genera of Holasteridæ, such as Cardiaster, Offaster, Stenonia, Ananchytes, and Asterostoma, with Palæopneustes, Homolampas and the Pourtalesiæ of the present day, while from the genera of the Toxasteridæ we naturally pass to the cretaceous Hemiaster; in this genus and the subsequent Micraster we find all the elements necessary for the modifications which appear in the Spatanginæ from the time of the Chalk to the present day. These modifications result in genera in which we trace the development of the fascioles, of the actinal, anal, and abactinal plastrons, of the

beak, the formation of the petaloid ambulacra, first flush with the test, and little by little changed into marsupial pouches, the growth of the anterior groove and the manifold modifications of the ambulacral system in Spatangus, Agassizia, and Echinocardium, often recalling in some of its features structural characters of families which have preceded this in time.

Apparently in striking contrast with the Echini of the secondary period and those which have succeeded them stand the Paleozoic Echini; but when we have examined the embryology of Echini, we shall be better prepared to understand their structure and the affinities of the Palæchinidæ with the Echini of the present day and their immediate predecessors.

Taking up now the embryological development of the several families which will form the basis of our comparisons, beginning with the Cidaridæ, we find that in the earliest stages they very soon assume the characters of the adult, the changes being limited to the development of the abactinal system, the increase in number of the coronal plates, and the modifications of the proportionally gigantic primary radioles.

In the Diadematidæ the changes undergone by the young are limited to the gradual transformation of the embryonic spines into those which characterize the family, to the changes of the vertical row of pores in the ambulacral area into arcs of three or four pairs of pores, and to the specialization of the actinal and abactinal systems.

In the Arbaciadæ the young stages are remarkable for the prominent sculpture of the test, for the flattened spines, for their simple poriferous zone, for their actinal system, and for their genital ring. The anal plates appear before the genital ring.

In the Echinometradæ the young thus far observed are characterized by the small number of their primary tubercles, the large size of the spines, the simple vertical row of pores, the closing of the anal ring by a single plate, and the turban-shaped outline of the test. Little by little, the test loses with increasing age this Cidaris-like character; it reminds us, from the increase in the number of its plates, more of Hemicidaris; then, with their still greater increase, of the Pseudodiadematidæ; and, finally, of the Echinometradæ proper. The spines, following *pari passu* the changes of the test, lose little by little their fantastic embryonic, or rather Cidaris-like appearance, and become more solid and shorter, till they finally

assume the delicately fluted structure characteristic of the Echinometradæ. The vertical poriferous zone is first changed into a series of connected vertical arcs, which become disjointed, and form, with increasing age, the independent arcs of pores, composed of three or more pairs of pores, of the Echinometradæ.

In the Echinidæ proper we find in the young stages the same unbroken vertical line of pores, which gradually becomes changed to the characteristic generic types. We find, as in the Echinometradæ, an anal system closed with a single plate, and an abactinal system separating in somewhat more advanced stages from the coronal plates of the test. This is as yet made up of a comparatively small number of plates, carrying but few large primary tubercles, with fantastically shaped spines entirely out of proportion to the test, but which, little by little, with the increase of the number of coronal plates, the addition of primary tubercles, and their proportional decrease in size, assume more and more the structure of the genus to which the young belongs. The original anal plate is gradually lost sight of from the increase in number of the plates covering the anal system, and it is only among the Temnopleuridæ that this anal plate remains more or less prominent in the adult. In the Salenidæ, of which we know as yet nothing of the development, this embryonic plate remains permanently a prominent structural feature of the apical system.[1]

Among the Clypeastroids the changes of form they undergo during growth are most instructive. We have in the young Fibularinæ an ovoid test, a small number of coronal plates surmounted by few and large primary tubercles, supporting proportionally equally large primary radioles, simple rectilinear poriferous zones, no petaloid ambulacra, — in fact, scarcely one of the features we are accustomed to associate with the Clypeastroids is as yet prominently developed. But rapidly, with increasing size, the number of primary tubercles increases, the spines lose their disproportionate size, the pores of the abactinal region become crowded and elongate, and a rudimentary petal is formed. The test becomes more flattened, the coronal plates increase in number, and it would be impossible to recognize in the young Echinocyamus, for instance, the

[1] The young of the following genera have served as a basis for the preceding analysis of the embryonic stages of the Desmosticha: Cidaris, Dorocidaris, Goniocidaris, Arbacia, Porocidaris, Strongylocentrotus, Echinometra, Echinus, Toxopneustes, Hipponoë, Temnopleurus, Temnechinus, and Trigonocidaris.

adult of the Cidaris-like or Echinometra-like stages of the Sea-urchin, had we not traced them step by step. Most interesting, also, is it to follow the migrations of the anal system, which, to a certain extent, may be said to retain the embryonic features of the early stages of all Echinoderm embryos, in being placed in more or less close proximity to the actinostome. What has taken place in the growth of the young Echinocyamus is practically repeated for all the families of Clypeastroids; a young Echinarachnius, or Mellita, or Encope, or a Clypeaster proper, resembles at first more an Echinometra than a Clypeastroid; they all have simple poriferous zones and spines and tubercles out of all proportion to the size of the test.[2]

When we come to the development of the Cassiduloids, we find their younger stages also differing greatly from the adult. Among the Nucleolidæ, for instance, the young stages have as yet no petals, but only simple rectilinear poriferous zones. They are elliptical with a high test, with a single large primary tubercle for each plate, and a simple elliptical actinostome, without any trace of the typical bourrelets and phyllodes so characteristic of this family. Very early, however, this condition of things is changed, the test soon becomes more flattened, the petals begin to form as they do in the Clypeastroids, and we can soon trace the rudiments of the peculiar bourrelets characteristic of the family, accompanied by a rapid increase in the number of tubercles and in that of the coronal plates.

Among the Spatangidæ some are remarkable in their adult condition for their labiate actinostome, for the great development of the petals, for the presence of fascioles surrounding certain definite areas, for the small size of the tubercles, the general uniformity in the spines of the test, and the specialization of their anterior and posterior regions. On examining the young stages of this group of Spatangoids, not one of these structural features is as yet developed. The actinostome is simple, the poriferous zone has the same simple structure from the actinostome to the apex, the primary tubercles are large, few in number, surrounded by spines which would more readily pass as the spines of Cidaridæ than of Spatangoids. The fascioles are either very indistinctly indicated, or else the special lines have not as yet made

[2] Among the Clypeastroids I have examined the young of Echinocyamus, Fibularia, Mellita, Laganum, Echinarachnius, Encope, Clypeaster, and Echinanthus.

their appearance; the ambulacral suckers of the anterior zone are as large and prominent as those of the young stages of any of the regular Echini. It is only little by little, with advancing age, that we begin to see signs of the specialization of the anterior and posterior parts of the test, that we find the characteristic anal or lateral fascioles making their appearance, only with increasing size that the spines lose their Cidaris-like appearance, that the petals begin to be formed, and that the simple actinostome develops a prominent posterior lip. In the genus Hemiaster, the young stages are specially interesting, as long before the appearance of the petals, while the poriferous zone is still simple, the total separation of the bivium and of the trivium of the ambulacral system, so characteristic of the earliest Spatangoids (the Dysasteridæ), is very apparent.[8]

From this rapid sketch of the changes of growth in the principal families of the recent Echini we can now indicate the transformations of a more general character through which the groups as a whole pass.

In the first place, while still in the Pluteus all the young Echini are remarkable for the small number of coronal plates, and for the absence of any separation between the actinal and abactinal systems and the test proper. They all further agree in the large size of the primary spines of the test, whether it be the young of a Cidaris, an Arbacia, an Echinus, a Clypeaster, or a Spatangoid. They all in their youngest stages have simple vertical ambulacral zones; beyond this, we find as changes characteristic of some of the Desmosticha, the specialization of the actinal system from the coronal plates, the formation of an anal system, the rapid increase in the number of the coronal plates, with a corresponding increase in the number of the spines and a proportional reduction of their size, the formation of an abactinal ring, and the change of the simple vertical poriferous zone into one composed of independent arcs.

In the Spatangoids and Clypeastroids we find common to both groups the shifting of the anal system to its definite place, the modifications of the abactinal part of the simple ambulacral system in order to become petaloid, and the gradual change of the elliptical ovoid test of the young to the characteristic generic test, accom-

[8] For this sketch of the embryology of the Petalosticha I have examined the young of Echinolampas, Echinoneus, Echinocardium, Brissopsis, Agassizia, Spatangus, Brissus, and Hemiaster.

panied by the rapid increase in the number of the primary tubercles and spines. Finally limited to the Spatangoids are the changes they undergo in the transformation of the simple actinostome to a labiate one, the specialization of the anterior and posterior parts of the test, and the definite formation of the fascioles.

Comparing this embryonic development with the paleontological one, we find a remarkable similarity in both, and in a general way there seems to be a parallelism in the appearance of the fossil genera and the successive stages of the development of the Echini as we have traced it.

We find that the earlier regular Echini all have more or less a Cidaris-like look, — that is, they are Echini with few coronal plates, large primary tubercles, with radioles of a corresponding size; that it is only somewhat later that the Diademopsidæ make their appearance, which, in their turn, correspond within certain limits to the modifications we have traced in the growth of the young Diadematidæ and Arbaciadæ. The separation of the actinal system from the coronal plates has been effected. The poriferous zone has either become undulating, or forms somewhat indefinite open arcs; we find in all the genera of this group a larger number of coronal plates, more numerous primaries, the granules of the Cidaridæ replaced by secondaries and miliaries, and traces of a Hemicidaris-like stage in the size of the actinal ambulacral tubercles.

Comparing in the same way the paleontological development of the Echinidæ proper, we find that, on the whole, they agree well with the changes of growth we can still follow to-day in their representatives, and that, as we approach nearer the present epoch, the fossil genera more and more assume the structural features which we find developed last among the Echinidæ of the present day. Very much in the same manner as a young Echinus develops, they lose, little by little, first their Cidaridian affinities, which become more and more indefinite, next their Didematidian affinities, if I may so call the young stages to which they are most closely allied, and, finally, with the increase in the number of the coronal plates, the great numerical development of the primary tubercles and spines, and that of the secondaries and miliaries which we can trace in the fossil Echini of the Tertiaries, we pass insensibly into the generic types characteristic of the present day.

Although we know nothing of the embryology of the Salenidæ,

yet, like the Cidaridæ, they have in a great measure remained a persistent type, the modifications of the group being all in the same direction as those noticed in the other Desmosticha; a greater number of coronal plates, the development of secondaries and miliaries combined with a specialization of the actinal system not found in the Cidaridæ.

An examination of the succession of the Echinoconidæ shows but little modification from the earliest types; the changes, however, are similar to those undergone by the Clypeastroids and Petalosticha, though they do not extend to modifications of the poriferous zone, but are mainly changes in the actinostome and in the tuberculation. In fact, the group of Echinoconidæ seems to hold somewhat the same relation to the Clypeastroids which the Salenidæ hold to the Cidaridæ, and the earliest genus of the group (Pygaster) has remained, like Cidaris, a persistent type to the present day.

The earliest Clypeastroids are all forms which resemble the Fibularina and the genera following Echinocyamus and Fibularia; they are mainly characterized by the same changes which an Echinarachnius or a Mellita, for instance, undergoes as it passes from its Echinocyamus stage to the Laganum or Encope stage. The comparison is somewhat more complicated when we come to the Cassiduloids. The comparison of the succession of genera in the different families, as traced in the Desmosticha and Clypeastroids, is made difficult from the persistency of the types preceding the Echinoneidæ and the Echinolampadæ, which have remained without important modifications from the time of the lower Cretaceous; previous to that time the modifications of the Cassidulidæ are found to agree with the changes which have been observed in the growth of Echinolampas. The early genera, like Pygurus, have many of the characteristics of the test of the young Echinolampas. The development of prominent bourrelets and of the floscelle and petals goes on side by side with that of genera in which the modification of the actinostome, of the test, and of the petals is far less rapid, one group retaining the Echinoneus features, the other culminating in the Echinolampas of the present day, and having likewise a persistent type, Echinobrissus, which has remained with its main structural features unchanged from the Jura to the present day. That is, we find genera of the Cassidulidæ which recall the early Echinoneus stage of Echinolampas, next the Caratomus stage,

after which the floscelle, bourrelets, and petals of the group become more prominent features of the succeeding genera. Accompanying the persistent type Echinobrissus, genera appear in which either the bourrelets or petals have undergone modifications more extensive than those of the same parts in the genera of the Echinoneus or Caratomus type.

The earliest Spatangoids belong to the Dysasteridæ, apparently an aberrant group, but which, from the history of the young Hemiaster, we now know to be a strictly embryonic type, which, while it thus has affinities with the true Spatangoids, still retains features of the Cassidulidæ in the mode of development of the actinostome and of the petals, as well as of the anal system. The genera following this group, Holaster and Toxaster, can be well compared, the one to the young stages of Spatangus proper before the appearance of the petals, when the ambulacra are flush with the test, and when its test is more or less ovoid, the other to a somewhat more advanced stage, when the petals have made their appearance as semi-petals. In both cases the actinostome has the simple structure characteristic of all the young Spatangoids. The changes we notice in the genera which follow them lead in the one case through very slight modifications of the abactinal system, of the anterior and posterior extremities of the test, to the Ananchytid-like Spatangoids of the present day, the Pourtalesiæ, the genus Holaster itself persisting till well into the middle of the Tertiary period; while on the other side we readily recognize in the Spatanginæ which follow Toxaster (a persistent type which has continued as Palæostoma to the present day) the genera which correspond to the young stages of such Spatangoids as Spatangus and Brissopsis of the present day, genera which, on the one hand, lead from Hemiaster (itself still represented in the present epoch), through stages such as Cyclaster, Peripneustes, Brissus, and Schizaster, and, on the other, through Micraster and the like, to the Spatangoids, in which the development of the anal plastron and fasciole performs an important part, while in the former group the development of the peripetalous fasciole and of the lateral fasciole can be followed. None of the genera of Petalosticha belonging to the other groups develop any fasciole in the sense of circumscribing a limited area of the test.

The comparison of the genera of Echini which have appeared since the Lias with the young stages of growth of the principal

families of Echini, shows a most striking coincidence amounting almost to identity between the successive fossil genera and the various stages of growth. This identity can, however, not be traced exactly in the way in which it has usually been understood, while there undoubtedly exists in the genera which have appeared one after the other a gradual increase in certain families in the number of forms, and a constant approach in each succeeding formation, in the structure of the genera, to those of the present day. It is only in the accordance between some special points of structure of these genera and the young stages of the Echini of the present day that we can trace an agreement which, as we go further back in time, becomes more and more limited. We are either compelled to seek for the origin of many structural features in types of which we have no record, or else we must attempt to find them existing potentially in groups where we had as yet not succeeded in tracing them. The parallelism we have traced does not extend to the structure as a whole. What we find is the appearance among the fossil genera of certain structural features giving to the particular stages we are comparing their characteristic aspect. Thus, in the succession of the fossil genera, when a structural feature has once made its appearance, it may either remain as a persistent structure, or it may become gradually modified in the succeeding genera of the same family, or it may appear in another family, associated with other more marked structural features which completely overshadow it. Take, for instance, among the Desmosticha the modifications of the poriferous zone of the actinal and abactinal systems of the coronal plates, of the ambulacral and interambulacral systems, the changes in the relative proportion of the primary tubercles, and the development of the secondaries. These are all structural features which are modified independently one of the other; we may find simultaneous development of these features in parallel lines, but a very different degree of development of any special feature in separate families.

This is as plainly shown in the embryological as in the paleontological development. In the Cidaridæ there is the minimum of specialization in these structural features. In the Diademopsidæ there is a greater range in the diversity of the structure of the poriferous zone and of the coronal plates, as well as of the actinal system. There is a still greater range among the Echinidæ, while among the Salenidæ the modifications, as compared to those of the

Echinidæ and Diademopsidæ, are somewhat limited again, being restricted as far as relates to the poriferous zone and coronal plates, but specialized as far as the actinal system is concerned, and specially important with reference to the structure of the apical system. The special lines in which these modifications take place produce, of course, all possible combinations, yet they give us the key to the sudden appearance, as it were, of structural features of which the relationship must be sought in very distantly related groups. It is to this specialty in the paleontological development that we must trace, for instance, the Cidarid affinities of the Saleniæ, their papillæ, the existence of few large primary interambulacral tubercles, the structure of their apical system, and their large genital plates; while it is to their affinities with the Hemicidaridæ that we must refer the presence of the few larger primary ambulacral tubercles at the base of the ambulacral area, and by their Diademopsid and Echinidian affinities that we explain the indented imbricated actinal system with the presence of a few genuine miliaries. But all the structural features which characterize the earliest types of the Desmosticha can in reality be traced, only in a somewhat rudimentary form, even in the Cidaridæ. The slight undulation of the closely packed, nearly vertical poriferous zone is the forerunner of the poriferous zone first separated into vertical arcs and then into independent arcs. The limitation in the number of the rows of granules in the ambulacral zone, and their increase in size, is the first trace of the appearance of the somewhat larger primary ambulacral tubercles of the Hemicidaridæ and Saleniæ. The existence of the smooth cylindrical spines of the abactinal region of the test naturally leads to similar spines covering the whole test in the other families of the Desmosticha. The difference existing in the plates covering the actinal system from those of the coronal plates leads to the great distinction between the structure of the actinal system and of the coronal plates in some of the Echinidæ.

Passing to the Clypeastridæ and Petalosticha, we trace a parallelism of the same kind, and readily follow in the successive genera of fossil Clypeastroids, but often in widely separated genera, the precise modifications which the poriferous zone has undergone as it first becomes known to us in Echinocyamus and Fibularia, and as we find it in the most complicated petaloid stage of the Clypeastroids of the present day. We readily trace the changes the test

undergoes from its comparatively ovoid and swollen shape to assume first that of the less gibbous forms, next that of the Laganidæ, and finally of the flat Scutellidæ; while we trace in the Echinanthidæ the persistent structural features of some of the earliest Clypeastroids, together with an excessive modification of the poriferous zone. Likewise for the Echinoconidæ we trace mainly the slight modifications of the poriferous zone and of the coronal plates, and finally, when we come to the Spatangidæ we find no difficulty in tracing from the most Desmostichoid of the Spatangoid genera, the modifications of a test in which the ambulacral and interambulacral areas are made up of plates of nearly uniform size, in which the anterior and posterior extremities are barely specialized, to the most typical of the Ananchytidæ, in which the anterior and posterior extremities have developed the most opposite and extraordinary structural features. In a similar way we can trace among the fossil genera of different families the gradual development of the actinal plastron from its very earliest appearance as a modification of the posterior interambulacral area of the actinal side, or the growth of the posterior beak into an anal snout, the successive changes of the anal groove, the formation of the actinal labium, or the development of the bourrelets and phyllodes from a simple circular actinostome, the gradual deepening of the slight anterior groove of some early Spatangoid to form the deeply sunken actinal groove. Equally well we can trace the modifications of the ambulacral system as it passes from the simple poriferous zones of the earlier Spatangoids to genera in which the petaliferous portion makes its appearance, and finally becomes the specialized structure of our recent Spatangoid genera, such as Schizaster, Moira, and the like. Finally, we can trace to a certain extent the development of the fascioles on one side from genera like Hemiaster, in which the peripetalous fasciole is prominent, to genera like Brissopsis, Brissus, and the like, of the present day; on the other, perhaps, or in both combined, the formation of a lateral and anal fasciole from genera like Micraster in Spatangus and Agassizia. Thus we must, on the same theory of the independent modifications of special structural features, trace the many and complicated affinities which so constantly strike us in making comparative studies, and which render it impossible for us to express the manifold affinities we notice, without taking up separately each special structure. Any attempt to take up a combination of characters, or a system of combinations, is

sure to lead us to indefinite problems far beyond our power to grasp.

In the oldest fossil Clypeastroids and Petalosticha, as well as in the Desmosticha, we also find the potential expression of the greater number of the modifications subsequently carried out in genera of later date. The semipetaloid structure of some of the earlier genera of Spatangoids, the slight modifications of some of the plates of the actinal side near the actinostome, are the precursors, the one of the highly complicated petaloid ambulacra of the recent Spatangoids, the other of the actinal plastron, leading as it does also to the important differences subsequently developed in the anterior and posterior extremities of the test, as well as to the modifications which lead to the existence of a highly labiate actinostome. The appearance of a few miliaries near the actinostome constitutes the first rudimentary bourrelets.

Going back now to the Palæchinidæ, the earliest representatives of the Echini in palæozoic times, without any attempt to trace the descent of any special type from them, we may perhaps find some clew to the probable modifications of their principal structural features preparatory to their gradual disappearance. In the structure of the coronal plates, the specialization of the actinal and abactinal systems, the conditions of the ambulacral system, we must compare them to stages in the embryonic development of our recent Echini with which we find no analogues in the fossil Echini of the Lias and the subsequent formations. In order to make our parallelism, we must go back to a stage in the embryonic history of the young Echini in which the distinction to be made between the ambulacral and interambulacral systems is very indefinite, in which the apical system is, it is true, specialized, but in which the actinal system remains practically a part of the coronal system. But here the comparison ceases, and, although we can trace in the paleontological development of such types as Archæocidaris or Bothriocidaris modifications which would lead us without great difficulty, on the one side to the Cidaridæ, and on the other to the Echinothuriæ and Diadematidæ of the present day, we cannot fail to see most definite indications in some of the structural features of the Palæchinidæ of characteristics which we have been accustomed to associate with higher groups. The minute tuberculation, for instance, of the Clypeastroids and Spatangoids, already existing in the Melonitidæ, the genital ring, and anal system, are quite as much Echinid

as Cidarid. The polyporous genera of the group represent to a certain extent the polypori of the regular Echini, and the lapping of the actinal plates of the Cidaridæ and of the coronal plates in some of the Diadematidæ, as well as the existence of such genera as Tetracidaris, of four interambulacral plates in Astropyga, and of a large number of ambulacral plates in some of the recent Echinometradæ, all these are Palæchinid characters which we can explain on the theory of the independent development of the structural features of which they are modifications. We should, however, remember, that the existence of a large number of coronal plates, especially interambulacral plates, in the Palæchinidæ, is a mere vegetative character, which they hold in common with all the Crinoids, — a character which is reduced to a minimum among the Holothurians, and still persists in full force among the Pentacrini of the present day, as well as the Astrophytidæ and Echinidæ.

It would lead me too far to institute the same comparison between the embryonic stages of the different orders of Echinoderms and their earliest fossil representatives. We may, however, in a very general way, state that we know the earliest embryonic stages of the orders of Echinoderms of to-day, which, with the exception of the Blastoidea and Cystideans, are identical with the fossil orders, and that as far as we know they all begin at a stage where it would be impossible to distinguish a Sea-urchin from a Star-fish, or an Ophiuran, or a Crinoid, or an Holothurian, — a stage in which the test, calyx, abactinal and ambulacral systems are reduced to a minimum. From this identical origin there is developed at the present day, in a comparatively short period of time, either a Star-fish, a Sea-urchin, or a Crinoid; and if we have been able successfully to compare, in the development of typical structures, the embryonic stages of the young Echini with their development in the fossil genera, we may fairly assume that the same process is applicable when instituting the comparison within the different limits of the orders, but with the same restrictions. That is, if we wish to form some idea of the probable course of transformations which the earliest Echinoderms have undergone to lead us to those of the present day, we are justified in seeking for our earliest representatives of the orders such Echinoderms as resemble the early stages of our embryos, and in following, for them as for the Echini, the modifications of typical structures. These we shall have every reason to expect to find repeated in the fossils of later periods, and,

going back a step further, we may perhaps get an indefinite glimpse of that first Echinodermal stage which should combine the structural features common to all the earliest stages of our Echinoderm embryos.

And yet, among the fossil Echinoderms of the oldest periods, we have not as yet discovered this earliest type from which we could derive either the Star-fishes, Ophiurans, Sea-urchins, or Holothurians. With the exception of the latter, which we can leave out of the question at present, we find all the orders of Echinoderms appearing at the same time. But while this is the case, one of the groups attained in these earliest days a prominence which it gradually loses with the corresponding development of the Star-fishes, Ophiurans, and Sea-urchins, it has steadily declined in importance; it is a type of Crinoids, the Cystideans which culminated during Paleozoic times, and completely disappeared long before the present day. If we compare the early types of Cystideans to the typical embryonic Echinodermal type of the present day, we find they have a general resemblance, and that the Cystideans and Blastoids represent among the fossil Echinoderms the nearest approach we have yet discovered to this imaginary prototype of Echinoderms.

This may not seem a very satisfactory result to have attained. It certainly has been shown to be an impossibility to trace in the paleontological succession of the Echini anything like a sequence of genera. No direct filiation can be shown to exist, and yet the very existence of persistent types, not only among Echinoderms, but in every group of marine animals, genera which have continued to exist without interruption from the earliest epochs at which they occur to the present day, would prove conclusively that at any rate some groups among the marine animals of the present day are the direct descendants of those of the earliest geological periods. When we come to types which have not continued as long, but yet which have extended through two or three great periods, we must likewise accord to their latest representatives a direct descent from the older. The very fact that the ocean basins date back to the earliest geological periods, and have afforded to the marine animals the conditions most favorable to an unbroken continuity under slightly varying circumstances, probably accounts for the great range in time during which many genera of Echini have existed. If we examine the interlacing in the succession of the

genera characteristic of later geological epochs, we find it an impossibility to deny their continuity from the time of the Lias to the present day. The Cidaris of the Lias and the Rhabdocidaris of the Jura are the ancestors of the Cidaris of to-day. The Saleniæ of the lower Chalk are those of the Saleniæ of to-day. Acrosalenia extends from the Lias to the lower Cretaceous, with a number of recent genera, which begin at the Eocene. The Pygaster of to-day dates back to the Lias; Echinocyamus and Fibularia commence with the Chalk. Pyrina extends from the lower Jura through the Eocene. The Echinobrissus of to-day dates back to the Jura. Holaster lived from the lower Chalk to the Miocene, and the Hemiaster of to-day cannot be distinguished from the Hemiaster of the lower Cretaceous.

Such descent we can trace, and trace as confidently as we trace a part of the population of North America of to-day as the descendants of some portion of the population of the beginning of this century. But we can go no further with confidence, and bold indeed would he be who would attempt even in a single State to trace the genealogy of the inhabitants from those of ten years before. We had better acknowledge our inability to go beyond a certain point; anything beyond the general parallelism I have attempted to trace, which in no way invalidates the other proposition, we must recognize as hopeless.

But in spite of the limits which have been assigned to this general parallelism, it still remains an all-essential factor in elucidating the history of paleontological development, and its importance has but recently been fully appreciated. For, while the fossil remains may give us a strong presumptive evidence of the gradual passage of one type to another, we can only imagine this modification to take place by a process similar to that which brings about the modifications due to different stages of growth, — the former taking place in what may practically be considered as infinite time when compared to the short life history which has given us as it were a *résumé* of the paleontological development. We may well pause to reflect that in the two modes of development we find the same periods of rapid modifications occurring at certain stages of growth or of historic development, repeating in a different direction the same phases. Does it then pass the limits of analogy to assume that the changes we see taking place under our own eyes in a comparatively short space of time, — changes which extend from stages

representing perhaps the original type of the group to their most complicated structures, — may, perhaps, in the larger field of paleontological development, not have required the infinite time we are in the habit of asking for them?

Paleontologists have not been slow in following out this suggestive track, and those who have been anatomists and embryologists besides have not only entered into most interesting speculations regarding the origin of certain groups, but they have carried on the process still further, and have given us genealogical trees where we may, in the twigs and branches and main limbs and trunk, trace the complete filiation of a group as we know it to-day, and as it must theoretically have existed at various times to its very beginning. While we cannot but admire the boldness and ingenuity of these speculations upon genetic connection so recklessly launched during the last fifteen years, we find that with but few exceptions there is little to recommend in reconstructions which shoot so wide of the facts as far as they are known, and seem so readily to ignore them. The moment we leave out of sight the actual succession of the fossils and the ascertainable facts of postembryonic development, to reconstruct our genealogy, we are building in the air. Ordinarily, the twigs of any genealogical tree have only a semblance of truth; they lead us to branchlets having but a slight trace of probability, to branches where the imagination plays an important part, to main limbs where it is finally allowed full play, in order to solve with the trunk, to the satisfaction of the writer at least, the riddle of the origin of the group. It seems hardly credible that a school which boasts for its very creed a belief in nothing which is not warranted by common sense should descend to such trifling.

The time for genealogical trees is passed; its futility can, perhaps, best be shown by a simple calculation, which will point out at a glance what these scientific arboriculturists are attempting. Let us take, for instance, the ten most characteristic features of Echini. The number of possible combinations which can be produced from them is so great that it would take no less than twenty years, at the rate of one new combination a minute for ten hours a day, to pass them in review. Remembering now that each one of these points of structure is itself undergoing constant modifications, we may get some idea of the nature of the problem we are attempting to solve, when seeking to trace the genealogy as under-

stood by the makers of genealogical trees. On the other hand, in spite of the millions of possible combinations which these ten characters may assume when affecting not simply a single combination, but all the combinations which might arise from their extending over several hundred species, we yet find that the combinations which actually exist — those which leave their traces as fossils — fall immensely short of the possible number. We have, as I have stated, not more than twenty-three hundred species actually representing for the Echini the results of these endless combinations. Is it astonishing, therefore, that we should fail to discover the sequence of the genera, even if the genera, as is so often the case, represent, as it were, fixed embryonic stages of some Sea-urchin of the present day? In fact, does not the very history of the fossils themselves show that we cannot expect this? Each fossil species, during its development, must have passed through stages analogous to those gone through by the Echini of the present day. Each one of these stages at every moment represents one of the possible combinations, and those which are actually preserved correspond only to the particular period and the special combination which any Sea-urchin has reached. These stages are the true missing links, which we can no more expect to find preserved than we can expect to find a record of the actual embryonic development of the species of the present day without direct observation at the time. The actual number of species in any one group must always fall far short of the possible number, and for this reason it is out of the question for us to attempt the solution of the problem of derivation, or to hope for any solution beyond one within the most indefinite limits of correctness. If, when we take one of the most limited of the groups of the animal kingdom, we find ourselves engaged in a hopeless task, what must be the prospect should we attack the problem of other classes or groups of the animal kingdom, where the species run into the thousands, while they number only tens in the case we have attempted to follow out? Shall we say "ignorabimus," or "impavidi progrediamus" and valiantly chase a phantom we can never hope to seize?

PAPERS READ.

On the Progress of Geological Investigation in New Brunswick, 1870–1880. By L. W. Bailey, of Fredericton, N. B.

To the student of the physical geography and geology of eastern America, I need scarcely say that the Province of New Brunswick possesses features of peculiar interest. Lying along the western border of the great St. Lawrence Gulf, and including the larger part of the great Acadian basin now above the sea level, it well exhibits in its different portions the peculiarities of that basin, and in the features of its rock-formations, the more prominent phases of its history. Adjoining also the extensive metamorphic tract of New England, from which spurs of highly crystalline rocks traverse portions of its area, it at the same time differs from the latter in being in general less profoundly altered, and in containing a much larger number of fossiliferous formations as horizons for its study. In this way it becomes, to a certain extent, a key to the geology of northeastern America, and as such, acquires a general as well as a local interest.

At the meeting of the Association in Salem, a little more than ten years ago, I had the honor of bringing before your notice, in connection with Mr. G. F. Matthew, of St. John, a paper upon this subject with special reference to its bearing upon the geology of eastern Maine. I now propose to summarize briefly the results, obtained for the most part in connection with the work of the Geological Survey of Canada, accomplished during the last decade — so far, at least, as these tend to confirm or to modify the views then advanced.

A very considerable portion of the work of the geological staff, in the interval referred to, has been topographical, embracing more exact measurements and delineations of the several formations. The results of this work, as well as the views now entertained as to the geological structure of the region, are roughly represented upon the accompanying map, and more accurately upon a series

of maps now being issued from the office of the geological survey, of which these are proofs.

Pre-Silurian.

Beginning with the older formations, we have found no reason to depart from the view first advanced by us, that, beneath the fossiliferous rocks of the St. John or Acadian Group, there exist two, if not three distinct formations, equivalent in part, at least, to the so-called Laurentian and Huronian formations in other parts of Canada. It has been objected that this reference has been based upon the wholly valueless ground of lithological characteristics, and that the strata in question, being destitute of fossils, may even be Silurian; but such objection entirely ignores the fact that, accompanying such differences of lithological characters, there is, at the same time, the most marked evidence of unconformability. A study of the Primordial rocks east of St. John, in 1879, placed this point beyond question, they having been then found by me to occupy irregular troughs in the older Pre-Silurian rocks, resting sometimes upon one and sometimes upon another of the subdivisions of the latter, crossing their strike obliquely, and having at their base coarse conglomerates made up of the waste of the underlying formations. The latter being thus unquestionably of Pre-Silurian age, it is equally obvious that in their vast thickness, in the markedly different conditions under which their several divisions were accumulated, and finally in the further unconformability indicated between these divisions, they represent a vast interval of time, and are at least as old as the Huronian and portions of the Laurentian system, which in all their physical characters they so nearly resemble. No more marked coördination of distant formations could be desired than is here furnished between the great mass of coarse gneisses at the base of the series, associated with finer gneisses, quartzites, graphitic and serpentinous limestones and dolomites (the probable equivalents of the Hastings' series of Mr. Vennor), and capped by the great volcanic series of the Huronian, with its petrosilicious and felsitic strata, ash-rocks and agglomerates, the whole unconformably traversed by bands of the lowest Cambro-Silurian, and the similar succession observed about Lake Huron and elsewhere.

I have only to add that these Pre-Silurian rocks, as represented upon the maps, are confined wholly to the region of the southern

metamorphic hills, nothing of equivalent age or character having yet been identified in the more central and northern portions of the Province.

It should be added in this connection that in the rocks here assigned to the Huronian, there are as a whole two well-marked divisions, the lower (or Coldbrook group) consisting almost entirely of fine grained felsitic strata, with diorites, amygdaloids and porphyries, and the upper (or Coastal group) of schistose rocks, often talcoid or nacreous, with conglomerates and limestones and holding ores of copper, and that between the two there is not unfrequently evidence of at least a partial unconformability, but in general their relations to each other are much more intimate than are their relations either to the underlying Laurentian, or to the Primordial strata which overlie them.

Lower Silurian.

Apart from the fossiliferous Primordial or Menevian beds of St. John, to which allusion has been made, and of which, since 1870, no less than four parallel belts have been recognized and mapped, reference may here be made to the probable age of some of the great belts of slaty and associated rocks which, in the year referred to, were still involved in obscurity. Of these, one, locally known as the Kingston group, and consisting chiefly of felspathic and hornblendic rocks, but which, from the intimate association therewith of fossiliferous Upper Silurian strata, was thought to belong to the latter formation, has been definitely shown to be older, embracing at the same time two members, of which the lower, as originally described, is Huronian, and the upper, composed largely of slates, Lower Silurian. A second group, which has been the subject of much discussion, and upon which much uncertainty even now exists, is that of the great belt of slate rocks which occupies the interval between the granite hills of the Nerepis range, and the southern margin of the great central coal-field. Among these rocks two prominent divisions may be distinguished, of which the one, and older, is usually of a dark color, and more or less highly crystalline (embracing fine-grained gneisses, micaceous quartzites and mica slates, with staurolites, andalusites and garnets), while the upper is pale green, calcareous and argillaceous, but little altered, and the mica, though abundant, only in the form of scattered scales. No evidence of unconform-

ability between these two divisions could be detected throughout the extensive area in which they occur, while the metamorphism of the lower beds seemed to be connected with and proportionate to the proximity of the granitic ridges upon and around which they were found to repose. At several points in connection with the lower beds, fossiliferous strata, pronounced by Mr. Billings to occupy an horizon near the junction of the Upper Silurian and Devonian, were found, while in the upper, obscure remains of plants were met with by Mr. Matthew, which in their outline recalled some of the characteristic ferns of the rich Devonian plant beds of St. John. For these reasons, in the paper submitted to the Association at Salem, in 1869, the term Siluro-Devonian was applied to the first described group and that of Devonian to the second, while some speculations based thereon were offered as to the age of the granites and associated slates which form so striking a feature in the geology of the central portion of the Province and of eastern Maine.

Since the date of the meeting referred to, though much attention has been devoted to the subject, no more definite data as to the age of these argillites in southern New Brunswick has been obtained, their highly disturbed condition favoring the occurrence of faults, and their obscure bedding making the study of their stratigraphy extremely difficult; while through far the greater part of the beds there appears to be an entire absence of organic remains. Some observations, however, made during the last two seasons, partly by Mr. Matthew and partly by myself, on the resembling bands of rock which traverse the more central portions of the Province, farther north, have afforded some facts which, though not yet fully worked out, promise to throw some important light upon the question under discussion.

The metamorphic tract bounding the great central triangular coal-basin of the Province on the north, like that to the south, consists essentially of an axis of granitic rock, varying from ten to twelve miles in breadth, which upon either side is flanked by highly disturbed sedimentary beds, of which the great bulk in each case are argillites, of various degrees of coarseness. Between these two sets of flanking strata, some very noticeable differences may be observed: for while, along the southern side of the granitic mass, no rocks are seen other than simple slates and sandstones, in repeated alternations, and these, even within a few yards of the

granite, retain their features unaltered; on the northern side, there is interposed, between the latter and the corresponding slates and sandstones of Woodstock, a wide belt of highly crystallized sediments, consisting of fine grained gneisses and quartzites, with micaceous, chloritic and hornblendic schists; while along the axes of the parallel folds into which the slates are thrown, are found considerable bands of other hornblendic rocks, such as diorites and amygdaloids, which at times graduate into true syenites. These crystalline strata are also highly calcareous and at some points contain considerable deposits of limestone, while the slaty and arenaceous beds are almost entirely destitute of this material. It is thus probable that somewhat lower beds are exposed upon the northern than upon the southern side of the granitic axis, but a careful study of both leaves little doubt that, as a whole, they are the same series, while the resemblance of both to the dark argillite series of the southern counties is most striking. Admitting their identity—and the resemblance can be traced even to the most minute particulars—we have in this central area, as in that to the south, two essentially distinct slaty groups, of which the dark colored and more or less altered strata last described are one, and the second a series of finer slates, of paler color and highly calcareous, with only scattered scales of mica, occupying much of the country north of the Woodstock Branch Railway. Neither of these two groups has yet been worked out in detail, but from such observations as have been made, it is thought probable that the uppermost group will be found to be continuous with the great area of Upper Silurian rocks known to occupy the northern portion of the county of Carleton, while the lower is an unconformable series, representing some portion of the Lower Silurian formation. In this connection it is interesting to notice that in the north-eastern part of the same county, and in a position corresponding to the trend of the beds now described, Mr. Matthew has recently succeeded in recognizing two unconformable series both containing graptolites and other fossils, and which seem to furnish strong corroboration of the conclusions referred to. It may be added that the character of a portion of these fossils, together with the occurrence with the argillites of the upper series of considerable deposits of iron ore, render it probable that they are in part the representatives of the Clinton group of the New York series, while the lower beds, as has been long supposed, are the probable

equivalents of the Quebec group and Taconic rocks of western New England. Should these views be confirmed by further investigation, the general structure of this region will be not unlike that recently described by Principal Dawson in connection with the fossiliferous iron ores of Pictou, Nova Scotia.

Granites, etc.

In this connection it is interesting to notice the peculiar and often very different relations in which the sedimentary beds, which have been described, stand to the granitic and other crystalline rocks with which they are associated. It has already been stated that while, on the southern side of the main granitic axis, the flanking strata even in close proximity to the latter show little evidence of alteration, those upon the north are but little less crystalline than the granite itself; but it may now be added that this difference is accompanied by another which is equally striking, viz., that while along the one the line of contact, where visible, is regular and uniform, on the other it is characterized by extreme complexity, veins from the granitic mass, of all sizes and shapes and of the most irregular description, penetrating the schistose rocks in all directions, while not unfrequently numerous blocks, or what appear to have been detached masses of gneiss or sandstone, are completely surrounded by or embedded in the granite in such a way as to look like a coarse conglomerate. Notwithstanding this intimate association, however, it is curious to observe that each rock commonly preserves its peculiar features unaltered, the contacts being sharp and abrupt, while the stratification, the texture and even the color of the enclosed masses have been entirely unaffected. While such facts point conclusively to an intrusive or exotic origin for these granitic bands, as well as to the very slight elevation of temperature which must have accompanied their formation, an equally marked but very different mode of occurrence is found to prevail in the case of the dioritic and syenitic rocks which accompany the slates and sandstones farther north. These are equally crystalline, but while containing hornblende in the place of mica, and often large quantities of epidote, they appear to be only a more highly altered form of the associated sedimentary beds, these latter consisting chiefly of bedded ash-rocks, felsites, amygdaloids, etc., passing by regular but very

gradual gradations into typical syenites. In neither case is any countenance given to the view that these great bands of crystalline rocks, which traverse the whole breadth of the Province, and extend to the westward through a large part of the State of Maine, are of Laurentian age or of greater antiquity than that of the stratified formations with which they are associated.

Upper Silurian.

Of the Upper Silurian formation, it is only further necessary to say, in the present connection, that, in addition to the areas already referred to, this age can now be definitely assigned to the very remarkable group of rocks surrounding Passamaquoddy Bay, and which include the peculiar orthophyres or felspar-porphyries of Eastport and Pembroke, Me., these latter having been found to rest directly and almost horizontally upon a series of fossiliferous sandstones, identical with those which at the last-named locality have been long known to contain a rich Upper Silurian fauna. Another instance of the difficulty of distinguishing the rocks of this most variable formation is to be found in the occurrence, first observed by Mr. Matthew, of corals and other Silurian organic remains on the Long Reach of the St. John River, in amygdaloidal ash-rocks, which are undistinguishable lithologically from those of the Huronian formation, and which, like those of Passamaquoddy Bay, had previously been referred to this horizon.

Of the later formations of the Devonian, Carboniferous and Triassic, it is sufficient to say that much detailed work has been devoted to each, the results of which are fully set forth in the published reports of the Canadian Survey, but none of these are of such general interest as to require more particular reference here. It is hoped that the geological map, now upon the point of being issued, will awaken renewed interest in the structure of our great eastern geological basin, and lead to its closer correlation with that of the west.

The Cupriferous Series in Minnesota. By N. H. Winchell, of Minneapolis, Minn.

The red shales and sandstones interstratified with the igneous rocks of the cupriferous series along the shore of Lake Superior in Minnesota, show various stages and kinds of metamorphism. While in some places, as at Good Harbor bay, they are not much changed by contact with the igneous layers separating them, in other places they show a broken stratification, and a tough and siliceous texture, as at Tischer's, near Duluth, where these beds are finely and angularly jointed, have a red color and sometimes a jaspery or conchoidal fracture. In other places they take on a dull brown color, passing to a greenish-brown, becoming slaty and firm, or when in close proximity to igneous dikes, becoming black, dense and basaltiform, as at points east of Grand Marais. In the segregation of minerals, the first to appear are calcite and laumontite. These are disseminated with varying abundance through the shaly layers, as well as through the aluminous and red conglomerates, as seen at the mouth of the Manitou river and at numerous other places. They gather in seams, or in certain parts of the mass, or in the form of amygdules throughout the thickness of the exposed layers. This formation of laumontitic amygdaloids is particularly noticeable in those layers whose thickness is from a foot or two to twenty-five or thirty feet, and sometimes several may be seen alternating in the same bluff, or in a few rods along the shore, with beds of undoubted doleritic rock, as on the west coast of Agate bay, where may be seen five layers of igneous rock with four alternating layers of crumbling, thin-bedded laumontitic amygdaloid, styled "volcanic grits" by Norwood.

These amygdaloids are very susceptible to the destroying action of the waves and of the atmosphere, and their disintegration is the immediate cause of many of the purgatories and isolated arched beds of traprock that ornament the north shore of lake Superior.

When the source and supply of the heat were more continuous, involving greater thicknesses of the sedimentary beds, the siliceous material was more thoroughly fused and disseminated among the other elements. The more limited supply of air and water at

these greater depths seems to have produced, at least is coincident with, a greater abundance of felspathic material, instead of calcite and the hydrous zeolites, throughout the sedimentary layers. Thus the whole is sometimes changed to a non-differentiated ferruginous felsite. When the process was carried a little farther, crystals of red orthoclase appear in the mass, or of orthoclase in the form of translucent adularia, as in the rock of the "great palisades." When the metamorphism is carried still farther, involving in its slower progress large thicknesses of the red sedimentary shales and sandstones, they become almost wholly crystalline, as seen in the red bluff that incloses Beaver bay on the west, and in the red granite bluff a few miles east of Beaver bay. The relationships of these changes with one another, and to the igneous rock, are evident at numerous places along the shore between Duluth and Grand Portage, and on Isle Royale; and their significance and application to the stratigraphic geology of the northeastern part of the state are very important. On passing inland from the lake shore back of Grand Marais, and up the Devil's Track and Brulé rivers, the red semi-metamorphic slates of the shore can be followed over a wide extent of territory, gradually becoming more changed and crystalline, in receding from the lake shore. They pass into red granite and gneiss (hornblendic) which rises in conspicuous hills, and shows perpendicular exposures along the lakes and streams, sometimes several hundred feet high, as at Brulé mountain, and at Misquah lake (T. 64.1 W., Sec. 32). In some places this highly crystalline condition of this red formation is seen to give place suddenly to areas of igneous rock of a dark color, and showing a very different mineral composition, and then to return again. This takes place sometimes on the high hills, the two kinds of rock alternating superficially in irregular patches, as at Duluth, and at Duck lake and Frog Rock river on the portage trail from Little Saganaga lake to the head waters of the Temperance river, in the northeastern corner of the state. Sometimes the tilted red sedimentary beds seem to be overlain by the igneous rock and sometimes underlain by it, the red rock, when consisting of sandstone at first, having been hardened into a quartzite. Several tilted red quartzite hills, very similar to the quartzite hills at New Ulm, and in Cottonwood and Rock counties, occur in this connection at

Duck and Wind lakes, their relation to the igneous rocks being most perfectly exemplified. Sometimes this red quartzite becomes micaceous and also felsitic, as may be seen at Wind lake. The great extent and the more intense metamorphism of this red formation, in the country lying to the north and west of Lake Superior, accompanied by larger belts of the igneous rock, more coarsely crystalline, not only show that the seat and source of the igneous action was there instead of in the basin of the lake, but also that it was longer continued. It implies also, that a similar modification of these beds may be looked for throughout the northwest, wherever the formation is known to have been upheaved by igneous forces, although the igneous rock itself may be wanting.

Northwest of Lake Superior the igneous rock forms the main watersheds, rising in two main ridges, or ranges of mountains, that run southwestwardly, one known as the *Mesabi*, and one as the *Sawteeth mountains*, though the former term is not restricted to this belt of high land. The width of the belt of metamorphic red shales and sandrock, associated with the igneous rock, is about thirty miles in a right line, extending from the headwaters of the Brulé and Temperance rivers to the shore of Lake Superior. The Sawteeth range of mountains, which is that nearer the Lake Superior shore, dies away in passing to the southwest, and the Mesabi belt of igneous outflow approaches the lake shore, appearing at Duluth in the form of the "Rice Point Granite." The tilted red shales, conglomerates and sandstones at Fond du Lac, a few miles west of Duluth, are the same as those seen associated with the igneous rock all along the shore. They lie there on a white-quartz, pebbly conglomerate, of a few feet in thickness, which lies unconformably on the roofing slates of the Huronian, the same formation that succeeds to the red rock formation toward the northwest, at Ogishke Muncie and Knife lakes, northwest of Grand Marais.

The mineralogical characters of these belts of igneous rock, which form some of the main features of the topography, seem to ally them to the Norian rocks of T. S. Hunt, and to the labradorite rocks of Canada. At least, if they be not the western extension of those formations, then those formations have not yet been discovered in Minnesota. But several traverses have been

made of the country northwest of Lake Superior, for the purpose of geological examinations without finding anything that is at all comparable to those formations, if it be not the rock of these hill ranges. The rock consists generally of some felspar, which at Duluth has been found to be labradorite in large per cent., and at some places constituting over ninety per cent. of the mass, with varying proportions of augite and magnetite, or magnetic menaccanite, with various accessory ingredients, or minerals that result from change. It is massive, firm, dark-colored, and rises in low mountain ranges, as already stated. If its relation to the red granites and gneisses with which it is accompanied were not so evident, by simply noting the changes from the lake shore northwestwardly, it would hardly be presumed to be a parallel of the igneous rocks of the coast, any more than the red gneisses and quartzites would be of the shales and sandstones that are interbedded with them at the coast. So far as yet examined, these labradorite rocks contain no bands of limestone, which is due probably to the fact that the Cupriferous Series in the northwest is not known to contain any beds of limestone. In the absence of this element, and in this only, so far as can be judged by the writer, these labradorite rocks seem to differ from the labradorite rocks of the "Upper Laurentian" of Canada.

Inferentially, therefore, the so-called "Upper Laurentian," containing *Eozoon Canadense*, seems to parallelize with the igneous rocks of the Cupriferous Series, or rather with the modified interbedded sedimentary portions of it, and hence the *Eozoon*, instead of being truly a Laurentian organism, seems to be one of the Cambrian or Lower Silurian. The abundant graphite of the "Laurentian" which pointed the way to the prediction of organisms in that formation, is also found in the modified quartzites and shales of the Cupriferous Series in perhaps equal abundance in the state of Minnesota.

AN ATTEMPT TO ESTIMATE APPROXIMATELY THE DATE OF THE CLOSE OF THE GLACIAL EPOCH, FROM AN INSPECTION OF THE KAMES AND KETTLE-HOLES OF NEW ENGLAND.

By G. FREDERICK WRIGHT, of Andover, Mass.

[ABSTRACT.[1]]

AFTER referring to the evidence connecting these phenomena with the closing period of the glacial epoch, he presented, as illustrative of innumerable other cases, the facts concerning a kettle-hole near Pomp's pond in Andover. The distance from rim to rim of the hole described is 380 feet. The rim is composed of gravel. The height of the rim above the surrounding plain is from forty to fifty-two feet. The height above the peat bog in the centre of the depression is fifty feet. The distance across this bog is ninety-six feet. The nature of the material composing the rim is such, that the depth of the depression could never have been more than twenty-four feet greater than now. Twenty-four feet of sediment in the bottom of a conical depression would only equal eight feet upon the present surface. The question is how long a time would be required for the wash of the rains, the dirt brought by the winds and the solid material collected by vegetation, to fill this depression to the amount indicated. No definite answer can be given, but if one hold, with Mr. Croll, that the great glacial period closed 80,000 years ago, he must believe that only an inch of sediment would accumulate upon the area of this peat bog in 1,000 years. Whereas, if he bring the close of this period down to 10,000 years ago, the rate of accumulation would seem sufficiently slow to tax severely even a credulous imagination.

[1] The paper with a plate is published in full, in the American Journal of Science for February, 1881.

The Iron Ores of the Brandon Period[1]. By Henry Carvill Lewis, of Germantown, Pa.

[ABSTRACT.]

The theory that a great portion of the iron ores of our lower Silurian limestone valleys are of a tertiary age was first proposed by Prof. E. Hitchcock, but has been rejected by many geologists. The present paper describes in full recent discoveries, made by the writer, of lignite associated with limonite iron ores in the limestone valley of Montgomery county, Pa., and shows their relation to the deposit at Brandon, Vt., and their bearing upon a theory of the age of iron ores in similar positions in the Atlantic States. The lignite of Brandon, lying within beds of plastic clay, kaolin and iron ore, was shown by Lesquereux to be of tertiary age. Lesley afterwards described strata of lignite in a similar position at Chambersburg, Pa., but regarded them as local deposits of late date. More recently Prime has found lignite in a plastic clay at Ironton, Pa., and supposed it to have been transported by a glacier. The present paper shows that in each of these cases the lignite lies below the surface drift, and that, as at Brandon, the latter lies unconformably upon the plastic clays containing the lignite.

The occurrence of lignite, in connection with limonite iron ore, plastic clay, kaolin and firesand in a number of places in Montgomery county, Pa., is described, and it is shown that these localities lie in a line corresponding to the line of strike of all the iron ores of the valley. Overlying the plastic clay which contains the lignite is what appears to be a decomposed lower Silurian hydromica slate, and for this reason the iron ores had been supposed to be of primal age. It is shown that this decomposed material and the underlying iron ores have been originally derived from lower Silurian slates, and have been re-stratified in an age intermediate between Triassic and Upper Tertiary.

The iron ores of this region are divided into four classes: (1) Gneissic Ore; (2) Primal Ore; (3) Tertiary (Brandon) Ore; (4) Drift Ore. The last two classes of ore are often found at the

[1] The Iron ores and Lignite of the Montgomery Co. Valley, by the writer. Proc. Acad. Nat. Sciences, Phila., 1880.

same locality; the latter lying unconformably upon the former. The paper discusses at length the age of the drift containing the latter. Notwithstanding the fact that a region of triassic red shale lies north and east of the valley, not a single fragment of such rock occurs in this drift. The pebbles are composed almost wholly of Potsdam sandstone,—a material now in great part eroded away in this vicinity. The evidence is strong that this drift was not caused by any flood from the north. That it is older than the Glacial Epoch is also indicated both by the great amount of erosion it has suffered, and by the fact that in the adjoining triassic region no trace of drift occurs. It was perhaps formed at a time when hills of Potsdam sandstone, since eroded, stood as a barrier between the limestone valley and the triassic rocks to the north. It is of interest to find that a large proportion of the pebbles of the sub-cretaceous clays of New Jersey are also formed of Potsdam. The four different gravels of the Delaware valley are described, and it is shown that the drift ore of the Montgomery county valley belongs to the oldest of these, and is probably of tertiary age.

It follows that the strata containing iron ore and lignite, which underlie this drift unconformably, are yet older. Some facts point to a Wealden age, but the identity of the deposits with that at Brandon, in which Tertiary plants are found, indicates a middle Tertiary, perhaps *Oligocene* age. Since an exact geological age cannot at present be assigned to these deposits, it is thought best to group them together under the name of the *Brandon Period.*

Attention is directed to another deposit of lignite and iron ore near Augusta, Ga., whose geological situation and the section given are so remarkably similar to those of Brandon, Chambersburg, Ironton and the Montgomery county valley, that it appears to belong to the same formation, and to indicate that in this Brandon Period there was an inland fresh-water formation in eastern America of large extent and importance.

On the age of the Copper-bearing Rocks of Lake Superior.

By M. E. Wadsworth, of Cambridge, Mass.

[ABSTRACT.]

Two principal views regarding the relative age of the copper-bearing traps of Keweenaw Point and the eastern sandstone have been and are now held. The first regards the traps and sandstone as of the same age; the second places the traps as the earlier formation (pre-Paleozoic), and the eastern sandstone as the later one (Paleozoic). The traps are said to have formed a sea wall along the eastern side of the Point, while the sandstone was deposited against the base of the cliff. The sandstone is said to be horizontal, or at most to have only a slight inclination at the base of the cliffs, at which point it contained pebbles of the trap derived from them. This evidence was principally obtained at the Douglass Houghton Falls, and on the face of the published accounts appears conclusive. It has given rise to the supposition that the traps formed a distinct geological series belonging to Azoic or Archean time, which series has been recognized by lithological characters far and wide. As this Keweenawan series was first established on Keweenaw point, it must stand or fall by observations made there, and not, as some would make it, by observations in Wisconsin, the northern shore of Lake Superior, or in Newfoundland.

By personal observations at the Douglass Houghton Falls, I have found that the eastern sandstone instead of being horizontal as stated, dips north 45° west, 25°, while passing down the river the dip gradually diminishes in angle until it is only 5° at the mouth of the ravine below the falls. Instead of the cliffs at the falls representing the termination of the copper-bearing traps as stated, I found several basaltic overflows below the falls interstratified with the conglomerate and sandstone. The relation of the copper-bearing traps below the falls to the interbedded sandstones and conglomerates, is the same as it is in the rocks above the falls. The last basaltic flow here is a thin sheet some two feet in thickness, which is interbedded between sandstones, having the same dip as the basalt. The finding of the copper-bearing rocks below the Douglass Houghton Falls, explains the presence of pebbles of melaphyr in the conglomerate at the falls, and shows that the observations referred to were made within the region of the copper-bearing traps, the observers not having found the junction of

the traps with the sandstone at all. Something more is necessary in such observations than simply to find a sandstone on the eastern side of the copper-bearing rocks; it is necessary to know that this is part of the eastern sandstone and not a bed intercalated with the trap. Now as I find that the eastern sandstone underlies the trap conformably, that is, as conformably as a bed can underlie a lava which has flowed over it, it must be older in order of time, but of the same geological age with the copper-bearing rocks. As my observations show the incompleteness of those on which the "Keweenawan series" was founded, it seems right to hold, until my observations shall be disproved, that the "Keweenawan series" has no foundation, but that the copper-bearing rocks are of the same age as the eastern sandstone. This series has been advocated upon lithological evidence, and said to be recognized in other localities from this evidence, but the above observations furnish another proof of the absurdity of founding and identifying geological systems simply by lithological characteristics.

In ascending the Hungarian river, I found the same relations of the trap to the sandstone. The last bed of the eastern sandstone, dipping north 45° west, 20°, is overflowed by the first bed of trap which has baked the former; the same as these basaltic flows indurate the sandstone within the copper-bearing rocks. This thin flow is overlaid by a conglomerate and sandstone, at the base of which we find pebbles of the underlying trap. Furthermore, similar alternations of trap, sandstone, and conglomerate extend until the western sandstone is reached.

It seems then that the writer has advanced sufficient evidence to prove that in the parts visited, the eastern sandstone conformably underlies the copper-bearing rocks and that both are of the same geological age.

The question of the relative ages of these rocks was the one with which my time was occupied, but so far as the absolute geological age of the sandstone, and therefore of the trap, is concerned, it would seem that the evidence brought forward by Dr. Rominger, until disproved, shows that Messrs. Foster and Whitney were correct in regarding it as of Potsdam age.

For a fuller treatment of this subject the reader is referred to another publication.[1]

[1] Notes on the Geology of the Iron and Copper Districts of Lake Superior. Bulletin of the Museum of Comparative Zoology, 1880, Vol. VII (Geol. Series, Vol. I), No. I, 157 pp., 6 plates.

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TURQUOISE OF NEW MEXICO. By B. SILLIMAN, of New Haven, Conn.

THE existence of this comparatively rare gem in New Mexico is a fact long known—the chief locality being at Mt. Chalchuitl in Los Cerillos, about twenty-two miles southwest of the ancient town of Santa Fé, the capital of that territory. We are indebted to Prof. Wm. P. Blake for our first detailed notice of this ancient mine in an article published in 1857, in the American Journal of Science.[1]

It was subsequently visited by Dr. Newberry and mentioned in one of his reports, and by others. I have lately had an opportunity of examining this very interesting locality, since it has been laid open in the old workings, by the recent explorations of Mr. D. C. Hyde, and thus rendered accessible to observation.

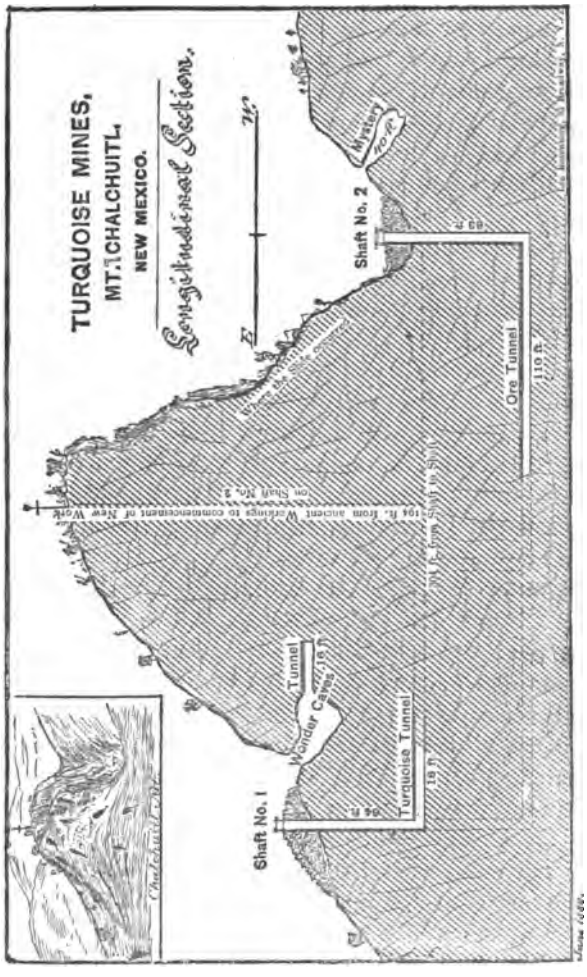
The Cerillos Mountains have recently come into notice from the partial and as yet, superficial exploration, of very numerous mineral veins which are found to intersect them and which carry chiefly argentiferous galena, with some gray copper, rich in silver, giving promise of mines of value when opened in depth. I have elsewhere spoken more particularly of these veins and of the rocks that contain them. These rocks are all eruptive or plutonic rocks of the family of the *Augite Trachytes*, the microscopic study of which reveals the interesting fact that they are of the same family which, the world over, carry the richest and most permanent ores of silver, with some gold, and which there is good reason to believe are here penetrated by true fissures which may be followed to any depth, without exhaustion. In the centre of this district, which is not more than about six miles by four in extent, rises the dome of Mount Chalchuitl, the summit of which is about 7,000 feet above tide, and is therefore almost exactly on a level with the Plazza of Santa Fé, across the valley of the river of that name, to the northeast. In the other direction this mountain has its drainage into the valley of the Galisteo, which forms the southern boundary of the Cerillos district. The geological age of the eruption of these volcanic rocks is probably Tertiary. The rocks which form Mt. Chalchuitl—the Indian name of the turquoise—are at once distinguished from those of the surrounding

and associated ranges of the Cerillos by their white color and decomposed appearance, closely resembling tuff and kaolin, and giving evidence, to the observer familiar with such phenomena, of an extensive and profound alteration, due, probably, to the escape through them, at this point, of heated vapor of water and perhaps of other vapors or gases, by the action of which the original crystalline structure of the mass has been completely decomposed or metamorphosed, with the production of new chemical compounds. Among these the turquoise is the most conspicuous and important. In the seams and cavities of this yellowish-white and Kaolin-like tuffaceous rock the turquoise is found in thin veinlets and little balls or concretions called "nuggets," covered on the exterior with a crust of the nearly white tuff, and showing on cross fracture the less valued varieties of this gem, more rarely offering fine sky-blue stones of higher value for ornamental purposes. It is easy to see these blue-green stains in every direction among these decomposed rocks, but the turquoise in masses of any commercial value is extremely rare, and many tons of the rock may be broken without finding a single stone which a jeweller, or virtuoso, would value as a gem.

The observer is deeply impressed on inspecting this locality with the enormous amount of labor which in ancient times has been expended here. The waste or débris excavated in the former workings covers an area which the local surveyor assured me, by his measurement, extends over at least twenty acres of ground, and which it is easy to see has a very large bulk. On the slopes and sides of these piles of rubbish are growing large cedars and pines, the age of which—judging from their size and slow growth in this very dry region—must be counted by centuries. It is well known that in 1680 a large section of the mountain suddenly fell in from the undermining of the mass by the Indian miners, killing a considerable number, and that this accident was the immediate cause of the uprising of the Pueblos and the expulsion of the Spaniards, which happened in that year just two centuries since.

The accompanying vertical section of the mountain from east to west will give a good idea of the old workings and of the shafts and tunnels projected and partly carried out, by Mr. Hyde. The irregular openings, named by Mr. H., "wonder caves" and the "mystery," are the work of the old miners, and the whole hillside from the flag-staff to the "mystery" was worked out by them also.

It was this sharp slope of the mountain which fell. In these chambers, which have some extent of ramification, were found abundantly the fragments of their ancient pottery, with some entire vessels, some of curious workmanship, ornamented in the style of



color so familiar in the Mexican pottery, and associated with these, numerous stone hammers, some to be held in the hand and others swung as sledges, fashioned with wedge-shaped edges and a groove for the handle. In one case, a hammer weighing over

twenty pounds was found, while I was at the Cerillos, to which the wyth was still attached, with its oak handle—the same scrub oak which is found growing abundantly on the hillsides now, and quite well preserved, after at least two centuries of entombment in this perfectly dry rock.

The stone used for these hammers is the same hard hornblende Andesite or Propylite, which forms the Cerro d'Oro and other Cerrillos hills, very hard and tough. With these rude tools and without iron and steel, using fire in place of explosives, these patient old workers managed to break down and remove the incredible masses of these tuffaceous rocks which form the mounds already described.

That considerable quantities of the turquoise were obtained can hardly be questioned. We know that the ancient Mexicans attached great value to this ornamental stone as the Indians do to this day. The familiar tale of the gift of large and costly turquoise by Montezuma to Cortez for the Spanish crown, as narrated by Clavigero in his history of Mexico, shows the high value attached to this gem. It is not known that any other locality in America has furnished turquoise in any quantity—the only other place outside of Los Cerillos, where it is found at all, being that near Columbus District in Nevada, discovered by Mr. J. E. Clayton, and this has not been worked as yet.

The origin of the turquoise of Los Cerillos in view of late observations is not doubtful. Chemically it is a hydrous alumi num phosphate. Its blue color is due to a variable quantity of copper oxide derived from associated rocks. I find the Cerillos turquoise contains 3.81 per cent. of this metal. Neglecting this constituent the formula for turquoise requires: Phosphoric acid, 32.6; alumina, 47.0; water, 20.5. Total, 100.1.

Evidently the decomposition of the felspar of the trachyte has furnished the alumina, while the apatite, or phosphate of lime, which the microscope detects in the thin sections of the Cerillos rocks, has furnished the phosphoric acid. A little copper is diffused as a constituent also of the veins of this region, and hence the color which that metal imparts.

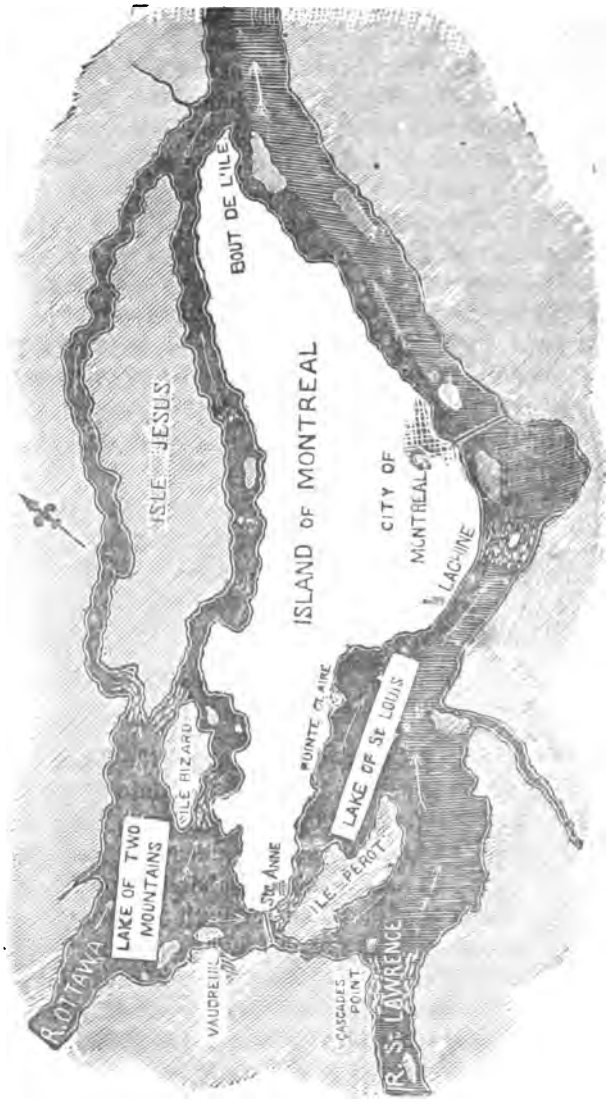
The inspection of thin sections of the turquoise by the microscope, with a high power, shows the seemingly homogeneous mass, of this compact and non-crystalline mineral, to consist of very minute scales, nearly colorless, having an aggregate polarization, and showing a few particles of iron oxide.

The rocks in which the turquoise occurs are seen, by the aid of the microscope and polarized light, in thin sections, to be plainly only the ruins, as it were, of crystalline trachytes showing remnants of felspar crystals, decomposed in part into a white kaolin-like substance, with mica, slag and glassy grains, and quartz with large fluidal enclosures, looking like a secondary product. There is considerable diversity in their looks, but they may all be classed as trachyte-tuffs and are doubtless merely the result of decomposition, as already indicated, of the crystalline rocks of the district along the line of volcanic fissures. In fact there are in the northeast direction other places, one of them at Bonanza City, a distance of probably two to three miles, where the same evidence of decomposition is found, and in the rocks at this place I find also the turquoise in forms not to be distinguished from that of the old mine. Mr. Hyde has shown me lately in New York a large number of the Cerillos turquoise polished, one of huge size; and among them a few of good color and worthy of consideration as gems, some of them an inch in length and quite thick, but they are not of faultless beauty.

The Island of Montreal an Island, not in the St. Lawrence, but in the Ottawa. By William Boyd, of Cambridge, Mass.

The waters of the Ottawa, at the foot of the Lake of Two Mountains, divide on the Island of Montreal into two branches of nearly equal volume. The lesser branch—making for the south of the island, and also for several miles farther south along the Vaudreuil shore to Cascades Point—subdivides on Isle Perrot, an insulation of seven or eight miles in length (from west to east) and from two to three miles in breadth, lying south, and part of it west, of the head of the Island of Montreal. There are rapids in each of the sub-branches. The southern branch of the Ottawa encounters the St. Lawrence at Cascades Point; and also at the foot of Isle Perrot, where the sub-branches reunite. From Cascades Point and the foot of Isle Perrot the Ottawa water flows in the same bed as that of the St. Lawrence,—of course on the

northern side of the St. Lawrence water, — filling from a quarter to a third of that bed; which fact is evident from the difference in



Map showing how the waters of Ottawa surround the Island of Montreal. Note direction of arrows.

color of the water of the two rivers, — that of the Ottawa being

of a brownish, that of the St. Lawrence of a greenish, hue. The two river-streams run side by side unmixed to the Ottawa's lowest mouth, at the foot of the Island of Montreal, where this southern stream or branch of the Ottawa joins the northern branch of the same river. (Thence these river-streams flow onward, side by side, as before,—the Ottawa's stream doubled in volume,—to Lake St. Peter, where they commingle in its slack water and the tidal head.)

If the River Ottawa should cease to exist and the River St. Lawrence should remain, what is now the Island of Montreal would probably—from the high level above St. Anne and below Vaudreuil of the bed of the then extinct Lake of Two Mountains, and from the very considerable fall which would on the disappearance of the Ottawa take place in the St. Lawrence below the Cascades Rapids—be an island no longer; but if the St. Lawrence should cease to exist and the Ottawa should remain, what is now the Island of Montreal would be an island still.

From the above facts the writer concludes that the Island of Montreal is an island, not, as has heretofore been held, in the St. Lawrence, but in the Ottawa.

THE LAW OF LAND-FORMING ON OUR GLOBE. By RICHARD OWEN, of New Harmony, Ind.

THE object of the present paper will be to state the law by which dry land shows itself above the ocean, and to sustain that statement by so many coincident facts, which can be traced on any good globe, or on large maps of separate continents, as to satisfy the scientist that the law is of universal application to geographical and geological phenomena, with few, if any, exceptions:

General Law: The land on our globe shows itself above the ocean level, in definite multiple proportions, by measurement; the unit is the angular difference between the axis of rotation and the axis of orbital progression.[1]

[1]For convenience, as that angle has been lessening for centuries, we might call it 24°. We then have:

The geographical and geological unit	$= 24° = \frac{360°}{15}$
Greatest width and length of continents	$= 3 \times 24° = 72° = \frac{360°}{5}$
Radius for continents	$= 36° = \frac{360°}{10}$
Half radius	$= 18° = \frac{360°}{20}$

The measure for oceanic distances is the complement of 24° = 66°. The ratio of land to water, as shown by Prof. Dana, is as 100 : 275. The ratio of 24° : 66° .: 100 : 275. All measurements are to be estimated at the equator.

To render the demonstration of the above general law more intelligible, it may be advantageous to consider it under several separate heads, or subdivisions, following up each of these heads or sections, by its demonstration.

I. *First subdivision or section of the law:* Many longitudinal elevations and depressions on the earth's surface (the result apparently of cooling and contraction), especially near the greatest median extension of each continent, north and south coincide with some meridian. Consequently, if we conceive the planes of these great circles produced to the earth's axis, the terminal axis of the spherical wedges thus formed would coincide with the axis of rotation. Further, this shrinkage has caused a north and south continent to appear in each of four equal spherical wedges of our earth, estimating the 90° for each wedge on the equator. Minor north and south extensions can be traced at intervals, often of 4½° or 9° apart, all around the globe, alternating usually, at least on the continental outlines, with trends which form with the meridians, angles of about 23½°.

Demonstration: After placing the artificial globe in such position that the terrestrial poles are at the wooden horizon, and the eastern extremity of Brazil at the brazen meridian, we find North and South America occupying one quarter, Europe and Africa a second, Asia and Australasia the third, while the fourth embraces North and South Oceanica, which may be regarded, either as a sunken continent, or as an ocean with north and south island groups.

In Europe and Africa combined, we find the greatest median elongation in about long. 21° to 22° E. of Gr. In Asia, the median elongation is about in long. 99° to 100° E. In North America, it is in long. 96° W., in South America in long. 69° to 70° W. In N. Oceanica, the elongation is from Point Barrow, through the Sandwich Islands to Tahiti, therefore in about long. 155° W.; lastly for Australasia, the elongation is about in long. 144° to 145° E.

To prove the minor north and south extensions, on coast lines as well as through continents, we may begin anywhere on the equator, and trace, north and south, great circles at intervals of about 9° apart. We find, likewise, evidences of alternate elevations and depressions: a great elevation in one continent seeming to have resulted in a depression on the opposite side of the globe.

To give a single example, follow the immense heights of the

eastern Himalayas, in about long. 90° E., and the elevated table-lands of Central Asia, to the opposite side of the globe. in long. 90° W., and you there find the great depressions of Hudson Bay our fresh-water lakes, the Gulf of Mexico, and the deep, south-eastern Pacific.

II. *Second subdivision of the law:* Although the median lines of continents run north and south, the outlines of continents form with the meridian lines, angles of about $23\frac{1}{2}°$[2]; the terminal edges of their planes, projected to the earth's centre, are consequently parallel to the axis of orbital progression. Reckoning from a trend which passes through the Straits of Bali and of Macassar; and which defines the eastern trend of Asia, including Japan, we find successively five eastern trends of great continents, exactly 72° apart, or one-fifth of 360°. These trends mark belts of great seismic force, apparently where the crust has less thickness than along the median lines of continents, where volcanoes and earthquakes are more rare; or where, as Dana expresses it, there are lines of fracture or weakest cohesion.

Demonstration: 1. Beginning with the eastern trend of North America, we find it passing from Yucatan, nearly parallel to the Gulf Stream, doubtless becoming the chief agent in gradually bringing the Appalachians to their present level, with all their Basaltic flows and their contorting power over the coal beds; thence past the palisades of New York, to the volcanic and geyser region of Iceland. On the opposite side of the globe, this belt crosses diagonally the Caspian Sea, near the Field of Fire (Baku) and by the upheaval of the western Ghauts defined the west coast of Hindostan.

2. The force connected with the eastern trend of South America probably separated the Falkland Islands from the continent; also Madeira proper (Funchal) from Porto Santo; produced the numerous depressions and elevations in Great Britain, during the Tertiary but especially the Glacial Periods, causing apparently the separation of Ireland and the vast rents along the almost fathomless and never-frozen Loch Ness, as well as other north-northeast fissures through England and Scotland, where all admit a change of level of at least 600 ft. in the Glacial Epoch, while others claim 2000 ft. At an earlier period the force exerted along this great circle may have

[2]This law I pointed out in "Key to the Geology of the Globe," a work which I published in Nashville, Tenn., in 1857. The generalization was accepted by scientists: see Dana's Manual, p. 38, and Dawson's "Origin of the World," p. 184, also Coues' "Structure of the Earth," p. 4.

aided in raising the Scandinavian Mountains, just as it is now elevating considerable portions of Norway and Sweden. On the opposite side of the globe, this belt or zone will be seen to pass through some of the volcanoes of Japan, of Solomon's Archipelago, and other oceanic fire-vents. Further, this seismic continental trend accounts for the fearful earthquakes of New Zealand, as it skirts the northwest coast of that Island, and passes through Cook's Straits.

3. The eastern trend of Africa seems either to have separated Madagascar from the continent, or to have brought up that island at a later period; but with the exception of passing through the volcanic region of the Aleutian Islands and Easter Island, its seismic power appears to have been less than that of the other four eastern continental outlines.

4. A trend 72° E. of the one just followed gives us the volcanoes of Java and Celebes, passing through the Straits of Bali and of Macassar (where Wallace observed a *continental* difference in the flora and fauna, east and west of those straits) thence along the volcanic belt of Japan and its warm Gulf Stream, then through the Kurile Islands and Kamchatka, also Alaska and along the line of depression connecting Bear Lake, Athabasca, etc., with the Lake of the Woods. Thence it passes through Lake Superior, South Carolina, Hayti and the Bahamas, and reaches the great earthquake region of Venezuela at Caraccas; thus accounting for the seismic disturbance in South Carolina in 1812, just previous to the destruction of La Guayra and Caraccas, as well as for the submarine table-land, recently discovered by the Coast Survey, extending from South Carolina to the Bahamas.

5. Lastly, we may trace, in the Pacific, the trend which, passing between Australia and New Zealand, intersects the terrestrial equator at or about the same nodes where it is crossed by the magnetic equator of dip defining, in this part of its course, the western coast of South Africa.

Time would not permit to give details following the other western trends; but any one with a good globe can easily find them.

III. *Third subdivision of the law:* Besides these two forces, which, as shown, exert their power along lines parallel respectively to the axis of rotation and to that of progression, each northern continent had two foci, nearly on its median line; the more northerly focus, centre, or dominant, near the Arctic circles; the other at the exact geographical centre of the continent. Concentric

circles, around these foci, not only mark important additions to the land and orography of each continent, but especially pass, as they enlarge, through areas of successive geological periods, from the older to the newer. Usually a radius, from the more northerly focus, of from 18° to 23½° or 24°, encloses areas chiefly Archæan to the junction with Paleozoic, while that of 30° marks Mesozoic, and 36°, areas of Cenozoic.[3] From the more southerly focus, which is also the geographical centre, a radius of 36° will usually enclose the continent with most of its collateral extensions, while 24° will define the continent proper. Between those two circles will be found almost exclusively Tertiary areas; outside of those in three continents exist the remains of remarkable Quaternary animals; and within the radius of 24° (at least until the southern arcs meet the northern curves) we have, as we approach the more central focus, successively Mesozoic and Paleozoic.

In the southern continents, there is only one centre. With a radius of 24°, from that centre, to 36° which completes the continent, we find, as before, Cenozoic; while, within the smaller circle, we have older formations, commonly Mesozoic.

Demonstration: For the sake of convenience the approximate position of the foci, for each continent, is subjoined; subject to such slight modifications as future investigations may require.

1. For *Europe*, the northern dominant is in Scandinavia, about long. 22° E. of Gr., lat. 68° N. The geographical and later centre is in long. 20° E. and lat. 49½° N.

2. For *Asia*, the northern focus is in Siberia about long. 99° E. and lat. 71° N. The centre is in the same long. and in about 51° N. lat.

3. For *North America*, the northern dominant is in Boothia Felix, about long. 96° W. and lat. 70°–71° N. The later focus is near the height of land, west of Lake Superior, about long. 94° W., lat. 47° to 48° N.

4. For *Africa*, the single centre is at St. Thomas's Island, where the magnetic equator of dip crosses the terrestrial equator.

5. For *South America*, the centre is on the Tropic of Capricorn, in about long. 65° W.

6. For *Australasia*, the centre is also on the Tropic of Capricorn, in about long. 148° E.

[3] Some variation seems to exist between those continents which, like North and South America, have their main extension from north to south, and the continents, which, like Asia, extend rather east and west.

Examining critically these continents, *seriatim*, a few of the most striking illustrations will be given:

1. *Europe:* Placing one foot of the compasses, or one end of a string, at the Scandinavian focus, with a radius of 12°, the area enclosed will be nearly all Archæan[4] (Hypogene of Lyell); with radius 18°, a semicircle from Iceland to the Urals, passes chiefly through Paleozoic regions, often marking coal fields, and, in Russia, a great Permian area; a radius of 24°, from same focus, reaches the Alps at Mount Rosa, and marks probably the beginning of their rise in Mesozoic Times, although their latest height was Cenozoic. Crossing the head of the Adriatic, this dynamic curve seems to have broken the way for the Danube at the "Iron Gates," to have formed the mud volcanoes of Perekop in the Crimea, and reached the south end of the Urals. A radius of about 29° to 30° gives a belt, chiefly Cretaceous, described by Dr. Dana as extending 1140 miles, from the Pyrenees to the Caucasus; while radius 36° gives us almost exclusively Tertiary from the Straits of Gibraltar along the south shore of the Mediterranean, over Mount Hermon to the volcano of Demavend, on the south shore of the Caspian.

Moving the compasses to the later focus, in N. Hungary, with a radius of 24° we enclose the whole of Europe to the Urals and Caspian, taking in also some of N. Africa and part of Iceland. A radius of 36° from this centre extends outside of Europe, but embraces some important islands chiefly of Tertiary formation, and may have extended exactly to the line which probably constituted the north of Africa, when the Sahara was under water. A contracted radius of 11° to 12° from the Hungarian focus passes through the Skager Rack, Gulf of Finland, Crimea, Bosphorus, Gulf of Corinth, earthquake regions of Calabria, Stromboli's fire, Straits of Bonifacio, etc., to the Straits of Dover; seeming to have been seismic rather than geological in character and effects.

2. *Asia:* With the compasses in the northern dominant, and a radius of 24°, we range from our former initial focus in Scandinavia, through Middle Asia, curving with, and enclosing, the Altai and Stanovoi Mountains; while radius 36° marks the Kuenlun range, and sweeping round to Behring's Straits, leaves, between this 36° radius and the 24° radius, the whole desert of Gobi. Transferring the compasses to the geographical centre in southern

[4]These details can be well traced on Sir R. Murchison's Geol. map of Europe, constructed by A. K. Johnston, F. R. S. E., Geographer to the Queen.

Siberia, a radius of 24° follows the Urals and the curve of the Himalayas, and also skirts the mountains east of the Amoor river. This brings us probably to that period in the Mesozoic when the giant Himalayas were preparing for the grand Cenozoic rise, which subsequently gave us the Miocene of the Siwâlik Hills, on the southern flanks of the western Himalayas. A radius of 36° encloses the whole of Asia, except the peninsulas, sweeping from about the geographical north pole, through Spitzbergen, also through Scandinavia, then midway of the Caspian by the Cutch (with its grand Ullah Bund upheaval, fifty miles long, after the earthquake of 1812), past the mouth of the Irawaddy, and round to Kamchatka; finally to Behring's Straits, marking, in its circuit, a zone, mostly Tertiary.

3. *North America:* The northern initial focus is in or about Boothia Felix. From that point, with the radius of 12°, we mark various early geographical breaks or depressions; and with a radius of 24°, we reach the southern point of the Archæan V-shaped area near Lake Superior, and its junction with the Paleozoic, as laid down by Prof. Dana in his "Manual."

Other circles, from this focus, give us a zone of coal from Queen Charlotte and Vancouver Islands (some of which, Taylor, in his great work on coal, marks as of Carboniferous age, although most is Tertiary Lignite) through the coal fields of N. Iowa and Michigan to that of New Brunswick. A somewhat more extended radius of about 29° to 30° connects the Mesozoic of Kansas with the New Red Sandstone (Triassic) of Connecticut and Massachusetts, leaving a valley of erosion between.

Removing our centre to the west shore of Lake Superior, a radius of 11° to 12° gives us Silurian (Lower and Upper) from Niagara to near Springfield, Ohio, Lexington, Ky., Nashville, Tenn., dominating the geological structure in at least the eastern half of the circle, while the west was still under water. A radius of 12° to 13° marks the Appalachian and other coal fields, that of 15° is Mesozoic, curving from the Cretaceous of Utah and Colorado, through the intermediate Greensand (Lower Cretaceous) of Arkansas and Tennessee to that of New Jersey. A radius of 24° outlines the continent proper from Cape Breton and Cape Sable to the Golden Gates; while with from 18° to 24°, we pass through the marine Tertiary of Nevada, California, northern Mexico, Texas, Louisiana, Mississippi, Alabama, Florida, Georgia, South Carolina, North Carolina, Maryland and New Jersey to

Martha's Vineyard and Barnstable (Mass.). The circle of 36° embraces Yucatan and Honduras; and, reaching nearly to Lake Nicaragua, it encloses several islands near our Pacific coast, besides taking in part of Alaska and Greenland. The northwest peninsula of North America and the northeast peninsula of Asia appear to belong rather to northern Oceanica.

4. *Africa:* Regarding this continent, suffice it to remark that a radius of 24° extends from the Tropic of Cancer to the Tropic of Capricorn, passing between Lakes Albert and Victoria, Nyanza, over Mt. Mfumhiro (10,000 ft. high) and Mt. Omatako (8,800 ft. high). A radius of 36° not only defines the continent, passing through the Straits of Gibraltar, Mediterranean and Red Seas, Mozambique Channel, aiding to separate Madagascar from Africa, or to elevate it at a later period; thence round the Cape of Good Hope, enclosing the islands of St. Helena, Ascension, St. Paul, Capes Verde and Canary, but also passes between Madeira and Porto Santo (where Lyell found a *continental* difference in the mollusks, see Principles, II Vol., p. 427) and reaches the place of beginning. Much of the belt passes through Cenozoic regions and excludes the Quaternary of Madagascar, with its gigantic Æpyornis, just as a similar radius in Australasia, excludes the Quaternary of New Zealand, with its Dinornis, etc.

It would occupy too much space to go into details regarding South America, Australasia and Oceanica; but as far as examined, the law applies there also.

IV. *Fourth subdivision of the law:* Besides these three modifying influences, toward the close of the Mesozoic and beginning of the Cenozoic, the western Alps became a dynamic focus, reaching, according to Elie de Beaumont, their present height during the Miocene Period. Mount Rosa is nearly, if not quite, the geographical centre of the entire dry land on the globe; and the Alps connect with the Himalayas and Andes, of similar geological age, by a great circle or belt of immense seismic activity, viz., along the Carpathians and flanks of the Caucasus, near the disturbed regions of the Caspian, through Persia to the southern slopes of the Himalayas; thence through the volcanic islands of the Bay of Bengal, the numerous volcanoes of Sumatra and Java, near Sumbawa's earthquake region to that of New Zealand. This belt or zone, on the opposite side of the globe, reaches that portion of the Andes, where almost daily rumblings are heard (about Chimborazo); thence, crossing the earthquake region of Vene-

zuela, marks the Bitumen Lake of Trinidad, and many active volcanoes of other West India Islands, as well as of the Azores.

Demonstration: A radius of 9° from Mt. Rosa defines accurately the Miocene Tertiary (as given on Murchison's map) on the east coast of England, also in the middle of Denmark; through Prussia it is Eocene; but Miocene again through Austria, S. Italy, Sicily, Algiers and vertically central Spain. With a radius of 12°, we may follow the force which probably aided in the separation of Ireland from England, and which is indicated by other important breaks or depressions. With a radius of 36° we pass, from the Miocene on the eastern flanks of the Urals, to that of Spitzbergen and of Greenland. Contracting this radius somewhat, we follow the Carboniferous and Permian of Russia also to Spitzbergen and Greenland. The great circle, pointed out in the law, as passing from the Alps to the Himalayas and Andes, marks chiefly tertiary regions.

Summary: The dynamics of land-forming would seem, from the foregoing demonstrations, to comprise first, a longitudinal force, scarcely, if at all, seismic, adding to continents chiefly by aqueous depositions, inasmuch as each northern continent, near the termination of the median line, has a large river delta. Secondly, there is an arctic-antarctic force mostly along continental coast lines, and connected with active seismic phenomena of alternate elevations and depressions: apparently from these being thinner or more yielding portions of the earth's crust than at continental elongations. Thirdly, in each continent there are radii and circles, connected with one or two important foci, which have aided in defining the geographical limits of each continent, and in bringing geological deposits, in successive curves of increase to, or near the surface: possibly because the wave impulse, directly under the upheaved plastic focus, sends its molten contents to equidistant circles, beneath the plastic crust.

The geology of each continent, but especially Europe, has also been somewhat modified by the Alpine central focus of the dry land hemisphere.

As corollaries, attention may be called to two additional great circles of activity, which are secondaries and great circles of illumination to those phases of the ecliptic which would present themselves, if we arrest the globe, respectively at the summer and winter solstice, when the meridian of Mount Rosa (the centre of

land) marks noon on June 21 and December 21. This gives us one great circle from Behring's Straits to its antipodal antarctic, due south of Mount Rosa; almost defining the junction between the land and water hemispheres; the other from Scandinavia, at the Arctic circle, to the antipodal point, on the Antarctic, which will be found due south from Behring's Straits. As these run through the northern hemisphere, the course of the more active one is from the volcanoes of Sumatra, nearly parallel to the Asiatic continental trend, formerly described, thus skirting the Japan Gulf Stream; thence they pass again, nearly parallel to the west coast trend, through North and South America; whereby a region or zone is marked of Nevada geysers, Arkansas and Virginia Hot Springs, Yucatan, Cuban, Venezuelan, Grenadan, Peruvian and Chilian volcanic and earthquake regions. The course of the other, while running nearly parallel to the North American east coast trend, is from near the thirty-nine volcanoes of Central America to the geysers and volcanoes of Iceland, thus skirting, and probably aiding to heat, our Gulf Stream, while on the opposite side of the globe, this zone or belt passes near the Hindoo Kush and western Himalaya elevations, the disturbed regions of Hindostan and islands in the Bay of Bengal (some brought up within the historical period) as well as the numerous volcanoes of Sumatra.

As corollary of the law that, as one continent comes up, there is usually a depression on the opposite side of the globe, let us follow the eastern Himalayas, north through the high Asiatic Table-lands and observe on the opposite side the depressions of Hudson's Bay, our fresh-water lakes, the Gulf of Mexico and the deep south-east Pacific, devoid of islands. May not this great depth of ocean, unobstructed by continents on the west, enable the moon to create here the initial, tidal wave, by raising the water in this part of the ocean, somewhat above the general level, chiefly to follow her apparent path west, but also to send a smaller wave, obstructed partly by the coast of Patagonia, to extend round Cape Horn and there meet the main west coming tide?

The evident connection of these laws with Terrestrial Magnetism, with Mineralogy and economic Geology, Archæology and Ethnology, I leave for others to elaborate; or possibly, if this paper is considered as furnishing sufficient demonstrations to point to a law, further elucidations bearing on these collateral branches may be attempted on my part, should health be spared.

On Maximum Synchronous Glaciation.

By W. J. McGee, of Farley, Iowa.

INTRODUCTION AND SYNOPSIS.

The present paper may be considered as a review of what is known as the "ice-cap theory." It is true that this theory, as originally propounded by Adhemar, has been virtually abandoned; but modifications of essentially the same theory are yet advocated by some of the ablest geologists. Some analysis of the underlying principles may not, therefore, be deemed inappropriate.

In Part I the agencies which effect glaciation are briefly outlined; and it is shown from the mode of operation of these agencies that precipitation over the central portions of any extensive ice-field must be very slight or even *nil*. Applying the same principles to the polar zones of the earth, it is shown that under no conceivable circumstances could ice accumulate very thickly around the poles. A glance at the present condition and past history (so far as known) of the polar regions, then indicates that there is no sufficient reason for believing that the polar regions were ever much more extensively glaciated than at present.

Part II is designed principally to show approximately (1) the relative temperature of all parts of the earth when the ice of a glacial epoch extends to any latitude; (2) the greatest possible accumulation of ice at any latitude; and (3) that the presence of an ice-field at any parallel is inimical to precipitation at any higher latitude. For the sake of simplicity it is, in three of the sections of this Part, assumed that the climatal and meteorological conditions are identical on all parts of the same parallel; or, what is equivalent, that the isotherms and isobars are parallel and separated by constantly uniform distances. Such a condition would be much more favorable to extensive glaciation than that which actually obtains. The influence which would be exerted on the earth's centre of gravity by such an ice-cap as that of which the dimensions are calculated has accordingly not been computed, as it is undoubtedly many times too large. Lest the correctness of the figures representing temperatures under varying conditions should be questioned, an additional section (Sec. III), showing

that the results are in perfect harmony with known meteorological phenomena, is appended.

In Part III the nature and cause of ice-motion are discussed, with the object of showing that the assumption of a vast polar ice-cap to force outward the peripheral portion of the ice is unnecessary and incompetent. The viscous theory of ice-motion is adopted, with some modifications; and some of the most serious objections to the theory are considered. As this part is to an extent speculative, it may be stated that the general conclusions reached are independent of it, and will stand even if the viscous theory of ice-motion be shown to be incorrect.

PART I.

SEC. I. SUMMARY OF PRINCIPLES.

1. Conditions of the problem.

Heat is the first essential.—The researches of recent years have shown that a diminution of the temperature of the earth, *as a whole*, could not produce glaciation. As shown by Tyndall, every pound of ice piled up on land represents an amount of heat sufficient to melt five pounds of cast-iron; [1] and the true difficulty lies in accounting for the heat stored up in the ice of the glacial epoch. But heat alone will not effect glaciation.

Necessity for a condenser.—"It is quite manifest that the thing most needed to produce the glaciers is an *improved condenser;* we cannot afford to lose an iota of solar action; we need, if anything, more vapor, but we need a condenser so powerful, that this vapor, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow. The problem, I think, is thus narrowed to the precise issue on which its solution depends."[2]

Evaporation and precipitation do not occur together.—Certain meteorological conditions accompany profuse evaporation, while precisely opposite conditions result from rapid condensation. In the first case the air is necessarily humid; and as the presence of aqueous vapor in the atmosphere checks radiation from the earth and from the lower atmospheric strata, the earth's surface and the

[1] "Heat as a Mode of Motion," Am. ed., p. 176; "Forms of Water," p. 153.
[2] "Heat" etc., p. 176.

surrounding air are warmed. Above a region of sufficiently low temperature to condense the aqueous vapor in the atmosphere the air is necessarily dry, and is hence diathermous. The solar heat is consequently radiated away directly, without raising the temperature of the region. If from other reasons the air is rendered dry over any region, it will be lowered in temperature; for each particle of aqueous vapor absorbs heat 16,000 times as rapidly as a like particle of air.[3] At the same time the temperature will be rendered variable, as is the solar intensity.

It follows that profuse evaporation and complete condensation cannot take place over the same region at the same time; and it equally follows that while the regions which furnish and those which condense vapor may be contiguous, they must be quite distinct. This will be incidentally demonstrated later.

2. Character of required condenser.

Material which meets requirements.—There is probably no other substance than water in the solid state which will form a condenser sufficiently powerful to meet the requirements of the problem. Ice preserves a temperature of 32° at the highest, at which temperature air is practically dry (see table I and diagram I); and to elevate its temperature one degree requires as much heat as would raise an equal mass of iron 1430 degrees. Then, when the sun is obscured or below the horizon, the stored-up heat is rapidly poured forth and dissipated. Such a condenser operates in a two-fold manner: (1) It cools the air by direct contact, and by absorbing all heat radiated upon it by the atmosphere; and (2) it dries the air by lowering its temperature and thus precipitating its moisture; and dry air is but slightly affected by the solar rays.

Now such a condenser must so far exceed in capacity any tax that may ever be placed upon it that it will *immediately* condense and congeal all moisture that may be brought to it by aërial currents; for if the vapor is not immediately condensed it will cut off radiation from the ice below, and thus accelerate melting; and if the vapor is only condensed but not congealed it will fall as rain, and every pound of it will melt 143 pounds of ice before it is itself frozen. *The integrity of the condenser hence depends on its capacity being far in excess of the work it may be called upon to perform.*

[3] *Ibid.*, p. 346.

Form assumed by any condenser.—The principles just mentioned may be applied to an assumed condenser consisting of an ice-sheet 1200 or 1400 miles in diameter, and situated on any part of the globe. The ice and the air above it would be cold, while the surrounding surface and atmosphere would retain (approximately) their normal temperature. Cold and dry winds would blow from the interior of the ice-sheet outward, while warm, vapor-laden winds analogous to sea-breezes, usually in the form of upper currents, would blow toward the ice. Now, if the quantity of vapor borne by the latter currents were excessive, the outer portions of the ice would not be able immediately to condense and congeal it. Hence radiation would be checked, the temperatures of the air elevated, and the moisture precipitated as rain. *But if the vapor were precipitated as rain, the margin of the ice-sheet would be thereby melted.* If this continued, the ice-sheet would eventually be destroyed. On the other hand, if the vapor were not excessive, all of it would be precipitated near the periphery of the condenser, *and none of it would reach the interior.* For if the vapor-laden air were borne inward at the rate of twenty miles per hour (which is a high estimate) it would require thirty or thirty-five hours for it to reach the centre of the ice-field. Now it is quite impossible that a thin vaporous stratum could so long resist refrigeration, surrounded as it would be by air of 32° or less, and continually retarded as it would be also by a cold counter-current; for every particle of vapor would lose its heat 16,000 times as rapidly as a like particle of air. If the stratum of humid air were thick, the heat radiated from its central portions would prevent the congelation of the outer portions, and the heat given out by the particles first condensed would prevent the congelation of neighboring particles; and moreover radiation from below would be completely cut off. But this again would destroy the subjacent ice-field. If the capacity of the condenser exceeded the tax put upon it, *as is absolutely essential*, little if any vapor could be carried far within its borders. Even on ordinary land-surfaces of considerable extent the normal precipitation of rain is much greater along the coast than inland, as well shown in New South Wales by H. C. Russell (Rain and River Obs. in N. S. W. during 1879. Sydney, 1880).

It follows that on any permanent condenser the congealed moisture will not accumulate uniformly over the whole surface, but in the

form of an annular belt thickest at the margin and more tenuous as the centre is approached, as shown in the accompanying figure

representing a section through the centre of such a condenser as has been assumed. Two causes would, however, modify the form of the accumulated mass of ice: (1) That property which enables ice to flow as do viscous bodies (and which has long since been shown to be largely dependent on the rate at which heat is received) would permit the attenuation of the peripheral portion. (2) It is inconceivable that the equilibrium should be so nicely adjusted that rain would not sometimes fall upon the marginal portion of the ice-field, and remove some of the ice. The outer portion would hence assume some such form as that indicated by the dotted lines in the figure. The highest point of the annular belt would, in any given case, be difficult to determine otherwise than empirically; but it would probably be many miles within the periphery.

The only reliable observations on the slope of the upper surface of an ice-field, now at hand, confirm the conclusion reached. Dr. Hayes, in exploring the Greenland ice-field, found that within the first thirty miles from the outer limit of the ice-sheet proper, the slope gradually diminished from 6° to 2°;[4] and at forty-five miles from the margin, Lieut. Jensen found the slope to be only 0° 49′.[5]

Necessity of vapor.—But the presence of a powerful condenser will not alone produce glaciation; ample (though not excessive) quantities of vapor are equally essential. It is known that glaciers extend in wet seasons and contract in dry ones, and that of two mountains in the same latitude the snow-line sinks lowest on that which has the greatest precipitation; also that the glaciers extend lowest on that side of a mountain range (as in the Himalayas, the Rocky Mountains, and the Southern Alps of New Zealand) which faces the vapor-source. Professor Dana

[4] "Open Polar Sea," p. 134. Cited by Dr. Croll, "Climate and Time," Am. ed. p. 379.

[5] *Am. Jour. Sci.*, [3] XIX, 417.

has shown in the *American Journal of Science* that the thickness of the American quaternary ice-sheet corresponded in a general way with the present rainfall, and that where the precipitation is very slight the ice did not accumulate, as in the western plains and in the Wisconsin driftless region. The absence of glacial indications in many arctic regions, in northern-central Asia, and in other localities, is undoubtedly due to the same cause. It is solely the necessity for vapor, indeed, that prompts Professor Tyndall and others who have intelligently studied glacial physics, to conclude that a diminution of terrestrial temperature could never produce glaciation. But, as just shown, excess of vapor is as fatal to the accumulation of ice as an insufficient supply. If an ice-sheet is decreasing, we may be certain that it is in consequence either of an over-supply or an under-supply of moisture; but we cannot *a priori* determine which.

SEC. II. PRESENT AND PAST CONDITION OF POLAR REGIONS.

1. Arctic Regions.

Present condition.—Properly speaking, continental glaciation is not now in progress in arctic regions, though there are extensive local glaciers in Greenland which serve to convey an inadequate idea of the immensity of the prehistoric ice-sheet. Precipitation is in excess of evaporation in the north-frigid zone, just as evaporation is in excess in the torrid zone. Nearly all the vapor precipitated in northern polar regions must indeed be brought in from lower latitudes. Local evaporation must be insignificant, since the temperature is low, and since a great proportion of the surface consists of dry land. But only an insignificant portion of the moisture borne northward by the warm upper currents ever reaches the polar zone, as, owing to their position, the heat conveyed by these currents is rapidly dissipated into interstellar space, and their vapor precipitated; so that when the upper current finally descends, the air is dry. Precipitation is therefore limited. Dr. Weikoff says[6]:—

[6] Discussion and analysis of Professor Coffin's "Winds of the Globe," *Smithsonian Contributions to Knowledge*, Vol. XX, p. 680. For the use of this volume the writer is indebted to the Hon. Secretary of the Smithsonian Institute, Prof. S. F. Baird.

"The constancy of the polar current in Northern Greenland is indirectly proved by the small precipitation of rain and snow. The quantity of snow falling at Polaris Bay and Lifeboat Cove was scarcely measurable, according to Dr. Bessels. He thinks the glaciers of Northern Greenland are the remnant of a former age, when the climate was different. The snow and ice that melt every summer are not now replaced by new snow, so that the glaciers must be decreasing."

It is probable, though, that during some seasons the precipitation is in excess of the melting. No one can doubt, for instance, that precipitation was much more abundant over and inland from the open polar sea seen by Hayes than over and around the paleocrystic sea seen in the same place by Sir George Nares. There is at any rate no evidence that the climate has been more favorable for glaciation than now within recent times.

The fact that reindeer cross the great Humboldt glacier every season has led to a belief that the climate of Northern Greenland is actually less inhospitable than that of the central part of the same island. Such a belief was indicated, with some of the grounds on which it is based, by the President of the Royal Society in his annual address for 1877. If there is such a habitable region near the pole it must be due to the precipitation of nearly all vapor at lower latitudes, which prevents the accumulation of ice. As has been shown, either lack of vapor or excess of vapor may prevent the accumulation of ice; but it is not excess of vapor that prevents accumulation in arctic regions. Though the temperature is quite low enough to precipitate nearly all moisture which the air can possess, there is very little precipitation; and moreover the greater part of the small precipitation is not rain but snow—a fact which furnishes *prima facie* evidence that the condensing capacity of the ice-covered regions is not overtaxed.

Past condition.—But the arctic regions are now about as favorably situated for receiving moisture from more southerly latitudes, or from any other source, as they ever have been, unless (1), the temperature has been elevated by the diminution of ice, or (2), the distribution of sea and land has been materially different from the present. The first of these possibilities does not affect the question at issue; and it has not been shown from geological evi-

dence that the distribution of sea and land was very greatly different, during the glacial epoch, from the present. There are now no equatorially-situated ice-fields to cut off the vapor supply, nor any other influence, aside from the gradual diminution of temperature toward the poles, to prevent the accumulation of ice in northern regions. Both aërial and aqueous currents tend to reduce the difference between polar and equatorial temperatures; and these currents must be about as efficient now as they ever were. Dr. Croll calculates that the Gulf-stream conveys nearly one-half as much heat to the arctic regions as is received directly from the sun by the same regions.[7] The Kuro Siwo similarly transfers a portion of the heat of the tropics to higher latitudes, and materially modifies the climate of northwestern North America;—the mean temperature of Sitka being higher than that of St. Louis,[8] though the former is more than eighteen degrees farther north than the latter. The effectiveness of these and other vehicles of warmth and moisture was probably never much greater than to-day. Hence if now, under the present favorable conditions, the arctic regions do not receive sufficient moisture to form a continuous polar ice-field, it may reasonably be doubted whether they ever did.

Furthermore, the geological evidence of extensive glaciation in arctic regions is either wanting, or, at the least, ambiguous and equivocal. Thus, Dall not only found bones of Pliocene mammals lying undisturbed and unbroken upon the surface in Alaska, but (though an accomplished geologist) failed to find the slightest evidence of glacial action.[9] The bones of Pliocene animals so abundantly found in northern Asia seem to be destitute of glacial markings, and are found in such positions as they would not be likely to occupy if glaciation had taken place since their accumulation. The geological age of the tree found by Sir Edward Belcher in lat. 75° 32′ N., long. 92° W.[10] is doubtful; but there seems to be some reason to believe that its age is that of the "forest bed" of both sides of the Atlantic.[11] If this be so (as

[7] "Climate and Time," p. 27.

[8] Dr. Weikoff, *op. cit.*, p. 688.

[9] Whymper's "Travels in Alaska." Appendix.

[10] "The last of the Arctic Voyages," Vol. I, p. 381. "British Association Report for 1855," p. 381. Cited by Croll, "Climate and Time," p. 265.

[11] Dr. Newberry, "Geological Survey of Ohio," 1874, Vol. II, pt. I, p. 3. Author, *Am. Jour. Sci.*, Vol. XVI, p. 339; also *Proceedings of the American Association*, Vol. XXVII. p. 198.

Dr. Croll also seems to consider), glaciation must have taken place in north-temperate countries since the tree reached maturity; but its presence furnishes the best possible evidence that the glaciation did not extend to that high latitude. As shown by the Rev. Prof. S. Haughton, from the geological observations of McClintock's voyage, the bowlders of the arctic archipelago were carried northward;[12] and it was long ago pointed out by Sir Charles Lyell that Scandinavia, Scotland, Labrador, and other elevated regions formed independent centres of dispersion of erratics during the glacial period,[13] as could not happen if a polar ice-cap occupied higher latitudes. During any glacial period, indeed, the ice in temperate regions would cut off the supply of moisture previously borne northward, in accordance with the principles which tend to prevent the accumulation of ice in the central portions of any extensive ice-field. It is accordingly extremely doubtful whether the arctic regions were ever much more extensively glaciated than now.

2. Antarctic regions.

Present condition.—Regarding the character of the antarctic continent, but little has been positively determined. "It seems, so far as is at present known, that the whole of the antarctic land, low and high, as well as the ice-cap of which a portion of the continuous continent may consist, is bordered to some distance by a fringe of ice, which is bounded to seaward by a perpendicular ice-cliff, averaging 230 feet in height above the sea-level. Outside the cliff a *floe*, which attains near the barrier a thickness of about twenty feet, and in some places by piling a considerably greater thickness, extends northward in winter to a distance varying according to its position with reference to the southward trending branches of the equatorial current; and this floe is replaced in summer by a heavy drifting pack, with scattered icebergs."[14]

The ice extends much farther from the pole than in the northern hemisphere, and forms what appears to be a true continental

[12] "In the Arctic Seas," Philadelphia (author's edition), Appendix, p. 368.

[13] See "Antiquity of Man," last edition.

[14] Sir Wyville Thomson, in address before the Geog. Section of the British Association, Dublin meeting. Brit. Assoc. Rep., 1878, p. 619. *Nature*, Aug. 22, 1878. Copied from *Am. Jour. Sci.*, Vol. XVI, No. XCV, p. 356.

glacier averaging nearly 2200 miles in diameter, and covering at least the peripheral portion of an area of about 4,700,000 square miles, according to Sir Wyville Thomson. Aside from all extra-telluric agencies (which seem to be necessary for the inauguration of glacial epochs proper, but which do not in any way affect the question under consideration), it is manifest, in accordance with glacial physics, that the considerable accumulation of ice here is due to the great quantities of moisture borne thither by winds and precipitated as snow upon the vast ice-masses already formed. But the ice has not reached the greatest possible thickness; for it falls far short of that attained by the quaternary ice-sheet in North America. As in the northern hemisphere, too, the precipitation is chiefly of snow. Therefore it must be only lack of sufficient quantities of vapor that prevents the accumulation of still greater masses of ice.

There is a remarkably constant and uniform belt of low barometric pressure extending around the globe at about S. lat. 65°, as shown by Buchan. Unlike the regions of low barometer in other portions of the globe, this belt remains constant in spite of the fluctuations of the seasons. To this region of low pressure, which corresponds roughly with the antarctic glacier-margin, and is undoubtedly largely due to its presence, the winds blow regularly from both north and south. The northerly winds bear thither the vapor from temperate oceans, and it is precipitated on the peripheral ice-belt. If any vapor is formed within, over the interior of the ice-region, it is borne outward and deposited with that brought from the opposite direction; and this polar current, refrigerated by contact with the ice, tends to check the bringing in of vapor, and aids also in its congelation.[15]

Now we cannot imagine a condenser in high latitudes to be more favorably situated for receiving moisture than is the antarctic ice-field, surrounded on all sides as it is with warmer seas, and consectaneous as its margin is with the median-line between two of the wind-systems of the globe, which bear thither and pile up in snow all the moisture with which they are charged. Yet here we find a glacier not over 1400 feet in thickness at the margin,

[15] The constancy and strength of these currents are imperfectly shown in Coffin's "Winds of the Globe." They are just such winds as would be formed by the presence of an ice-field on any part of the globe.

and rising with a gentle slope as far as the eye can reach toward the interior — perhaps half a degree, and certainly not much more than a degree in any case. It has been supposed that the upward slope continues to the pole; but this seems to be a wholly gratuitous assumption, which is disproved not only by the known meteorological laws already detailed, but by the observation of explorers. Sir Wyville Thomson, whose opportunities for investigating this matter were unrivalled, believes that the ice is not even continuous within the outer circumscribing belt.

Past condition.—We have no means of ascertaining the past history of the antarctic continent from the geological record; but it is quite certain that it never could have been more favorably situated for glaciation than to-day; and hence, unless meteorological laws were widely different in the past from those now obtaining, that it never could have been much more deeply covered with ice than now. If the antarctic ice-cap ever extended so far as to spread over parts of Africa and South America, the precipitation of moisture must have taken place along the outer margin of the ice, and none could have been carried within it to the latitudes now bounding the southern ice-cap.

Conclusion.

The foregoing statement of principles and review of facts warrant the provisional conclusion that *the polar regions are about as extensively glaciated to-day as they ever can have been.*

PART II.

SEC. I.—CLIMATAL EFFECT OF AN ICE-CAP EXTENDING TO ANY LATITUDE.

1. Distribution of heat will be stopped.

Marine currents.—An immense amount of heat is annually carried within the arctic and antarctic circles by ocean currents; and the climate of all northern lands is very considerably ameliorated through the influence of these vehicles of heat, as admirably shown by Croll;—indeed this physicist insists, and apparently with good reason, that if oceanic circulation were stopped, the climate of the globe would be so changed that it would no longer

be habitable for existing orders of beings. Now it is manifest that if a continuous ice-cap extended to any distance from the pole, marine currents would be inoperative anywhere within its outer margin. The stoppage of this heat-distributing agency would materially increase the difference in temperature of equator and poles.

Aërial currents.—Winds, especially when vapor-laden, play an important part in the distribution of the equatorial heat over the surface of the globe; and as they are operative alike over the whole of the terrestrial surface, it is probable that their effect is approximately commensurate with that of marine currents. But, as intimated in Part I, if an ice-cap extended to any latitude, the winds from without would blow only to its margin, where they would be met by a cold counter-current from the interior, just as is now the case in the antarctic ice-field. In arctic regions too, as stated by Dr. Weikoff, high barometric pressure coincides with low temperature. This gives rise to a gentle but constant polar current; or, when the pressure is less, simply prevents the in-blowing of equatorial winds. The same eminent meteorologist states that the proportion of calms increases with the augmentation of barometric pressure and diminution of temperature. "Arctic America," he says, "is noted for its frequent calms in the colder part of the year—a feature observed by nearly all who have wintered in these regions."[16] Dr. Bessels found that at Lifeboat cove during the winter of 1872-3 there were, on an average for seven months, 140 hours out of 1,000 during which "a self-registering wind-vane did not indicate any movement of air whatever."[17] "Parry and Barrow believed that a perfect calm exists at the pole." It is accordingly safe to assume that the winds on any extensive polar ice-cap would consist of gentle polar under-currents, increasing in force toward the margin of the ice, and equally gentle upper currents of dry air moving toward the interior.

Elimination of aqueous vapor.—The efficiency of aërial circulation in distributing the tropical heat over the surface of the globe is mainly dependent on the aqueous vapor intermingled with the air. But the vapor would be practically eliminated from the air over an ice-field in consequence of the low temperature which it would be forced to assume. If the vapor were not eliminated, the ice would

[16] Discussion and analysis of Coffin's "Winds of the Globe," *Smithsonian Contributions*, Vol. XX, p. 677.

[17] *Ibid.*

be melted, as already shown. The air being rendered dry and hence diathermous, the aërial currents which continued to exist over the ice-field would be practically inefficacious in distributing the heat derived from the sun.

Throughout this paper it will be assumed that (*a*) the amount of vapor in the atmosphere, (*b*) the precipitation at any latitude, and (*c*) the efficiency of aërial currents as heat carriers, are proportional to the temperature. One object in so doing is to keep all conclusions well within bounds. Actually the vapor-tension, or elastic force of vapor, decreases in a decreasing ratio with the temperature, as shown in the following table and diagram. "The elastic force may be regarded as representing approximately the absolute quantity of vapor suspended in the atmosphere."[18] Precipitation rarely occurs except when the point of saturation, or dew-point, is exceeded. But the atmosphere is less frequently saturated in polar than in equatorial regions, as attested by the less frequent precipitation. As already shown (Part I, Sec. II), the precipitation in arctic regions is almost *nil* in winter, and very slight in summer. Even the table and diagram, therefore, present too favorable a showing. On the other hand, however, the heat-absorbing and conveying power of perfectly dry air will be disregarded. This will be more than counterbalanced by the former allowance.

TABLE I.

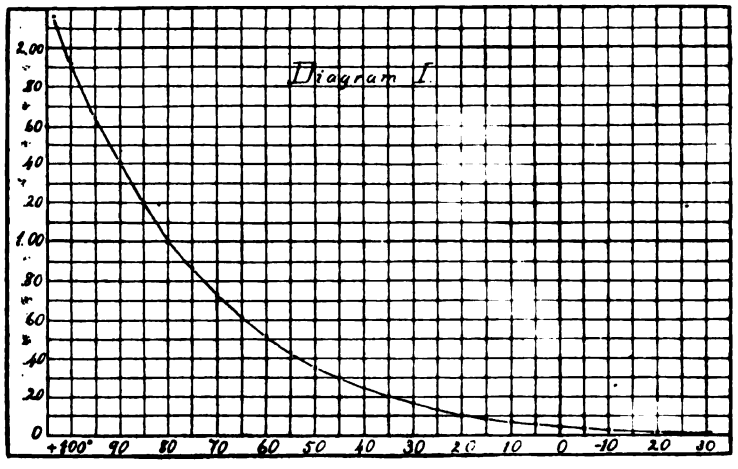
Vapor-tension at various temperatures.

Temperature.	Vapor-tension.	Temperature.	Vapor-tension.	Temperature.	Vapor-tension.	Temperature.	Vapor-tension.
-31° F.	0.0087	+5°	0.0545	+45°	0.2993	+85°	1.2031
30	92	10	684	50	3606	90	1.4097
25	124	15	857	55	4331	95	1.6468
20	163	20	1078	60	5176	100	1.9179
15	210	25	1347	65	6173	104	2.1617
10	270	30	1668	70	7329		
5	343	35	2037	75	8676		
0	434	40	2476	80	1.0232		

Table I is condensed from table VI, of class II (Hygromet-

[18] A. Buchan on "Atmosphere," in Encyc. Brit., 9th ed., vol. III, p. 32.

rical Tables) in *Smithsonian Miscellaneous Collections;* by Arnold Guyot, 1854. That table exhibits the "Elastic force of aqueous vapor, expressed in English inches of mercury for temperatures of Fahrenheit. Reduced from Regnault's table." Diagram I exhibits the relation graphically; inches of mercury being represented by abscissæ, and temperatures by ordinates.



Conclusion.—In view of the foregoing considerations it seems safe to conclude that the ordinary and most efficient heat-distributing agencies would be wholly inoperative over any extensive ice-field.

2. Direct loss of heat will be augmented.

Increased radiation.—The earth dissipates energy in the form of heat as rapidly as it is supplied by the sun and the stars; but its physical constitution is such that normally the solar heat is not *immediately* radiated away. In consequence, the exterior portions of the earth are considerably elevated in temperature. This storing up and utilization of the solar energy are mainly accomplished by the protecting envelope of aqueous vapor which surrounds the greater portion of the earth. When the air is dry (and consequently practically diathermous, as shown by Tyndall—"Heat," etc., p. 346) the solar heat, though pouring upon the earth's surface with undiminished fervor, is just as rapidly thrown back upon the interstellar ether and lost to the earth. Thus in Sahara, though the heat of the sun is painful, the

nights are so cold, in consequence of the unchecked radiation, that ice sometimes forms over exposed water-surfaces; and the daily range of the thermometer is so great (sometimes 70° or more) as to cause serious apprehension in the minds of the engineers of a proposed railway that the resulting expansion and contraction of the rails would be fatal to stability of the track.[19] In south-central Africa, Dr. Livingstone found the thermometrical range from sunrise to noon to be 48°.[20] In central Australia, Captain Sturt found the range of temperature from sunrise to mid-day to reach sometimes 53° or even 56°; and in one case the thermometer varied 72° within twenty-four hours.[21] These figures show that where the atmosphere is diathermous from any cause, the temperature varies much more nearly as the solar intensity than where the air is humid. In arctic regions the same tendency is very strikingly manifested. Pitch, on the sides of a vessel, has been seen to melt by solar action, while the surrounding air was below the freezing-point;[22] and the difference between the reading of a thermometer exposed to the solar rays and the actual temperature of the air has been found to be as high as 80°.

Snow- and ice-covered surfaces dissipate heat much more rapidly than land-surfaces equally destitute of vaporous covering. "Of the numerous wonderful properties of water, not the least important is this extraordinary power which it possesses, *in all its states of aggregation*, of discharging the motion of heat upon the interstellar ether."[23] During the intense cold following snowstorms in the northwestern states, when the mercury is far below zero or perhaps solidified, the air is so nearly free from vapor that the blinding glare of the sun is painful; yet so perfect is the radiation that the solar rays may pour upon freshly-fallen snow sheltered from the wind for the whole day without perceptibly softening it. The air may not be abnormally dry at such times, either, but may be fully saturated, as attested by solar parhelia, and by a constant shower of minute scales and spiculæ of ice, which can be seen glistening in the sun as high as the eye can reach. Besides radiating so readily while exposed to solar action, ice absorbs heat without becoming sensibly warmed thereby; and when the sun is below

[19] *Scientific American*, vol. XXXIX, p. 373.
[20] Cited by Tyndall, "Heat," etc., p. 348.
[21] *Op. cit.*, 349.
[22] Scoresby, "Arctic Regions," vol. II, p. 379, cited by Croll, "Climate and Time," p. 58.
[23] Tyndall, "Heat," etc., p. 340.

the horizon the heat so stored is radiated away with equal facility. Indeed, it will be seen from a subsequent table that it is extremely doubtful, if the earth were blanketed with ice, and its own proper heat cut off, whether melting would ever take place.

It will be shown in Sec. III that any increase in annual or diurnal thermometrical range is accompanied by a diminution of mean temperature.

Reflection from snow-covered surfaces.—Reliable data as to the exact proportion of incident heat reflected from snow- and ice-covered surfaces are not at hand; but both reflect light effectively; and as the law of the reflection of heat is "precisely the same as that of light,"[24] it seems probable that heat also is reflected with equal facility. Franklin's well-known experiment, of placing differently colored pieces of cloth on snow and noting the melting under each in order to determine the heat-absorbing capacity of the several colors, would also equally exhibit the radiating and reflecting power of each as compared with that of snow, if measurements were properly made. That earth drifted from ploughed fields during wind-storms greatly facilitates the melting of the snowbanks, over which it is spread, is known to every resident of northern United States; Lieut. Weyprecht found that a heap of ashes or of the excreta of bears caused the melting of spots in the field of ice which enclosed the *Tegetthoff*, during the late Austro-Hungarian expedition, and that sometimes these materials sank entirely through the ice;[25] a serrate or lobate leaf will sink an inch below the surface of an ice-covered pond, while the melting of the surrounding ice is so slight that the cavity retains the precise outline of the leaf; and we are even told in text-books that in certain mountainous parts of northern Europe the farmers spread soot and black earth over the snow covering the fields in spring to hasten its melting. In each of these cases radiation and reflection coördinately produce the observed effect; but just what part is to be attributed to each is difficult to determine.

Pending the results of certain experiments which have been instituted to determine the actual proportion of heat radiated from ice and snow, it may be assumed that the ratio is 25 *per cent.;* and this allowance will be made in the tables and diagrams of succeeding sections. It seems scarcely possible that this estimate is too large; but if it is, the excess is doubtless more than counter-

[24] *Ibid.*, p. 228.

[25] "Die Metamorphosen des Polareises." See *Nature*, Oct. 16, 1879, p. 573.

balanced by the radiation, which will be disregarded. It should be borne in mind that if the storing-up of heat by its superficial portions were stopped, and the effect of interior heat eliminated, the exterior of the earth would assume the temperature of stellar space, or—239.°

Atmospheric absorption.—It is usually considered that, in consequence of the oblique passage of the solar rays, the polar atmosphere absorbs more of the solar heat than the tropical atmosphere; but it is probable, in view of that rapid sifting out of discordant rays which was observed by Melloni and clearly illustrated by Tyndall, that nearly all of the absorption not due to contained aqueous vapor takes place within the upper strata of the atmosphere. Eliminating the agency of aqueous vapor, therefore, it is probable that the difference in thickness traversed does not exercise much influence upon the heat-absorbing power of the atmosphere. But aqueous vapor is less abundant in polar than in equatorial regions. Atmospheric absorption may therefore be disregarded.

Conclusion.—In view of the considerations detailed in this section it may safely be concluded that over any continental ice-field the heat-distributing agencies which now modify terrestrial climate would be eliminated or rendered inoperative; and that the temperature of the various portions of such a region would be that due to the *direct* accession of heat from the several sources, *minus* the increased loss by direct reflection and immediate radiation.

SEC. II.—ABSOLUTE TEMPERATURE OF A HEMISPHERE BEARING AN EXTENSIVE POLAR ICE-FIELD.

1. TEMPERATURE IF AËRIAL AND AQUEOUS CIRCULATION WERE STOPPED FROM EQUATOR TO POLE.

Actual temperature of the earth.—Though published nearly thirty years ago the late Professor W. H. Dove's determinations of the temperature of the globe have not been superseded, but form the standard in use to-day. Table II exhibits the temperature of the northern hemisphere and of the whole earth, as recorded in Professor Dove's "Verbreitung der Wärme auf der Oberfläche der Erde," in degrees Reaumur and reduced to degrees Fahrenheit; and the same temperatures are graphically represented by the most nearly horizontal curve[26] in diagram II, in

[26] To avoid the possibility of error, the determinate points in all the diagrams are connected by straight lines.

which ordinates represent latitudes, and abscissæ temperatures. The method by which these figures were originally obtained will be described in Section III.

TABLE II.

Temperature of the Earth — Dove.

Latitude.	Reaumur.	Fahrenheit.	Latitude.	Reaumur.	Fahrenheit.
90°[27]	—13.2°	+ 2.3°	20°	+20.2°	+77.4°
80	—11.2	6.8	10	21.3	79.9
70	— 7.1	16.0	0	21.2	79.7
60	— 0.8	30.2	N. Hemisphere.	12.4	59.9
50	+ 4.3	41.7	S. Hemisphere.	10.9[28]	56.5
40	10.9	56.5	Earth.	11.7	58.3
30	16.8	69.8			

Heat-sources.—The temperature of the earth's surface is due to the heat derived from three several sources, viz.: (1) the sun, (2) the other stellar and planetary bodies, and (3) its own interior. That from the last named source is usually disregarded, partly because its amount has not been accurately determined;—thus, while Pouillet estimated it at 1 : 40 of the solar heat, Fourier estimated it at only 1 : 600. In a series of papers published in the Dubuque *Times* some years ago the writer endeavored to show from known physical laws, as well as from a comparison of Fourier's coëfficient with certain data furnished by McClintock with regard to the warming of the interior of a snow-hut by proper terrestrial heat, that the thermal effect of the heated interior of the earth is greater in cold than in warm regions; but while the figures obtained indicated that the heating effect in arctic America is several times greater than in Paris, the data were so imperfect as to vitiate the result. While there is undoubtedly some difference in the rate at which the interior heat is conveyed to different portions of the surface (as has indeed been recently pointed out by M. Faye and Professor Milne with regard to sea and land), it is probably so slight that this factor may safely be disregarded.

The temperature of interstellar space has been determined by

[27] Latitudes refer to northern hemisphere only.

[28] Recent observations collocated by Dr. Hann in *der Zeitschrift für Meteorologie* show that these figures are slightly inaccurate (see section III); but as nearly all the tables in this Part were computed before Dr. Hann's paper was seen, Dove's figures have not been altered.

Herschel and Pouillet,[29] to be about —239° F.; and absolute zero is, according to Tyndall,[30] 491.4° below the melting-point of ice, or —459.4°. Stellar heat, therefore, raises the temperature of the earth's surface 220°, disregarding fractions. Practically, this heat is uniformly distributed over the whole of the terrestrial surface. As pointed out by Croll,[31] the effect of the earth's atmosphere is to elevate the temperature of the earth slightly above that of space; and the presence of aqueous vapor undoubtedly tends to increase this effect. This factor also may safely be disregarded, however.

Solar energy suffices to raise the temperature of the terrestrial surface from —239° to +58° (disregarding fractions), or, absolutely, 297° on an average; but the solar heat is very unequally distributed. The relative solar intensity at the various parts of the earth's surface has been computed by Meech,[32] and is shown in the accompanying table III. The amount of heat falling upon each square foot of surface on the equator at the time of the vernal equinox was taken as 80 heat-units (which are of course purely arbitrary), and the accession at other latitudes computed accordingly.

TABLE III.

Solar Intensity at various Latitudes.

Latitude.	Intensity.	Latitude.	Intensity.
0°	81.50	50°	55.73
10	80.38	60	46.36
20	77.03	70	38.61
30	71.63	80	34.95
40	64.39	90	33.83
Mean for the whole Earth.....66.73			

The table is graphically depicted in diagram II, in which the solar intensity, reduced to temperature by the method about to be

[29] Pouillet, *Comptes Rendus*, July 9, 1838. Herschel, Meteorology, sec. 36. There can be but little doubt that the determination is too high.

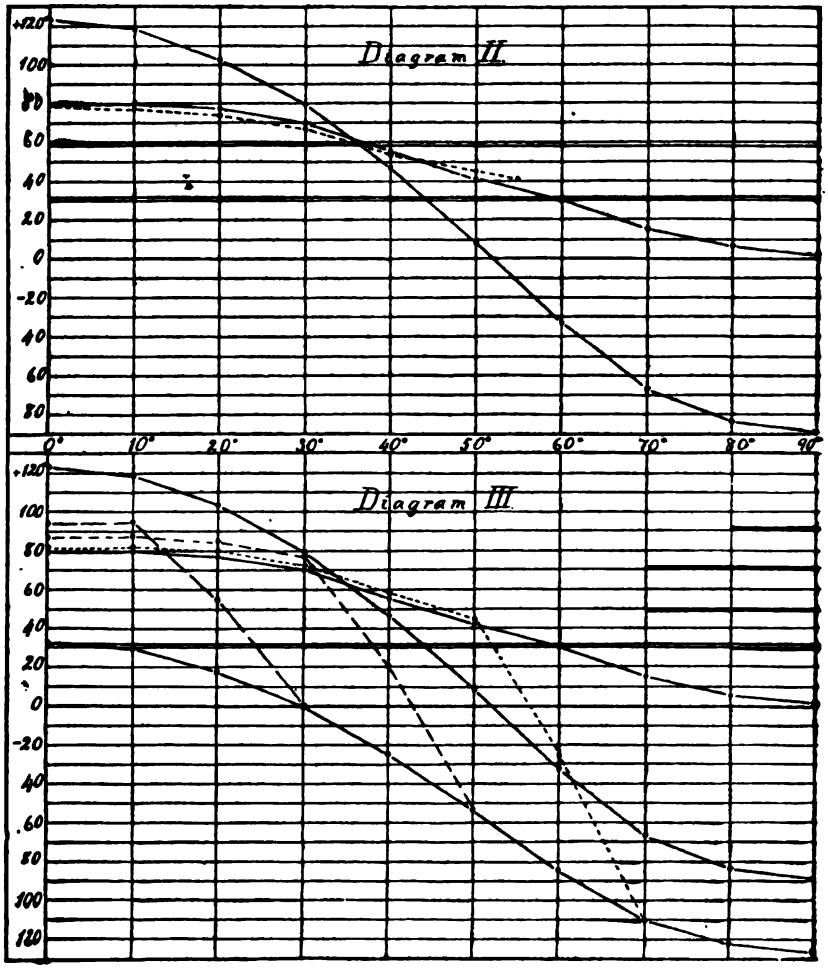
Since the above was written the writer has been informed that the question has recently been re-investigated in the Royal Observatory of Russia, and the value of —202° obtained; but the publication (*Repertorium für Meteorologie—Meteorologitcheski Zbornik*—vol. VII?) has not yet been seen.

[30] "Heat," etc., p. 72, *et passim*.

[31] "Climate and Time," p. 39.

[32] "On the Relative Intensity of the Heat and Light of the Sun." *Smithsonian Contributions to Knowledge*, vol. IX, 1855, p. 30.

detailed, is represented by the curve having the widest range. The relation to existing temperatures is numerically shown in table IV.



Temperature if proportional to direct thermal effect.—If the temperature of an ice-covered hemisphere varies as the heat received from all sources, *minus* twenty-five *per cent.* of the solar intensity, it can easily be calculated for any latitude from the simple expression $\frac{c}{a} \times (n - 25$ *per cent.*$)$, in which a represents the mean solar intensity, c the mean temperature above stellar

space, and n the intensity at the latitude whose temperature is required. Table IV has been computed accordingly; and some other figures are added. Column I gives the actual mean annual temperature of the northern hemisphere as determined by Dove, and column II, the vapor-tension corresponding to such temperatures, taken from the table already mentioned. These vapor-tensions are introduced to show how much greater is their range than that of temperatures. Column III exhibits the solar intensity, as computed by Meech; column IV represents the corresponding temperature above stellar space calculated by means of the above formula; column V, the same temperature expressed according to the Fahrenheit scale; and column VI, the corresponding vapor-tension. The three succeeding columns exhibit like values diminished by 25 *per cent.* The principal values are graphically represented by the full lines in diagram III, into which the actual temperature-curve is introduced for comparison. The relation of vapor-tension to temperature was shown in diagram I.

TABLE IV.

Temperature of an ice-covered Hemisphere.

Latitude.	I. Temperature—Dove.	II. Vapor-tension—Guyot.	III. Solar Intensity—Meech.	Undiminished. IV. Temperature above space.	V. Equivalent in deg. F.	VI. Vapor-tension.	Diminished 25 per ct. VII. Temperature above space.	VIII. Equivalent in deg. F.	IX. Vapor-tension.
0°	+79.7° F.	1.00 in.	81.50	362.67°	+123.67°	.....	272.00	+33.00°	0.188
10	79.9	1.02	80.38	357.69	118.69	.....	268.26	29.26	.162
20	77.4	.94	77.03	342.78	103.78	2.14in.	257.08	18.08	.098
30	69.8	.72	71.63	318.79	79.79	1.00	239.00	0.	.043
40	56.5	.45	64.39	286.53	47.53	.33	214.90	— 24.10	.013
50	41.7	.26	55.73	248.00	9.00	.065	186.00	— 53.00	.005[a]
60	30.2	.17	46.36	206.30	—32.70	.008	154.73	— 84.27	.002
70	16.0	.09	38.61	171.81	—67.19	.....	128.86	—110.14	.0013
80	6.8	.06	34.95	155.52	—83.48	.....	116.64	—122.36	.0010
90	2.3	.05	33.83	150.54	—88.46	.....	112.91	—126.09	.0008

The results exhibited in the above table and diagram may appear excessive; but care has been exercised to exclude all

[a] Vapor-tension below 0.013, in the last column, is estimated.

possible sources of error in the data employed. The allowance for loss through direct reflection will probably be accepted without question. This will reduce the difference between polar and equatorial temperature from 212° to 159°. It may be observed that Croll has, by an independent but probably less reliable method (indeed it is little more than an estimate), calculated that the difference in temperature of equator and poles would equal 218°, if the distribution of heat were stopped.[34] Principal Forbes' often-quoted paper on the temperature of a globe all sea or all land, published in the *Transactions of the Royal Society of Edinburgh*, will be remembered in this connection. It has not been seen by the writer.

There are two causes which may affect the correctness of the figures as obtained: (1) Conduction of equatorial heat toward the poles might exercise a slight distributing effect. This element has not been duly investigated, owing to lack of data. (2) It is the general opinion among physicists that the accepted determination of the temperature of space is too high. If this is the case the difference in temperature between equator and poles indicated in the table is too small; and the probability that it is furnishes additional reason for believing that the above figures are not excessive.

It has already been suggested (Section II, 2) that if the globe were encrusted with ice, it is extremely doubtful whether the crust would ever be removed by the action of solar and stellar heat. Inspection of the diagram will show that the lowest temperature-curve is far below the freezing-point for nearly all of its length, and rises above it at the equator by only a single degree. The question of melting hinges on that of the formation of vapor to store up and render efficient the solar energy; but, as already shown, very little vapor could exist above the ice-field — perhaps not enough to cause melting to take place.

2. Temperature when stoppage of circulation extends to any latitude.

Probable equatorial limit of polar ice-cap.—It is manifest from an inspection of diagrams II or III that a diminution of polar temperature will, by interfering with heat-distributing agencies, cause an increase in equatorial temperature. Consequently, an ice-sheet pushing out from the pole will meet not only with the simple

[34] "Climate and Time," p. 42.

resistance due to the normal increase of temperature, but also that due to the additional increase occasioned by its own presence. It is manifest, moreover, that, without the margin of the ice, the temperature and atmospheric conditions would be approximately the same as at present, and the solar heat, instead of being discharged into space, would be stored up more effectively than to-day. It is accordingly doubtful whether, under existing meteorological conditions, an ice-sheet could ever approach within a considerable distance from the equator — probably not nearer than about latitude 37°, where the undiminished temperature-curve crosses the curve of present temperature. Glaciers nearer the equator probably owe their origin to local causes; for, unless an extensive ice-field is assumed, the rapid loss of heat necessary to prevent melting near the equator, could not take place.

Equatorial temperature.—While any diminution of polar temperature will augment equatorial temperature, the increase at the equator will be less than the decrease at the poles. This necessity was recognized by Forbes, and also by Croll, who explained it by considering that the warm currents from the tropics had to warm temperate as well as polar zones.[35] It will be shown in the succeeding section, however, that the result is but a manifestation of what appears to be a general law, the nature of which is perhaps not thoroughly understood, but which may be stated as follows: — *any cause which increases the diurnal or annual thermometrical range over any region, diminishes the mean temperature of the region.* The greater decrease at the pole is hence due (in part, at least) to the greater annual thermometrical range there.

Dr. Croll assumed (arbitrarily) that a rise of 1° at the equator corresponded with a decrease of 1.5° at the poles; but inspection of either of the diagrams last mentioned, or of table IV, will show that the simple stoppage of heat-distributing agencies would elevate the temperature of the equator 43.97°, while it would lower that of the pole 90.76°. The ratio is thus approximately as 1 : 2. But this ratio represents the rise at the equator when both hemispheres alike cease to transfer heat toward the poles; and it would be much less if circulation were stopped on but one hemisphere. It may be assumed that in such a case the ratio would be as 1 : 4. Then, when an ice-cap extended to any higher latitude, the rise at the equator would be still less, as the excess of heat would

[35] *Ibid.*, p. 42.

be distributed over the whole zone between the equator and the margin of the ice. It is probable that the ratio would thus be diminished as the area covered by the ice-field. Therefore, when the margin of the ice was at any latitude above the equator, the ratio, instead of being as 1 : 4, would be as $(1 \times \frac{s}{a}) : 4$, where a represents the area of the hemisphere, and s that of the zone bounded by the higher latitude. From this expression the following table V has been computed.

TABLE V.

Rise of temperature at Equator for each degree of diminution at the Pole.

Latitude of margin of ice.	Rise at Equator.	Latitude of margin of ice.	Rise at Equator.
0°	0.250°	50	0.058°
10	.207	60	.033
20	.164	70	.015
30	.125	80	.004
40	.089	90	.0

Temperature at higher latitudes.—It has been pointed out that the rise of equatorial temperature, in consequence of depression at the pole, would be approximately the same over the whole of the zone from the equator to the margin of the ice. Hence table V furnishes a basis for calculating the temperature of the whole hemisphere when the ice extends to any latitude. Table VI has been calculated accordingly. The temperatures outside of the ice-field were obtained by the formula $90c + p$, in which c is the coëfficient of rise given in table V, and p the present temperature; those in the ice-covered zone are the lowest values in table IV; and those at the margin of the ice-sheet are assumed to be a mean between the two. For it is obvious that the temperature cannot pass directly from the one extreme to the other; there must be a zone of intermediate temperature; and it is within the polar portion of this zone that the ice of a glacial epoch is formed. It is assumed, however, that neither the ice-covered nor the iceless zone affects the other for more than ten degrees from their junction, *i. e.*, the margin of the ice-field. It was shown in Part I that in order for any aqueous vapor to be carried ten degrees upon any condenser it would have to be borne inward at the rate of twenty

miles per hour for thirty-five hours; and it was also shown that if such a thing occurred it would be fatal to the integrity of the ice-sheet. Moreover, it has been sufficiently shown in section I of this Part that dry air will not convey heat so effectively as to affect materially the temperature 700 miles from the heat-source. It is possible, though, that the ice would affect the temperature to a greater distance on the equatorial side.

The broken lines in diagram III represent graphically the temperatures when the ice extends to latitudes 20°, 40° and 60° respectively.

TABLE VI.

Temperature of Hemisphere when ice extends to any Latitude.

Margin of Ice-sheet.	Temperature at the various Latitudes.								
	0°	10°	20°	30°	40°	50°	60°	70°	80°
10°	+98.33°	+63.89°	+18.08°	0.°	−24.10°	−53.00°	−84.27°	−110.14°	−122.36°
20	94.46	94.66	55.12	0.	24.10	53.00	84.27	110.14	122.36
30	90.95	91.15	88.65	+40.52	24.10	53.00	84.27	110.14	122.36
40	87.71	87.91	85.41	77.81	+20.20	53.00	84.27	110.14	122.36
50	84.92	85.12	82.62	75.02	61.72	3.04	84.27	110.14	122.36
60	82.67	82.87	80.37	72.77	59.47	+44.67	25.55	110.14	122.36
70	81.05	81.25	78.75	71.15	57.85	43.05	+31.55	46.40	122.36
80	80.06	80.26	77.76	70.16	56.86	42.06	30.56	+16.36	57.60

3. ANNUAL VARIATIONS IN SOLAR INTENSITY.

Extent of variation.—Hitherto only mean temperatures and solar intensities have been dealt with; and it might with apparent reason be objected that the great annual variations in the sun's intensity vitiate the results stated; for, as shown by Meech, the solar intensity is actually considerably greater over the frigid zones during summer than it ever is at the equator. The accompanying double table (VII) has been calculated in such a manner as to render these variations apparent; and the computed values are graphically depicted in diagram IV, in which, let it be observed, the scale of abscissæ is only one-half that employed in the other diagrams. The first column in the table exhibits the heat-units as determined by Meech (p. 18 of the memoir cited above), for June 15th and December 13th, the second the equivalent in

degrees Fahrenheit, and the third the absolute temperature, calculated as in previous tables. The fourth and fifth columns represent the corresponding values if 25 *per cent.* is allowed for loss. *Obviously these temperatures could never obtain;* but the figures represent the temperatures that would obtain if directly proportional to the accession of heat from all sources.

TABLE VII.

Solar Intensity.—June 15 and December 13.

Latitudes.	Heat-units —Meech.	Undiminished.		Diminished by 25 per cent.	
		Equivalent in °F.	Absolute temperature.	Equivalent in °F.	Absolute temperature.
SUMMER EXTREME.					
0°	72.0	320.4°	+81.4°	240.3°	+1.3°
10	79.6	354.2	115.2	265.6	26.6
20	85.2	379.0	140.0	284.2	45.2
30	88.4	393.4	154.4	295.0	56.0
40	90.1	400.9	161.9	300.7	61.7
50	89.9	400.0	161.0	300.0	61.0
60	88.8	395.1	156.1	296.3	57.3
70	91.7	408.0	169.0	306.0	67.0
80	96.1	427.6	188.6	320.7	81.7
90	97.6	434.3	195.3	325.7	86.7
WINTER EXTREME.					
0°	76.9	342.2°	+103.2°	256.8°	+17.8°
10	66.9	297.7	58.7	223.2	−15.8
20	55.4	246.5	7.5	184.8	54.2
30	43.0	191.3	−47.7	143.4	95.6
40	30.3	134.8	104.2	101.4	137.9
50	16.3	72.5	166.5	54.5	184.5
60	4.9	21.8	217.2	16.4	222.6
70	0.	0.	239.0[36]	0.	239.0[36]
80	0.	0.	239.0	0.	239.0
90	0.	0.	239.0	0.	239.0

[36] Temperature of space.

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Effect of variation.—"The total amount of solar heat received by the earth in a year, if distributed uniformly over the earth's surface, would be sufficient to liquefy a layer of ice 100 feet thick, and covering the whole earth;"[37] but forty *per cent.* of this amount is cut off by atmospheric absorption. The heat received by the frigid zones in a year is to the amount received by the whole earth during the same period as 5.45 to 9.83, according to Meech.[38] Diminishing 100 feet by 40 per cent and by $\frac{5.45}{9.83}$, we find that but 33.26 feet could be melted from the polar zone in a year, provided all the solar heat were utilized, or only 24.95 feet, if 25 *per cent.* be allowed for loss. Each square foot of surface would therefore receive about 205,400 units of heat.

Now ice is a conductor of heat, though an imperfect one; and, as will be shown in Part III, each particle of ice on being raised to the melting-point, suddenly absorbs a large quantity of heat, before falling into water. It may accordingly be doubted whether melting would begin at the surface until the subjacent ice had been raised about to the melting-point, to a depth of probably twenty or thirty feet, and certainly not less than ten feet. But to elevate a column of ice 20 feet in height from —239° to the melting-point would require the expenditure of 311,650 heat-units per square foot of surface; and if the column were 30 feet high, it would absorb considerably more than twice the total annual accession of solar energy. Besides, the supply is not constant and uninterrupted for any considerable period; throughout a great portion of the half-year, during which the frigid zone receives heat from the sun, the supply is intermittent; and what is received during the day is dissipated during the night. Even if the ice were raised to 32° for only ten feet beneath the surface, it may be questioned whether the slightest melting would ever take place. The annual range in solar intensity may therefore be safely disregarded.

As a check on the above calculation, we have the testimony of explorers of polar regions, who find that the polar ice is not only not removed, but is apparently but little affected by the heat of summer. Even on Great Bear Lake, which is mostly within the north-temperate zone, and is surrounded by lands sufficiently moist, and protected from chill by an atmosphere sufficiently humid, to nourish tolerably luxuriant vegetation, the ice is not melted until the end of summer.

[37] Tyndall, "Heat as a mode of motion," p. 443. The calculation has been verified by the writer.

[38] "Relative Intensity of the Heat and Light of the Sun," p. 35.

Conclusion.

The tables and diagrams of this section exhibit very clearly the principles which it is the object of this paper to elucidate, viz.: that to whatever latitude a polar ice-field may extend, precipitation can take place only along its outer margin; and that the temperature of its central portions must sink too low to sustain appreciable quantities of aqueous vapor. They corroborate the statements made in Part I in regard to the atmospheric conditions produced by any vast condenser.

SEC. III. EVIDENCE THAT THE FOREGOING CONCLUSIONS ARE LEGITIMATE AND REASONABLE.

1. Heat-distributing agencies are least efficient when the atmosphere is cold and dry.

Effect of diminution of temperature.—Professor W. H. Dove's invaluable work on the "Distribution of heat over the surface of the Earth"[39] summarizes the results of a vast number of observations on terrestrial temperature, at many stations; and it is yet the standard authority on the subject. The "normal temperature" of each tenth parallel from the equator to N. lat. 70° was determined by taking the mean of a series of observations at thirty-six equidistant points on each; and from the figures so obtained, the temperatures at N. lat. 80° and at the pole, were found by construction.[40] Table II (in Section II) exhibits the temperatures thus obtained. It shows that the mean range of temperature from equator to pole is 77.4° F.—from +79.7° to +2.3°.

[39] "Die Verbreitung der Wärme auf der Oberfläche der Erde. Mit 7 charten und 2 Temperatur-Tafeln." Berlin, 1852.

[40] Auf den zwölf Charten der Monatsisothermen habe ich von 10° zu 10° Länge zwischen den Isothermen graphisch für die Parallelkreise 0. 10. 20. 30. 40. 50. 60. 65. 70 interpolirt und auf diese Weise für jeden der angeführten Parallelkreise die Temperatur von 36 gleich weit von einander abstehenden Punkten erhalten. Das Mittel aus diesen 36 Zahlen nenne ich die *normale Temperatur* des Parallels, d. h. die, welche er an allen Punkten zeigen würde, wenn die auf ihm wirklich vorhandene aber verschieden vertheilte Temperatur auf ihm gleichförmig vertheilt wäre. Für den Parallel 80° und den Pol wurde sie auf folgende Weise erhalten. Ich theilte die Polar-projection der Erde in vier gleichseitige sphärische Dreiecke und bestimmte innerhalb jedes derselben die mittlere Wärmeabnahme von 0° bis 70° Breite. Die Temperaturabnahme in den beiden einander gegenüberliegenden Dreiecken wurde nun als Ordinaten für eine Abscisse aufgetragen, welche der die beiden Dreiecke als Loth halbirende Meridian war. Der Pol lag in der Mitte dieser Abscisse. Anfang und Endpunkt denselben im Aequator. Dadurch erhielt ich für die beiden Dreieckspaare zwei concave Curven, welchen beiden das mittlere Stück (80° — 90° — 80°) fehlte, die sich aber der Natur der Sache nach in der mittlesten durch 90° gehende Ordinate schneiden mussten. Wählt

Now if the heat-distributing agencies are as efficient in equalizing the temperature of the globe in winter as in summer, the winter range of temperature from equator to pole will not be very much greater than the summer range. On the other hand, the considerations detailed in section I enable us to predict a considerably greater range in winter. The accompanying table VIII shows the mean January and July temperatures at each of the several parallels as determined by Dove,[41] and also the thermometrical range (from equator to pole) for each of these months. The table is graphically depicted in diagram V, into which the mean annual temperature is also introduced for comparison.

As indicated by the physical laws already referred to, the thermometrical range is much greatest in winter, being in fact 2.2 times as great as in summer.

TABLE VIII.

January and July Temperatures of the Northern Hemisphere.

Latitude.	January.		July.	
	R.°	F.°	R.°	F.°
90°	−26.0°	−26.5°	− 0.6°	+30.6°
80	23.3	20.4	+ 0.9	34.0
70	19.5	11.9	5.8	45.0
60	12.6	+ 3.6	10.8	56.3
50	5.4	19.8	13.6	62.6
40	+ 3.7	40.3	17.9	72.3
30	11.8	58.5	20.6	78.3
20	16.9	70.0	22.1	81.7
10	20.1	77.2	21.7	80.8
0	21.1	79.5	20.7	78.6
Range.	47.1	106.0	21.3	47.9

A part of this difference in the thermometrical range for the two

man nun die Dreieckspaare so, dass die Temperaturabnahme in dem möglichst verschieden ist von dem in dem gegenüberliegenden Dreieck, so sind die unverbundenen Schenkel jeder der beiden Curven sehr verschieden gegen die Abscissenachse geneigt. Dies combinirt mit der Bedingung des Durchschneidens beider in der durch den Pol gehenden Ordinate, engt die Weit erführung der Curven in ziemlich enge Grenzen ein und man erhält auf diese Wiese die Temperaturen der fehlenden Breiten von 80° und 90° — *Op. cit.*, p. 13.

[41] *Op. cit.*, p. 13.

seasons is undoubtedly due to the varying position of the hemisphere with relation to the sun. It is only a part however. To demonstrate this, table IX and diagram IV have been prepared. The first column in the table exhibits the mean July temperature of the northern hemisphere as ascertained by Dove, and the second the heat-units received from the sun on June 15th, as determined by Meech. The maxima and minima of the solar intensity and terrestrial temperature do not correspond in time, the latter being about a month later than the former. The two columns are therefore practically equivalent. The third column represents corresponding absolute temperature in degrees Fahrenheit, calculated as in table VII, and of course, subject to the same limitation. The fourth column exhibits the difference between observed and calculated temperatures, and *pari passu*, the effect —absolute and relative—of the heat-distributing agencies in equalizing the temperature of the northern hemisphere in summer. The remaining columns exhibit similar values for December 13th (computed) and January (observed).

TABLE IX.

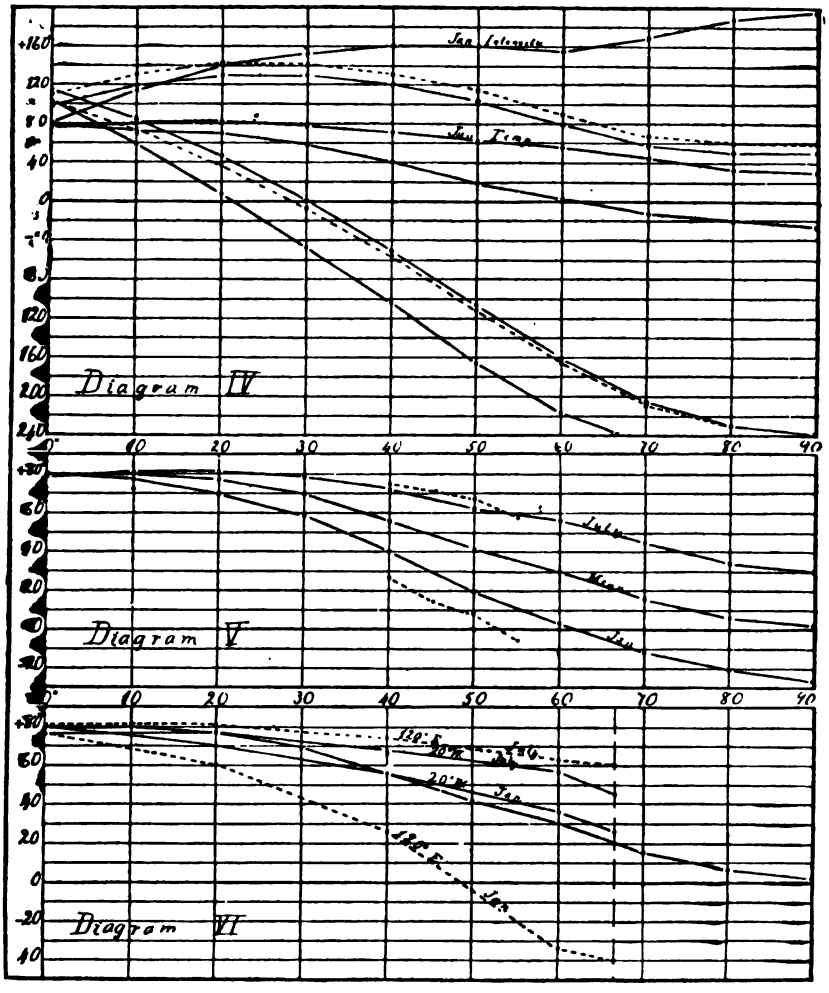
Observed and computed July and January Temperatures of the Northern Hemisphere.

Latitude.	Temperature June 15 (Meech) = July (Dove).				Temperature Dec. 13 (Meech) = Jan. (Dove).			
	Temperature —Dove.	Heat-units—Meech.	Corresponding temp.	Difference.	Temperature —Dove.	Heat-units—Meech.	Corresponding temp.	Difference.
90°	+30.6°F.	97.6	+195.3°F.	—164.6°	−26.5°F.	0.	—239.°[42]	+212.5°
80	34.0	96.1	186.6	—152.6	20.4	0.	239.	+218.6
70	45.0	91.7	179.0	—133.9	11.9	0.	239.	+227.1
60	56.3	88.8	156.1	— 99.8	+ 3.6	4.9	217.2	+220.8
50	62.6	89.9	161.0	— 98.4	19.8	16.3	166.5	+183.3
40	72.3	90.1	161.9	— 89.6	40.3	30.3	104.2	+144.5
30	78.3	88.4	152.2	— 73.8	58.5	43.0	47.7	+106.2
20	81.7	85.2	140.0	— 58.3	70.0	55.4	+ 7.4	+ 62.6
10	80.8	79.6	115.2	— 34.4	77.2	66.9	58.7	+ 18.5
0	78.6	72.0	81.4	— 2.8	79.5	76.9	103.2	— 23.7
Mean.[43]	+62.0		+152.9	— 90.9	+29.0		—108.3	—137.3

[42] Temperature of space, as determined by Herschel and Pouillet.

[43] The means are arithmetical.

In diagram IV, this table is graphically represented (by the heavier lines)—the vertical scale being only one-half that employed in the other diagrams. The most widely separated curves will be seen to resemble those drawn by Meech[44] to represent the actual



temperature of the earth for the months mentioned—the effect of heat-distributing agencies having been overlooked by him. A

[44] Plate IV of the memoir already cited. The ordinates and abscissæ have not, however, the same relative proportion as in diagram IV.

glance at the diagram will show that the solar intensity and terrestrial temperature are far from proportional.

Now, if the efficiency of the heat-distributing agencies were constant in spite of fluctuations in temperature and humidity, we ought to find the difference between observed and computed temperatures the same in the two opposite seasons. In direct opposition to this, but in accordance with the physical laws heretofore alluded to, we find the difference to be much greater in winter than in summer; the means being as 90.9 to 137.3, indicating that the equalizing agencies are more than 1.5 times as effective in summer as in winter. The relative areas enclosed by the upper and lower pairs of curves in the diagram illustrate this relation very clearly. In this case all vicious perturbations due to the inclination of the terrestrial axis to the plane of the ecliptic are, of course, eliminated.

As shown by the table, diminishing the mean temperature of the northern hemisphere 33° (from 62° to 29°)[45] reduces the efficiency of the temperature-equalizing agencies by one-third. Now assuming the efficiency to be reduced in the same ratio by any further diminution of temperature, it is manifest that when the mean temperature falls to −38° (or 100° below +62°), the efficiency of these agencies will be wholly destroyed; yet this is but little more than half of the diminution that would take place, according to table IV, over an extensive polar ice-sheet. Moreover, the ratio is probably not a direct but a decreasing one, as the vapor-tension (which is an index of the temperature-equalizing capacity, as will be shown in the following subtitle) decreases much more rapidly than the atmospheric temperature, as exhibited in the first table and diagram of Section I.

Effect of atmospheric dryness.—The largest and most elaborate of Professor Dove's charts (Charte III) exhibits the monthly isotherms for January and July on the same sheet. A glance at the chart shows that these lines approach most closely over humid regions, and diverge most widely over arid areas, as in central Asia and in western-central North America. The same tendency is equally manifested in the chart devoted to the representation of "Isothermen von 4 Grad u. 20 Grad in ihrer jährlichen Wanderung."

[45] These are simple arithmetical means and are lower than the true means in about the ratio of 46:58.

Table X is taken from the first-named chart. It exhibits the temperature from the equator to the polar circle on two different meridians, viz., long. 20° W. and 120° E. of Greenwich. The first of these passes over the Atlantic ocean, and the second over the arid plateaus of central Asia.[46] The temperatures recorded in the table were, in the greater number of cases, obtained by interpolation. To facilitate reference and verification, therefore, they are given in the scale used in the original (Reaumur's), as well as in Fahrenheit's. The table is graphically depicted in diagram VI, in which the curve of mean temperature is also represented.

TABLE X.

January and July Temperatures at Long. 20° W. and 120° E. of Greenwich.

Latitude.	Long. 20° W.				Long. 120° E.			
	January.		July.		January.		July.	
	R°.	F°.	R°.	F°.	R°.	F°.	R°.	F°.
Polar Kreis.	−2.6	+26.2	+6.0	+45.5	−32.0	−40.0	+12.5	+60.1
60°	+1.8	36.0	11.2	57.2	29.3	34.0	14.0	63.5
40	10.6	55.9	16.0	68.0	2.5	+26.4	19.3	75.4
20	16.9	70.0	20.0	77.0	+13.7	60.6	22.0	81.5
0	21.0	79.2	20.0	77.0	20.0	77.0	22.0	81.5
Mean.		53.5		65.0		18.0		73.4
Range.	23.6	53.0	14.0	31.5	52.0	117.0	9.5	21.4

Inspection of the table reveals the following facts:—

1. The thermometrical range from the equator to the polar circle is greater in January than July (*a*) in the ratio of 53 : 31.5 on the water-meridian, (*b*) in the ratio of 117 : 21.4 on the land-meridian, and (*c*) in the ratio of 170 : 52.9 on both. *Corollary:* Vapor being equally accessible (approximately), the equalizing agencies are most effective when the temperature is highest.

[46] Principal Forbes' paper in the Trans. Roy. Soc. Edin. (vol. XXII, p. 75), in which he calculated that the equatorial and polar temperature of a globe all water would be 71.7° and 12.5° respectively, and that of a globe all land, 109.8° and −25.6° respectively, was based on the temperatures of (probably) these meridians. It has not been seen by the writer.

2. The mean annual temperature of the water-meridian is higher than that of the land-meridian in the ratio of 59.2 : 45.2. *Corollary:* Accession of heat being the same (approximately), the temperature is highest where the atmosphere is most humid.

3. The water-meridian is (*a*) warmer than the land-meridian in January in the ratio of 53.5 : 18, and (*b*) colder in July in the lesser ratio of 72.4 : 65. *Corollary:* The temperature is not only higher but *much more equable* where the atmosphere is humid than where dry.

4. The difference between the January and July ranges (which is the measure of the influence of humidity upon temperature) is greater on the land-meridian than on the water-meridian in the ratio of $(117-21.4):(53-31.5)=95.6:21.5$, or as 4.45 : 1. *Corollary:* In a dry and hence cold atmosphere, the temperature corresponds *much more nearly* with the solar intensity than in a humid and hence warm atmosphere.

The difference in mean annual temperature of the two meridians ($59.2°-45.2°=14°$) is partly due to the influence of the Gulf stream in elevating that of the water-meridian, and partly to the greater dryness of the land-meridian. For the present it will be assumed that the two causes are equally effective.[47] 7° is therefore

[47] It is however scarcely possible that so much of the effect is due to the influence of the Gulf stream. Below are given the July temperatures on the twentieth meridian, from 0° to 60°, as recorded in the above table, and also the July temperatures of the northern hemisphere, at the same parallels taken from table VIII. Though the Gulf stream must be about as effective in July as at any time, it will be seen that the (arithmetical) mean temperature of the twentieth meridian is actually 2.4° lower than that of the northern hemisphere.

LATITUDES.	TEMP. 20th MERIDIAN.	TEMP. N. H.
60°	57.2°	56.3°
40	68.0	72.3
20	77.0	81.7
0	77.0	78.6
Arithmetical means	69.8	72.2

It might be objected that the comparison of July instead of annual temperatures does not afford a just criterion; but it is manifest, in view of the corollaries deduced from table X, that the comparison of mean annual temperatures would be at least equally imperfect and misleading. It might be more satisfactory to compare the mean of the January and July temperatures on the twentieth meridian with the mean annual temperature of the southern hemisphere; though, for reasons given in the following title, the difference in favor of the single meridian must be considered as materially in excess of what it should be. Such a comparison is here given, the temperature of the southern hemisphere being taken from table XIV (*ante*)—the temperature at lat. 60°

the measure of the influence of atmospheric dryness in diminishing the mean annual temperature. Now this diminution of 7° in mean annual temperature, brought about by atmospheric dryness, increases the annual thermometrical range (or at least accompanies an increase in range) from 21.4° to 95.6°, or by 74.2°; and if its influence increases in the same ratio with farther diminution of temperature by the same cause, when the temperature sank to the freezing-point the range would be amply sufficient to meet all the requirements of the preceding sections.

In nature this cause operates conjointly with diminution of temperature produced by other agencies. The above figures illustrate its importance, however.

Table XI exhibits the mean summer and winter temperature of a small portion of North America. It has been taken from another source, viz., from an isothermal chart of the region north of the 36th parallel, compiled under the direction of Governor Isaac I. Stevens.[48] The temperatures were taken on the 100th meridian, and were determined, as before, by interpolation; and there is some uncertainty as to the winter temperature of the 55th parallel, since no isotherms of lower temperature than 0°F. are indicated. The temperatures are graphically represented by the short, broken lines in diagram V. Let it be observed that here the *mean summer* and *mean winter* isotherms are placed in juxtaposition with the isotherms for July and January.

Inspection of the table and diagram shows (1) that the thermometrical range is much greater than the mean, (2) that the winter range of temperature is much greater than the summer range, and (3) that the mean temperature is considerably below the normal. These facts support and corroborate the foregoing corollaries.

being determined by construction. It will be seen that the mean difference, which may be regarded as the measure of the influence of the Gulf stream, is but 3.6°—only about half the amount allowed above.

LATITUDE.	TEMP. 20th MERIDIAN.	TEMP. S. H.
60°	46.6°	38.0°
40	62.0	54.5
20	73.5	74.1
0	78.1	79·7
Arithmetical means	65.0	61.4

It should be borne in mind that only a small portion of that part of the meridian under consideration is materially affected by the Gulf stream.

[48] "Rep. on Expl. and Surv. for a R. R. Route from the Mississippi to the Pacific," vol. XII, pt. I.

TABLE XI.

Mean Winter and Summer Temperature of the 100th Meridian.

LATITUDE.	SUMMER.	WINTER.
55°	+57.5°	— 5.0°?
50	66.5	+ 8.5
45	71.5	15.0
40	74.5	27.0
Mean.	67.5	11.4
Range.	17.0	32.0

Conclusion.—The foregoing figures demonstrate that the climatal perturbations pointed out in Section II, as the necessary result of the considerable extension of a polar ice-sheet, do not differ in kind, but only in degree, from those whose constant occurrence over the terrestrial surface is a matter of authoritative record; and analogy with observed phenomena indicates that the calculated extent of these vicissitudes is in perfect harmony with the magnitude of the formulated cause.

2. LAW INDICATED BY THE PRECEDING TABLES.

Enunciation of the Law.—Though it has not heretofore been specially pointed out, it is very noticeable that when the temperature curves in the diagrams diverge, the downward range is greater than the upward range. This is so universal, not only in the cases referred to in this paper, but in many others which have been investigated expressly to determine whether the exhibition of the tendency is constant, that it may provisionally be viewed as the expression of a law, which may be stated as follows:—*Any increase in thermometrical range is accompanied by a diminution in mean temperature.*

Illustration of its operation.—Table XII exhibits the effect of this tendency as illustrated in the foregoing diagrams (IV, V, and VI) and the corresponding tables numerically, and in such form as to be seen at a glance. The first line shows the excess of mean summer temperature on the 100th meridian over the mean July temperature of the northern hemisphere, and the second the *Abundance* of the mean winter temperature of the same meridian below the mean January temperature of the northern hemisphere, as depicted in diagram V. The third and fourth lines

represent the difference in degrees between the homologous lines of diagram IV, and the fifth and sixth the same for diagram VI. Below are given the aggregates, in the seventh and eighth lines, and, finally, the difference between aggregates (*always in favor of the coldest month*) in the ninth line. The means for each diagram are shown in the right-hand column, and are summed up in the aggregates. In every case the signs + and — refer to the relation to normal temperature, and *not* to the position in the thermometrical scale.

TABLE XII.

Showing greater decrease below than increase above Normal Temperature when Range is increased.

Diagram.	Jahrszeit.	LATITUDES.									MEAN.
		20°	60°	70°	Polar circle.	60°	50°	40°	30°	20°	
V.	Summer.	.....	.....	.....	.....	.....	+1.0°	+2.2°	.....	.....	+3.1°
	Winter.	.....	.....	.....	.....	.... ..	—13.3	—11.3	.....	.....	—12.3
IV.	July.	+164.6°	+152.6°	+133.9°	.....	+99.8°	+98.4	+89.6	+73.8	+58.3°	+109.0
	January.	—212.5	—218.6	—227.1	.....	—220.8	—186.3	—144.5	—106.2	—69.6	—172.3
VI.	July.	.....	.....	.....	+14.5°	+6.2	.....	+8.0	.....	+4.5	+8.3
	January.	.....	.....	.....	—66.0	—70.0	.....	—80.0	.....	—11.0	—44.3
Aggregate.	July.	+164.6	+152.6	+133.9	+14.5	+106.0	+102.4	+99.8	+73.8	+63.8	+120.4
	January.	—212.5	—218.6	—227.1	—66.0	—290.8	—199.6	—185.8	—106.2	—78.6	—228.9
Difference.		—47.1	—66.0	—93.2	—51.5	—184.8	—97.2	—86.0	—32.4	—10.8	—108.5

The difference in aggregates of means (108.5°), which represents twice the mean diminution of temperature occasioned by the operation of the law, is diminished (1) by comparing summer and winter with July and January temperatures in diagram V, and (2) by the junction of the lower curve in diagram IV with the temperature of space. To show that this excess of decrease over increase is not due to the employment of a diagram to some extent hypothetical, a summary of the means for the two diagrams based upon actual observation is appended in table XIII. The difference here indicated is considerably less than it would be if the observations extended quite to the poles instead of only to the polar circle, as the range is greatest at the highest latitudes.

TABLE XIII.

Excess of decrease over increase in Temperature.

DIAGRAM.	JAHRSZEIT.	MEANS.
V.	Summer.	+ 3.1°
	Winter.	—19.3
VI.	July.	+ 8.3
	January.	—44.3
Aggregate.	July.	+11.4
	January.	—56.6
Difference.		—45.2

The excess of decrease over increase at each of the several latitudes reached by the curves is as follows:—

At the polar circle,	51.5°
" lat. 60°,	63.8
" " 50,	9.3
" " 40,	31.1
" " 20,	6.5

Numerical expression of the efficiency of the Law.—While the ratios of diminution of mean temperature to increase of range do not vary greatly in the three cases tabulated, that exhibited by diagram VI is most reliable. Here the means with their differ-

ence, the increase of range, and the diminution of mean temperature, are as follows:—

Mean increase,	8.3°
" decrease,	44.3
Difference,	36.0
Increase of range,	52.6
Diminution,	18.0

It would be premature to say that the diminution is the result of the increase of range, or *vice versa*. It is sufficient that the factors are reciprocal.

The mean temperature at the several points is about 45°. The increase of 52.6° in range is accompanied by a diminution in mean temperature of 18°; and if the ratio is the same with further increase of range, the range of 212°, which would obtain if the heat-distributing agencies were eliminated, would be accompanied by a diminution in mean temperature of 72.5°. The mean would consequently be — 27.5°, instead of + 45°. It is probable, however, that the ratio is a decreasing one with regard to the diminution of temperature. At the best, the figures are but a rude approximation to the truth.

Conclusion.—The evidence in support of the law above indicated seems sufficient to warrant the provisional admission of its existence. Its nature and mode of operation are perhaps sufficiently explained by the identity (in a great measure) of the heat-retaining and temperature-equalizing agencies, preëminently aqueous vapor.

3. Present condition of the southern hemisphere.

Mean temperature.—Table XIV exhibits the mean annual temperature of the southern hemisphere as compared with that of the northern. The figures for the northern hemisphere, and for the southern to lat. 30°, are taken from Dove's work above cited, and those for south latitude 40° to 55° from a paper by Dr. Hann, in the *Zeitschrift für Meteorologie*.[49] The relation is graphically rep-

[49] "Ueber die Wärmevertheilung auf der südl. Halbkugel." *Op. cit.*, vol. VII (1872), p. 263. For the use of the above-named *Zeitschrift*, the writer is indebted to Dr. Gustavus Hinrichs, of the Iowa State University.

resented in diagram I, in which the broken line denotes the temperature of the southern hemisphere.

TABLE XIV.

Mean Temperature of Northern and Southern Hemisphere.

LATITUDE.	N. H.	S. H.
0°	79.7° F.	79.7° F.
10	79.9	77.9
20	77.4	74.1
30	69.8	66.9
40	56.5	54.5
45	49.1 = 53.2	50.4 = 52.7
50	41.7	46.2
55	36.0 = 40.2	41.7 = 45.3
True mean.	73.0	71.8

It would seem at the first glance that the southern hemisphere furnishes an exception to the usual status as illustrated in the last section; for while the mean temperature is lower than that of the northern hemisphere by 1.2°, as seen in table XIV, the annual range is less in the ratio of 2.6 : 9.8, according to Dove.[50] A little study will show, however, that the present condition of this hemisphere is in perfect harmony with the law, and serves as a forcible illustration of its operation.

Greater variability of solar intensity in the Southern Hemisphere.—It may for convenience of illustration be assumed that the summers of the southern hemisphere occur exactly in perihelion. Then, while the mean accession of solar heat is the same on both hemispheres, as long ago demonstrated by D'Alembert, it is manifest that the annual variation in solar intensity is greater on the southern hemisphere than on the northern. Assuming the mean distance of the sun to be 91,430,000 miles, and taking 0.0168 as the present eccentricity of the terrestrial orbit, the perihelion and aphelion distances are found to be 89,897,000 miles, and 92,963,000 miles, respectively. The solar intensity varying as the square of the distance, it will be to the normal as 0.967 : 1 in winter, and as 1.034 : 1 in summer.

[50] "Verbreitung der Wärme," p. 24.

Table XV has been prepared to illustrate the annual variation in solar intensity over the hemisphere whose summers occur in perihelion. The mean winter and summer intensities are given

TABLE XV.

Increased range in solar intensity when winter occurs in aphelion.

Latitude.	WINTER.					SUMMER.					
	Normal Intensity.		Aphelion Int'sity.		V.	Normal Intensity.		Aphelion Int'sity.		X.	VI.
	I. Intensity.	II. Corr. temp.	III. Intensity.	IV. Corr. temp.	De-crease.	VI. Intensity.	VII. Corr. temp.	VIII. Intensity.	IX. Corr. temp.	In-crease.	Increase of Range.
0°	79.5	+114.8° F.	76.9	+103.2°	11.6°	75.9	+ 98.8° F.	73.5	+110.8°	11.5°	23.1°
10	73.9	85.4	70.5	74.7	10.7	80.5	119.2	83.3	131.3	12.0	22.7
20	64.3	47.1	59.3	37.8	9.3	89.9	129.9	85.7	149.4	19.5	21.8
30	54.0	1.3	52.3	—6.7	8.0	89.9	129.9	85.7	142.4	12.5	20.5
40	43.5	—49.9	41.1	56.1	6.9	90.8	130.6	83.5	133.6	19.0	18.9
50	29.9	105.9	28.9	110.4	4.5	77.0	103.6	79.6	115.1	11.5	16.0
60	17.5	151.1	16.9	163.8	2.7	71.5	79.2	73.9	89.9	10.7	13.4
70	7.4	206.1	7.1	207.4	1.3	66.7	57.8	69.0	68.0	10.2	11.5
80	2.0	230.1	1.9	230.5	0.4	65.1	50.7	67.3	60.5	9.8	10.3
90	0.	239.0	0.	239.0	0.	64.9	49.8	67.1	59.6	9.8	9.8
Mean.	37.0	—74.3	35.8	—79.7	5.4	74.8	+ 93.9	77.3	+105.9	11.3	16.7[a1]

[a1] 16.7° is the arithmetical mean. The true mean is 20.0°.

in columns I and VI. They were obtained as follows:—On p. 18 of Meech's memoir, already cited, is a table exhibiting the sun's intensity at the various latitudes at intervals of fifteen days. Column I is the mean (at each latitude) of the units of intensity from September 29th, to March 17th, inclusive; and column VI is similarly the mean from April 1st to September 14th, inclusive. In columns II and VII the corresponding temperatures, calculated as heretofore, are exhibited. Then in columns III and VIII, the intensities, multiplied by .967 and 1.034 respectively, are recorded; and in the next succeeding columns the corresponding temperatures are shown. The difference from the normal is given in columns V and X, and the increase in range in column XI. The values are graphically represented by the broken curves in diagram IV; the full lines (which are the lowest for summer and the highest for winter), being normal.

A glance at the diagram will show that the greater part of the increase of range is upward—the ratio being 11.3°: 5.4°, as indicated by the table; and it might be objected that this would counterbalance the operation of the law stated above. The fact should not be overlooked, however, that the temperature of any planetary body is as greatly dependent on the rate of *loss* as of *accession* of heat, as pointed out by Adhemar; and the greater length of the season during which loss preponderates no doubt more than compensates for the slightness of diminution of accession in winter. The whole of the computed increase in range may therefore be considered as tending, in conformity with the law, to decrease the mean temperature; and employing the ratio suggested by table XIII, we find that the corresponding diminution is nearly 6°. Inspection of the diagram will show, too, that the mean increase of range is greatest over those portions of the southern hemisphere in which the temperature sinks lowest as compared with the northern hemisphere, as may be readily seen by comparison with diagram II.

The foregoing table does not afford the means of directly comparing the condition of the southern hemisphere with that of the northern; for here there is a like effect, and of equal value, but in the opposite direction. The annual variation in solar intensity is hence greater in the southern hemisphere than the northern in the ratio of 1.14: 1. Table XVI exhibits the increase in annual thermometrical range that will obtain in the northern hemisphere

when its winters occur in aphelion, provided the increase is commensurate with the variation in the solar intensity, as it probably will be approximately. The July temperature above space has been multiplied by 1.068, and the January temperature by .933. The range is thereby nearly trebled.

TABLE XVI.

Increase of range when northern winters occur in aphelion.

Month.	Present temperature.			Temperature when winter is in aphelion.		Difference.
	Found by Dove.		Above space.	Above space.	Absolute.	
July	+17.3° R.	+70.9° F.	309.9	331.0°	+92.0°	21.1
January.	7.5	48.9	287.9	268.6	29.6	19.3
Ann. range.		22.0			62.4	40.4

The mean temperatures indicated in the above table are represented by the short horizontal lines in the upper left hand corner of diagram III.

By means of the ratio already employed we find the corresponding diminution of mean temperature to be 14°. It is doubtful, however, whether the thermometrical range would increase in direct proportion with the variation in solar intensity; but even if only one-fourth of the above increase should take place (and this estimate is undoubtedly too low), the corresponding diminution of mean temperature would be nearly three times the present difference in temperature of the opposite hemispheres.

Conclusion.—The foregoing figures seem to afford an adequate reason for the present low temperature of the southern hemisphere, and to show that its condition is in harmony with the principles herein advocated; and it seems probable that the diminution of temperature would be yet greater if the effect of the solar intensity were not so thoroughly masked by the atmospheric and geographical conditions of that hemisphere.

Bearing on Croll's theory.—The bearing of the considerations, above detailed, upon Dr. Croll's justly celebrated theory of secular variations in the earth's climate, is too obvious to require extended comment; and, aside from its relation to the subject under discussion, this title is offered as a contribution thereto. [This phase of

the subject has been considered in a paper entitled, "A contribution to Dr. Croll's Theory of Secular Changes in Terrestrial Climate," read before the Iowa Academy of Sciences June 25, 1880, and published, in very brief abstract, in the *Proceedings* of the Academy, vol. I, pt. I, p. 24; which paper was prepared after the completion of this. The effects of variations in eccentricity of the terrestrial orbit were there discussed.]

SEC. IV.—THICKNESS OF ICE-SHEET AT ANY LATITUDE.

1. Estimates of thickness.

First preliminary estimate.—It was shown in Part I of this paper that the accumulation of glacier ice is dependent on precipitation; and in a general way it may be considered proportional therewith. It may also be assumed that the precipitation, and hence of course the accumulation of ice, is proportional to the vapor-tension. If then the thickness at any latitude is known, that at all other latitudes can be readily computed.

Professor Dana has shown[52] that the thickness of the quaternary ice-sheet over the Canadian highlands (about N. lat. 48° to 50°) must have been at least 12,000 feet. As this accumulation took place under conditions less favorable than those considered in the present discussion, it may be assumed that a thickness of 3 miles might obtain at lat. 40°. The thickness at each latitude from 40° to the pole would accordingly be as represented in table XVII. The data forming the basis of the computation are derived from sources previously enumerated.

TABLE XVII.

Greatest thickness of Ice-field from Lat. 40° to the Pole.

Latitude.	Temperature.	Vapor-tension.	Thickness of Ice.
40°	+56.5° F.	0.457 in.	3.000 miles.
50	41.7	.264	1.733
60	30.2	.168	1.103
70	16.0	.090	.591
80	6.8	.059	.387
90	2.3	.048	.315

Second preliminary estimate.—It would doubtless be more satis-

[52] *American Journal of Science and Arts*, March, 1873.

factory to base estimates upon the present accumulation of ice over polar regions, if the quantity were at all definitely known. The uncertainty regarding the exact amount is so great, however, especially in arctic regions, that any such estimate will serve only as a check on that already made.

It may be almost arbitrarily assumed that, if the land ice existing on the zone bounded by the eightieth parallel were uniformly distributed, it would form a sheet fifty feet in thickness. Now too little aqueous vapor is conveyed into arctic regions to permit the accumulation of sufficient ice to form an effective condenser. It is probable that, in consequence of this imperfection of the arctic condensing apparatus, enough moisture is not congealed, but allowed to fall as rain and thus to melt a portion of the ice, to reduce the accumulation which should take place by fully two-thirds. Were it not for this the accumulation might reach 150 feet on an average, and 300 feet near the margin. The corresponding maximum thickness when the ice extended ten degrees further from the pole would be about 400 feet. These estimates enable us to institute a comparison with the antarctic ice-sheet.

Only about one-seventh of the seventieth parallel of north latitude is so free from land as to present no obstruction to the carrying in of vapor from more southerly regions. In the southern hemisphere, on the other hand, the whole parallel is practically open to the introduction of vapor from the adjacent temperate zone. The accumulation here ought accordingly to be seven times as great as in arctic regions, or 2,800 feet near the margin. It will probably not be objected that these estimates are too low, as they have purposely been made as large as seems at all consistent with the present condition of polar regions. It has already been shown that the present accumulation in these regions is probably about as great as ever can have existed.

Accepting the largest of these estimates as representing the greatest possible thickness of the ice-cap at lat. 70°, and computing the thickness at other latitudes as in table XVII, the respective values are found to be as follows:—

Lat.	40°	14217	feet,	= 2.693	miles.
"	50	8213	"	= 1.555	"
"	60	5226	"	= .990	"
"	70	2800	"	= .530	"
"	80	1835	"	= .348	"
"	90	1493	"	= .283	"

The approximate correspondence between the two estimates is apparent.

Final estimate.—It may be assumed that, in a hemisphere with parallel isotherms and isobars, all vapor is precipitated nearer the poles than where it was formed. Two factors (perhaps unequal), tending to produce opposite results in the final computation, will be disregarded. These factors are (1) the elevation of temperature outside the ice-field illustrated by table VI, and (2) the less frequent saturation of the atmosphere in frigid climates. As shown by the tables of Section II, when the ice-sheet reached any latitude the vapor which had previously been borne polarward would be precipitated near the margin of the sheet, mainly in the form of snow. The precipitation would hence be greater than the normal, at the border of the ice, in the ratio of $p : p + \frac{p\,o}{n}$, where p denotes normal precipitation, o area of zone bounded by margin of ice, and n area of hemisphere. Table XVIII has been computed in accordance with this ratio. It is graphically depicted in diagram VII, in which the vertical scale of the ice-cap is greatly magnified.

TABLE XVIII.

Maximum thickness of Ice-cap.

Latitude.	Temperature. —Dove.	Vapor-tension.	Thickness of Ice-cap.		
			Value of $p + \frac{p\,o}{n}$.	Feet.	Miles.
10°	+79.9° F.	1.020 in.	1.863	55 871	10.582
20	77.4	.940	1.559	46 753	8.855
30	69.8	.728	1.092	32 749	6.203
40	56.5	.457	.620	18 594	3.522
50	41.7	.264	.326	9 777	1.852
60	30.2	.168	.191	5 728	1.085
70	16.0	.090	.095	2 800	.530
80	6.8	.059	.060	1 799	.341
90	2.3	.048	.048	1 440	.273

It is almost needless to reiterate the proposition already demonstrated, that vapor could not be borne far enough within the margin of the ice to affect materially the above results, without

seriously deranging the sequence of phenomena to which the ice owes its origin and conservation.

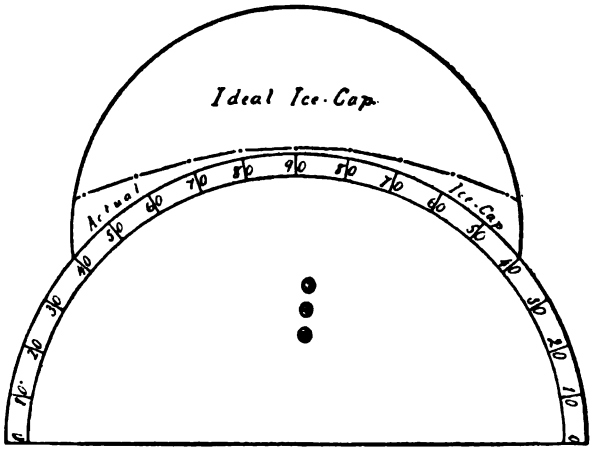


DIAGRAM VII.

The suggestion that the property of flowing might enable the ice to assume a uniform depth may be anticipated by mentioning that the polar slope above given is less than one-tenth of that requisite, according to Hopkins' experiments, to produce the slightest motion.

2. COMPARISON WITH THE ICE-CAP THEORY.

Concomitants of the theory.—The ice-cap theory seems to have been framed chiefly to account for the equatorial motion of the quaternary glaciers. Now, to be consistent with itself, the theory requires that the assumed thickness of the cap shall be sufficient to form a slope down which ice will flow by gravitation alone. Hopkins found that ice barely moves on a slope of one degree; and there is no evidence that existing glaciers move on a less slope. To form such a slope from lat. 40° to the pole, the polar thickness of the ice would have to be 60 miles—the "twenty leagues" of Adhemar. If, with the same mean thickness, it extended only to lat. 45°, the content of the cap would be 575,000,000 cubic miles, equal (the density of ice to water being as .92 to 1) to 529,000,000 cubic miles of water. But taking the water-area of

the globe at 145,000,000 square miles, and the mean depth at 12,144 feet, or 2.3 miles,[53] we find that all the water of the globe amounts to only 333,500,000 cubic miles or but little more than three-fifths of that required to form the assumed ice-cap.

If the above estimate seems too large, let it be reduced by seven-eighths, which will bring it well within the bounds prescribed by more moderate advocates of the theory; but even then it is too large to be admissible; for it would require one-fifth of the water of the globe to form even the smaller ice-cap. But diminishing the water of the globe by one-fifth would diminish the water-covered area by a considerably larger fraction; for the sea-bottom does not descend uniformly to the deeper abysses. The slope is, usually, gentle for a considerable distance from the shore, and then steep and precipitous to the abyssal depths. Reducing the water by one-fifth would therefore probably reduce the area covered by it one-third. Suppose now the ice-cap to be around the south pole: The diminution caused by the removal of so much water, and the farther diminution resulting from the displacement of the earth's centre of gravity, would drain nearly all the water from the northern hemisphere. But the consequent stoppage of marine circulation and of the formation of aqueous vapor would, as shown in Section I, so increase the diurnal and annual thermometrical range as to render the hemisphere uninhabitable for existing organisms.

Relative mass of the two ice-caps.—Assuming the ice-field tabulated above to be of uniform thickness for five degrees on each side of each of the parallels given, and to extend to lat. 45°, its mean depth would be 1.356 miles. Its mass would therefore be only $\frac{1}{15}$ of the larger or a little over $\frac{1}{2}$ of the smaller of the ice-caps considered in the preceding paragraphs. It is the smaller ice-cap which is represented in diagram VII. It should be borne in mind, too, that this is the *maximum synchronous accumulation* under more favorable conditions than would be likely to obtain in nature. The consequent displacement of the earth's centre of gravity has accordingly not been computed.

[53] Sir Wyville Thomson says: "It seems now to be thoroughly established by lines of trustworthy soundings which have been run in all directions, that the average depth of the ocean is a little over 2,000 fathoms." *American Journal of Science*, vol. XVI, (1878). p. 351. Dr. Krümmel estimates the mean depth at 1877 fathoms. See note in *Popular Science Monthly*, vol. XVI, Dec. 1879, p. 287.

CONCLUSION.

It seems quite safe to affirm that in any extensive polar ice-field the thickness will decrease from near the margin toward the pole, where the attenuation will be greatest. It may accordingly be concluded that a sufficient accumulation of polar ice to displace seriously the earth's centre of gravity, or influence the motion of middle-latitude glaciers, never can have taken place in this hemisphere.

PART III.

SECT. I.—THE NATURE AND CAUSE OF ICE-MOTION.

1. PRESENT STATE OF THE QUESTION.

Properties of ice-streams.—The surface of ice-streams has been found to possess a slow differential motion, analogous to that of rivers. As in rivers too, the motion is more rapid near the surface than near the bottom; though it does not seem to have been established that the decrement from the locus of greatest velocity to that of the least corresponds precisely with that of rivers, which is represented, according to Humphreys and Abbott, by a parabola. Ice-streams also follow irregular and tortuous channels, and two or more may unite and become incorporated; and moreover, when the slope of the channel increases, the ice diminishes in depth and increases in velocity just as does the water of rivers. But here the analogy with rivers ceases.

When a glacier terminates at the brink of a precipice, the ice may break off by its own weight and fall down in sections like a solid, or it may bend downward and become drawn out into stalactite-like masses, as in Kane's Northumberland glacier.

Properties of smaller ice-masses.—A bar or sheet of ice supported at its extremities gradually bends downward by its own weight; and a slender bar may be bent into any form by application of gentle pressure; but if the pressure is excessive, a clean fracture ensues. If the fractured surfaces are brought into contact, they re-unite; and if firmly pressed together, the union becomes strong. When subjected to great pressure, ice may be moulded into any shape, or even forced through a small aperture in the form of a slender rod; and, as shown by Bianconi's experiments, when a pebble or a plate of iron is forced into its surface,

the ice rises all around precisely as in the case of pitch, or oil, or even water, but the motion is very much slower.

Theories of ice-motion.—Lack of space will prevent the discussion of the several theories which have been framed to explain ice-motion. The "viscous theory" is that provisionally adopted; and some of the objections thereto will be noticed in the following pages.

2. The viscous theory.

Difficulties of the theory.—More than a hundred years ago, Bordier suggested that the ice of glaciers moves forward in consequence of plasticity or semi-fluidity of the ice. Nearly forty years ago the same idea was more fully developed by Rendu; and shortly after the viscous theory proper was enunciated by Principal Forbes. This theory has been almost generally rejected, not because it fails to explain glacier or ice-motion in all its phases, but apparently because it *seems*, at a casual glance, to be contradicted by our ordinary experience of ice.

Thus, ice is apparently perfectly rigid in its normal condition, and so brittle, as shown by Tyndall, that a slight blow, if properly directed, will split open a block ten or fifteen cubic feet in volume. Moreover, while the rate of glacier-motion is found to vary with the temperature (in consequence of which Principal Forbes was led to suggest that ice is more viscous and yielding when mixed with water than when wholly unmelted), small masses of ice are found, when experimented with, to pass *at once* from the solid to the liquid state without any intermediate viscous stage. An effort will be made to show that these difficulties are only superficial.

Rigidity and brittleness of ice.—The rigidity of ice is only apparent; for it has been shown repeatedly that when ice is subjected to pressure it invariably yields without visible fracture.

Its brittleness is due to its peculiar molecular arrangement, and does not in any way militate against the viscous theory of ice-motion. In nearly every case of fracture, it can be shown that the disruption took place along lines or planes of tension initiated by the blow or other application of force producing the fracture. The effect of tension may therefore be considered, bearing in mind the fact that in ice at 32°, *which contains no portion of its latent heat* (the necessity for this proviso will be developed later), each and every crystal is at its greatest possible size. Any further diminution of temperature will diminish the size of each crystal.

Owing to the greater density of water than ice, ice may be melted by pressure. It will be shown a page or two hence that a given pressure is not only the mechanical equivalent of a measurable quantity of heat, but that its specific effect upon the molecules of ice is precisely similar. Conversely, tension lowers the temperature of ice, and thus diminishes the size of its crystals; and the diminution may occur so rapidly that there is not sufficient time for the distribution of the strain, and hence fracture flies along the line of tension, perhaps with explosive rapidity and violence. If from any cause the ice is already in tension, a slight expenditure of force may lead to extraordinary results, as when on a cold, still night the steel runner of a boy's skate initiates a fracture miles in length in the ice bridging a river.

If a plane of low temperature could be projected rapidly (but not instantaneously) through a block of ice, fracture would be likely to take place along the plane, even if no other mechanical force were exercised. In case of a blow, disruption is the conjoint result of the propagation of such a plane and of the simple tension acting as in other bodies.

Molecular effect of heat upon ice.—The least possible number of molecules of H_2O that can be so grouped together as to form not water but ice, may be denominated a crystal when frozen, and a droplet when melted. To elevate the temperature of such a crystal one degree requires the expenditure of one unit of heat; but to melt it, when at 32°, requires the expenditure of 143 units of heat. Now what becomes of the heat so expended? Professor Tyndall tells us that it is employed in pulling the molecules asunder; or in other words, in diminishing their mutual attraction. Now if this is true of an isolated crystal, we must conclude that the rigidity of the crystal diminishes with the accession of heat, just as the rigidity of iron, or copper, or tallow does—though in these bodies the heat does not become latent. It would seem that in ice the heat is spent in changing the *direction* of motion, instead of only the velocity, as in other bodies; and that the ice, being in unstable equilibrium, greedily absorbs all accessible heat, and employs it in that particular capacity. It is not therefore allowed to become sensible. We shall see that an effect of this change in the direction of motion is to diminish the size of the crystal.

Similarly with the isolated crystal, if we could *equally* apply heat to every crystal in a block of ice or in a glacier, it would

seem to necessarily follow that rigidity would gradually diminish and plasticity or viscosity supervene—that the melting should be gradual as in other solids. The natural objection to such a conclusion, and that usually considered fatal to it, is this: practically a block of ice does not thus become plastic; when experimented with it is always either rigid ice or mobile water; and the transformation from the one to the other is instantaneous, as beautifully illustrated by the sudden appearance of the liquid stars in ice subjected to pressure. A competent answer to this objection was suggested by the interesting experiments with floating magnets, conducted by Professor A. M. Mayer, over a year ago. It is simply this: In practice the crystals are not equally heated. How unequal heating of the crystals prevents plasticity from accompanying the absorption of heat may be made clear by a study of the behavior of a single crystal in a block of ice exposed to the action of heat. Professor Mayer's diagrams, two of which are reproduced in figure 2,[54] will aid us in forming a conception of the molecular change probably accompanying liquefaction.

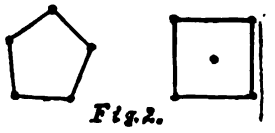


Fig. 2.

This physicist has found that small magnets floating freely upon the surface of a liquid arrange themselves in certain regular forms when acted upon by a larger magnet suspended above them. Several different numbers of magnets have the property of arranging themselves in two or more different forms. Thus, five magnets may assume either the form of a regular pentagon or of a square with a central magnet. It is these two figures that we are interested in: the pentagon is designated form W, and the square with its central magnet form I. Now the area circumscribed by the magnets in form I is greater than that in form W in about the ratio of the density of water to ice; and Professor Mayer sug-

[54] The original diagrams were formed by inking the tips of the magnets, and bringing down upon them a clean sheet of paper. They were then engraved on a somewhat reduced scale by the photo-engraving process. Those here given were formed by pricking through Mayer's diagrams in the *American Journal of Science* (vol. XVI, Oct., 1878, p. 248) into a sheet behind, which was then inked, and again reproduced to a smaller scale by the same process.

gests that the greater density of water than ice may be due to some such different grouping of the molecules as that exhibited by the magnets—the molecules being grouped together in water as in form W, and in ice as in form I. Of course the illustration is imperfect, as the magnets float in a space of but two dimensions, while the molecules arrange themselves in one of three; and moreover we do not know how many molecules form a crystal of ice. Nevertheless, though the illustration may be wide of what actually occurs, it is quite certain that the results about to be stated follow.

These magnets of Mayer, when in the form I, may be drawn toward the positions occupied in form W by the application of external force. This requires a constant but constantly decreasing expenditure of force until they reach an intermediate position (theoretically) in which the attraction in both directions is equal. If they are pushed beyond this position they will of themselves move into the positions occupied in form W, *and will do work in so moving*. Precisely similar must it be with the molecules forming a crystal of ice. Energy in the form of heat has the power of pushing these molecules from the positions occupied in ice toward those occupied in water; and to push them to the intermediate point, where the attraction in both directions is equal, requires an expenditure of just 143 units of heat. The molecules are then ready to fall into form W—to change from ice to water; *but to do so the size of the crystal must be still farther reduced*, and a tension is formed all around it. But the tension is equivalent to a reduction of temperature, as shown by Dr. Joule. Hence as much heat (probably) as was required to push the molecules into the intermediate position is suddenly absorbed when the form W is assumed —when the crystal of ice becomes a droplet of water. But whence comes this heat? Obviously it is abstracted from the surrounding crystals of the block. Thus each crystal of ice probably abstracts as much heat from its neighbors on melting as would cool a like quantity of ice 143 degrees if taken from it alone. If two crystals were about to melt, the melting of the one might absorb all of the latent heat possessed by the other.

This rapid absorption of heat by melting ice has been observed by members of Accademia del Cimenta, "who found that the water in a vessel surrounded by ice cools more rapidly if the ice be heated to accelerate fusion;" and it appears from the experiments of Dr. Grassi that a perfectly analogous phenomenon occurs

when water is heated to its critical temperature (*Scientific American*, vol. XLIII, 1880, p. 160).

The first effect, then, which is produced upon a block of ice subjected to the action of heat, is to diminish equally the cohesion of the exposed molecules. But ice is an imperfect conductor. Hence if the block receives heat at all rapidly, it cannot so distribute it as to affect all the crystals alike; and as soon as melting begins, the remaining crystals are made rigid again. This principle explains the anomalous deportment of ice when experimented with. It is only when the accession of heat is so slow that it may be equally distributed through all parts of the mass that rigidity can gradually diminish and plasticity or viscosity supervene. But ice in glaciers often receives heat so slowly that it may all be equally distributed; and even when melting is in progress the mass is so great that only a small portion is affected by the abstraction of heat accompanying liquefaction.

That ice becomes plastic when slowly melted is a matter of common observation. Professor Bianconi's experiments undoubtedly owe a large part of their success to the fact that they were performed in air whose temperature was slightly higher than that of ice. Canon Moseley's conflicting results in endeavoring to determine the shearing-force of ice are explained by Dr. Croll as being at least partly due to the reception of heat by the ice while undergoing experiment. Lieut. Weyprecht found that a block of ice immersed five metres below the surface of an ice-covered sea becomes covered with a crust of new ice, which, before melting, became so soft as to be easily impressed by the finger.[55] A blow from a hammer upon a block of ice, which has for some time been exposed to a temperature below the freezing-point, grinds a little of the ice to powder, and perhaps starts off a few scales and splinters, but makes no other indentation. The effort to drive a spike into it starts off a few scales; and if the driving is persisted in, minute cracks diverge from the spike, and the block is split into fragments. If, however, the block has for some days been exposed to a temperature so little above the freezing-point that superficial liquefaction does not take place, its behavior will be found quite different. A blow from a hammer produces a distinct indentation,

[55] Bianconi, *Scientific American*, vol. XXXV, 1876, p. 407. Croll, "Climate and Time," Am. ed., p. 502, *et seq.* Weyprecht, review of "Die Metamorphosen des Polareises," by H. N. Moseley, *Nature*, vol. 20, 1879, p. 574.

but may not start off a splinter; and a slender spike may be driven through the mass without a sign of fracture. *Such a condition does not supervene if the ice is exposed to a high temperature*, as it usually is in summer. Ice on ponds and sluggish rivers, when slowly melted beneath a cloudy sky, becomes first softer and more yielding and afterwards so spongy and "rotten" as to sustain almost no weight. The interstices formed by the decrease in size of the crystals become filled with water by capillary attraction, and it gradually changes to a mass of isolated crystals—the "sludge" of sailors.

In freezing, the same stages are often passed through, as has frequently been observed in arctic seas. Thus Dr. Kane records that, on the 9th of September, 1850, Lancaster Sound was "a viscid sea of sludge." On the 11th, "the sludge ice had drawn in around us, and almost congealed under our stern." On the 13th, the tenacity of the ice-fields increased, the temperature having fallen to $+5°$; and during the following night the progress of the vessel was more and more retarded, and finally altogether stopped.[56]

Nor is it until the freshly formed ice has been for many days exposed to the intense cold of the arctic winter that it parts with so much of its latent heat as to become perfectly rigid and brittle. Dr. Kane, whose experience may be taken as representative of that of arctic explorers generally, relates further that even after the ice had become solid, and had reached a thickness of fourteen inches, when driven against the vessel "the semiplastic mass became impressed with a mould of her side." "With matured ice nothing of iron or wood could resist such pressure, as that to which the Reliance was subjected."[57]

The foregoing considerations indicate that ice, in spite of its apparently contradictory behavior under certain circumstances, becomes plastic and viscous, and tends to decrease in volume on absorbing its latent heat. These corollaries follow:—(1) Ice may increase or diminish in volume, while its temperature remains constant. (2) The viscosity of ice may increase or diminish, while it remains constantly at a temperature of 32°. (3) The rate of ice-motion, if due to viscosity as appears, will vary with the rate of accession of heat.

Influence of pressure.—If the molecular effect of heat upon ice is

[56] "In the Frozen Zone," p. 433, *et seq.*
[57] *Ibid.*, pp. 415–6.

as detailed above, it follows that a given pressure is not only the mechanical equivalent of a certain quantity of heat, but also affects the molecules of the ice in precisely the same manner. That pressure lowers the melting-point of ice is well known — indeed Mousson melted ice at zero, Fahrenheit, by subjecting it to a pressure of 13,000 atmospheres.[58] Viewed as a whole, then, a thick glacier would be more plastic and viscous than a thin one, if the rate of accession of heat were equal; and it would be plastic at a lower rate of accession. For ice may be perfectly rigid and brittle (and must be nearly so) if kept constantly a little below the freezing-point and not subjected to pressure. Professor Tyndall found that snow below 32° does not possess the property of regelation.[59]

3. Analogies of ice with other substances.

Analogies with solids.—It has already been stated that a slender bar of ice may be bent into any form by gentle pressure, and that, when broken, it may be welded by simply pressing the fractured surfaces together; but these properties are common to most solids. It is true, however, that greater relative pressure is requisite to weld other solids, because the ice, when experimented with, is usually covered with a film of water which aids in, or even wholly accomplishes, regelation as lucidly explained by Croll;[60] and even if not so covered before the pressure is applied, a slight exertion of force is sufficient to liquefy the projecting points first brought into contact, and the water so formed immediately freezes when the pressure is removed. This distinction from other solids should be borne in mind.

A stick of shoemakers' wax may be bent into any form by gentle pressure; but if the pressure is excessive, a clean fracture ensues. If these fractured surfaces are pressed together, the stick re-unites; and if the pressure is considerable, and is continued for some minutes, the union becomes strong.[61] The slow fluid motion

[58] *Ann.* of *Chem.* and *Phys.*, 3d Series, vol. LVI, p. 252.

[59] "Heat," etc., Am. ed., p. 170.

[60] "Climate and Time," p. 556.

[61] While preparing the final copy of this paper for the press the writer stumbled on the following item in a daily newspaper: — "Shoemakers' wax has been used with success in Glasgow to illustrate to the students of natural philosophy, in a model, the flow of glaciers. It is wonderful how closely the flow of this wax resembles that of ice. Sir W. Thomson has also employed this sort of wax to show the motion of lighter bodies like cork, and heavier bodies like bullets, through a viscous substance." A more extended notice of experiments has accordingly been stricken out. The authentic records of those named above have not been seen up to this writing (January, 1880).

of Stockholm pitch so solid and brittle as to separate into angular fragments, with a conchoidal fracture and glassy lustre, on the application of sudden force, was long ago pointed out by Forbes ("Occasional Papers on the Theory of Glaciers," 1859, pp. 93, 269).

A thin bar of lead and a sheet of zinc (weakened by perforations an inch in diameter and an inch and one-half from centre to centre) were bent into the form of arcs of circles, and were laid, with the convexity downward, upon a level surface. After a time ranging from one to several days, according to the degree of convexity, the curvature disappeared, with the exception of a small portion at either end. If a slight weight was placed upon either extremity of the arc, the time of straightening was diminished, and the extremities were brought down upon the subjacent surface. That lead may be united by simple pressure is sufficiently attested by the process of manufacturing lead pipe, and also by the finding of bullets thoroughly welded evidently by impact in the air. It is said to require constant care, in armories where leaden bullets are punched from bars, to prevent the welding of the bullets to the steel punches.

In his experiments on the welding of solids by pressure, M. Spring has found that out of eighty pulverized solid bodies all of crystalline structure were capable of welding at pressures of 10,000 atmospheres or less. M. Spring says that the facts described do not essentially differ from those observed when two drops of a liquid meet and unite."[62]

Professor W. A. Norton, in a paper on the "Variability of the Ultimate Molecule,"[63] shows that when stress is applied to iron or other material a permanent set results, which remains after the stress has been removed. It is not shown by Professor Norton's experiments that the set increases with the time during which the strain is applied; but that it does so is practically recognized by mechanicians, especially when wood is the material subjected to strain. Moreover, Professor R. H. Thurston discovered some years ago that when a bar of iron was left in his testing machine under heavy strain for sixty-two hours, the strength of the metal increased 20 *per cent.*[64] Similar experiments have shown that the

[62] *Nature*, vol. 22, No. 15, p. 350.

[63] *American Journal of Science*, vol. XVII, March, 1879, p. 183.

[64] *Scientific American*, vol. XXIX, Nov. 29, 1873, p. 336; *Trans. Am. Soc., C. E.*, vol. II, p. 330; *Jour. Franklin Inst.*, Mch., 1874, pp. 150–1.

strength of a bar of iron, strained to the point of rupture and then relieved, increases within some months to a greater degree than before the application of the stress. The above experiments show that molecular rearrangement takes place not only when the material is subject to strain, but when idle. Crystallization is an exhibition of the same property. That iron may be welded by simple pressure is attested by numerous correspondents to the *Scientific American* (the foremost mechanical journal in the United States) who have recorded cases of the welding of mill spindles to the steps upon which they revolved, in almost all cases without heat.[65] Such a weld is perfect, and seems to be due to the pressure which first drives the lubricant from between the highly polished surfaces, and then forces the parts into molecular contact.

The most anomalous properties of ice, such as taking the form of a mould, or of flowing in the direction of least resistance, are common to metals, as experimentally demonstrated by the director of the *Conservatoire des Arts et Métiers* in Paris, M. Tresca, and others. M. Tresca's experiments, published in *La Poinçonnage des Métaux*, indicate the law, common to metals, which he states as follows:—"When pressure is exerted upon the surface of any material, it is transmitted in the interior of the mass from particle to particle, and tends to produce a flow of metal in the direction in which resistance is least."[66] Lead forced through a narrow aperture by pressure exhibits all the characteristics of a flowing liquid, even to "contraction of the vein."[67] The practical bearing of the law of the flow of solids has been discussed by Mr. Chandler Roberts, F. R. S., Chemist to the Mint, who says,—"when metals are subjected to compression, they so closely resemble fluids in their behavior, that the shape they will assume can be deduced by calculation. It is even possible to lay down the trajectory of the molecules of the compressed metal, and to establish with certainty the final places they will occupy as compared with their initial positions."[68]

The plasticity of large rock-masses is too well known to all

[65] See especially vols. XXXVI and XXXVII (1877). For character of weld, see vol. XXXVI, p. 294, 24. It is in discussing these phenomena that a correspondent mentions the welding of leaden bullets to steel punches in Woolwich Arsenal.

[66] Quoted in *Scientific American Supplement*, No. 119, Apr. 13, 1878, p. 1855, by David Townsend, who describes numerous experiments illustrating the flow of iron.

[67] *Loc. cit.*, No. 82, July 28, 1877, p. 1304. Mr. Lewis C. Ware, C. E., here verifies the theory of the flow of metals by mathematical analysis.

[68] *Loc. cit.*, No. 191, Aug. 30, 1879, p. 3046.

students of orographical dynamics to require extended mention; and similar plasticity in smaller masses is also not uncommon. Geo. L. Vose several years ago described the distortion of the pebbles forming a conglomerate at Rangely lake, Maine; [69] Zirkel mentions similar phenomena as exhibited maicroscopically by the nagelflues of Switzerland and microscopically by the older clastic rocks of Nevada; [70] and Clarence King describes cambrian quartzitic conglomerates of the same territory in which "all the pebbles are much flattened, and not infrequently they are welded together, squeezed into one another, having evidently become plastic when under great pressure. There is not a crack or divisional plane in these welded pebbles." [71] The same writer "has observed that slabs of marble when supported by their ends sag in the middle, taking a permanent set." [72]

It is these identical properties which preëminently characterize ice; and it behooves those who deny its viscosity to explain also the flowing of solids on some other hypothesis. There seems to be no essential difference between the flow of ice and that of other solids.

Analogies with liquids.—When a pebble is dropped into water, the liquid is forced out in all directions, forming an elevated rim circumscribing the central depression occupied by the pebble. In oil, the same effects are produced, but more slowly; and in Venice turpentine identical changes in form take place so slowly as to be easily studied. In Professor Bianconi's experiments already mentioned, granite pebbles were placed upon the surface of ice and subjected to pressure, the surrounding temperature being from 34° to 47° F. A deep cavity surrounded by a raised rim was produced in from six to ten hours. When an iron plate pierced by a square aperture took the place of the pebble, the ice rose through the aperture in a square column, and curved over until it touched the plate; the surrounding ice having risen meanwhile and extended over the margin of the plate. A bar of iron, plane below and convex above, being pressed upon the surface, the ice rose as before and applied itself to the convex upper surface. Defining the viscous state as that in which the molecules are mutually

[69] *Mem. Bost. Soc. Nat. Hist.*, vol. I, pt. 4, No. XII.

[70] "Microscopical Petrography" of U. S. Geol. and Geog. Surv., 40th Parallel, vol. VI, p. 263.

[71] "Systematic Geology" of same Survey, vol. I, p. 230.

[72] *Ibid.*, p. 752.

attractive but subject to local or general rearrangement on the application of stress, it would seem that it is one common to solids and liquids, and possessed in a remarkable degree by ice.

SEC. II.—ICE-STREAMS ARE IN TENSION.

1. Modus operandi of ice-motion.

Forces producing glacier-motion.—Glacier-motion seems to be the resultant of two forces, viz.: (1) heat, and (2) gravitation. The influence of gravitation is shown in the motion of all glaciers; that of heat is well shown in the more rapid motion of the Alpine glaciers during day and summer than during night and winter. Time is an important factor, but may be disregarded in this discussion.

Operation of gravitation.—A glacier or an ice-field may be assumed to receive heat equally in all its portions, and at a sufficient rate to produce viscosity. The mass will then be governed by the same laws, in regard to motion, as water or oil. If within a basin, the surface will slowly assume horizontality. If there is an outlet from the basin, the ice will flow through in a stream whose velocity will vary as the resistance offered by the channel. The velocity at the perimeter of cross-section will be less than that near the centre of the surface; but the difference in velocity will probably be less than in rivers, because (1) the mutual attraction among the ice-particles is greater than among the water-particles, and because (2) there is less entanglement of the particles at the base of the ice with those of the bed than in rivers; for melting is constantly taking place at the junction. The mean velocity will vary with the slope of the channel, and the cross-section will be greatest where the slope is least; and if the channel is rugged and tortuous, the motion will be correspondingly retarded. The motion at a little distance from the outlet will not be affected by the pressure within the basin; for a less exertion of force would pile the ice in hummocks near the outlet than would drive it through a narrow, tortuous valley.

Influence of heat.—But heat is not received equally by all parts of the glacier. That part next the *débouchure* receives heat more rapidly than the upper reaches, on account of (1) the warm vapor-laden winds which blow across that portion, and (2) the conservation of heat by the athermous atmosphere of the valleys;

for, as already shown, the low temperature of the source of the glacier is chiefly due to the diathermancy of the superfluitant atmosphere. Hence if the channel of the ice-stream were perfectly uniform, and the mass were bisected across its length, the lower portion would move more rapidly than the upper, and the chasm separating them would continually widen. A like result would follow its vertical section at any point. No part of the mass could be subjected to greater pressure than that due to the weight of the ice; but the tension might at any time increase beyond the tensile strength. If, therefore, glaciers are not pulled in two in nature, it is manifest that the upper portion is drawn forward by the lower, which is correspondingly retarded.

The surface of a glacier receives heat more rapidly than its base, as all the solar and stellar heat is cut off and absorbed by the upper portions—ice being an imperfect conductor. The lower portion receives only the proper terrestrial heat, and that due to pressure and friction. Hence, if the glacier were bisected by a plane parallel with its surface, the upper half would move more rapidly than the lower; and if the plane were passed above or below the centre, a like result would follow (provided the locus of greatest velocity is—as is probable—at the surface). The basal portion must therefore be dragged forward by the upper portions, which are thereby retarded; but fracture cannot result, on account of the pressure of the superincumbent mass.

The centre of an ice-stream also tends to move more rapidly than the sides, on account of the resistance offered by the banks. The central portions must therefore drag forward the lateral portions; and if in the effort to accomplish this the tensile strength of ice is exceeded, *crevasses* must result.

These postulates are indicated by laws governing the motion of viscous bodies; and in so far as they accord with the observed behavior of ice they confirm the viscous theory of ice-motion. Only two or three coincidences will be pointed out. Others are too well known to require comment.

2. Evidence of tension in glaciers.

Crevasses.—It is well known that *crevasses* are common in glaciers, and invariably at right angles to the line of greatest tension. Their frequency and width are greatest in steep and regular por-

tions of the channel; and when more nearly horizontal stretches are reached, the chasms contract, and sometimes close, when the ice becomes welded into a solid mass again. It seems to be unusual for crevasses to extend entirely across ice-streams, and to increase progressively in width, as they should according to theory if the channel were uniform; but inequalities in the bed and the progressive decrease of slope characteristic of valleys in mountainous districts probably serve to counteract all tendency in that direction.

Evidence furnished by the Great Aletsch Glacier.—During a recent visit to the Great Aletsch Glacier, Professor W. H. Niles, of Boston, found that this glacier does not extend into and fill up all the depressions in its bed, but moves forward on the higher portions. "The ice of the glacier, however, is sufficiently plastic to conform to certain kinds of irregularities of surface." "These are long, narrow ridges, the trends of which are the same as the strike of the rock, and nearly parallel with the direction of the motion of the glacier." "The ice had time enough to conform to these longitudinal * * ridges as it flowed over them lengthwise; and in August, 1876, as it passed over the lee end of the ridge, it preserved the mould of the profile so perfectly that for more than twenty feet the blue arch" retained the corrugations formed by the irregularities of the ridge.[73] Now if the force impelling the ice down-stream were not greater than the weight of the mass, the ice would be pressed into all depressions of the bed, and the impress of the ridge could not extend beyond its lee end. Another observation enables us to measure approximately the relation between the two forces.

A bowlder was seen embedded in the lower surface of the ice, and having with it a slow motion. The ice, however, moved more rapidly than the stone, continually creeping over and around it. An inverted trough-like furrow was thus formed in the lower surface of the glacier. This furrow was three feet deep at the bowlder, and extended down-stream from it for thirty feet. The existence of such a furrow proves that the force impelling the glacier forward is ten times as powerful as that pressing the ice upon the bowlder from above. The thickness of this glacier is not mentioned; but it may be taken at the mean of several Alpine

[73] *American Journal of Science*, vol. XVI, Nov. 1878, p. 366; also *Proc. Boston Soc. Nat. Hist.*, vol. XIX. Mch. 20, 1878, pp. 330–36.

glaciers, or 350 feet. Neither is the temperature of the ice recorded; but it is unlikely, situated as it is near the *débouchure*, that it is below the mean observed by Agassiz 200 feet below the surface of the ice, or 31.24°. [If the assumed thickness is too great, it is probably counterbalanced by taking the temperature too low.] It has been found that the pressure of a mile of ice is sufficient to lower the melting point of snow or ice one degree centigrade. In the same ratio, a pressure of 3,500 feet would lower the melting point nearly 1.25° F. Hence if the force impelling the ice down-stream were pressure, the ice would melt at 30.75°. In reality its temperature must be half a degree above that point. Pressure, then, cannot cause the forward motion of the Great Aletsch Glacier.[74]

Other cases of high temperature have been recorded, too high to exist even if the ice were under only the compression due to the weight of the superincumbent ice. All such afford demonstrative evidence that the ice is in tension.

SEC. III.—APPLICATION OF VISCOUS THEORY.

Viewing the ice-field described in Part II in the light of the theory that ice is a viscous body when it has absorbed a portion of its latent heat, and that its viscosity increases with the accession of heat, it will be manifest that from the highest point of the annular belt of ice toward the pole the temperature would be too low and the slope too gentle to produce motion; while from the highest point to the peripheral margin, the rate of motion would progressively increase. The profile of the upper surface would therefore assume some such form as that represented in figure 1 (Part I); and the pressure of the central ice would exercise no influence whatever on the motion of the peripheral ice.

[74] Since the above was written some reason to suspect that the data employed are not very reliable has appeared; but as precise values could not be obtained, it has been deemed advisable to allow the paragraph to remain, partly as an incentive to observations of the character indicated.

The Kames or Eskers of Maine. By George H. Stone, of Kent's Hill, Maine.

Classification of the Kames.

Kames which are arranged in well marked lines so as to have evidently a common origin are classed together as one system in the accompanying list, and the systems have received names from one or more localities along their routes. They are also for convenience of reference numbered in order beginning at the east. The longest tributary is regarded as the main kame and is marked with the system number. Evident branches are marked by annexing one of the first letters of the alphabet to the system number; probable branches by annexing the middle letters; and possible branches that cannot as yet be proved to be distinct systems, by annexing the last letters. Thus the first or most eastern branch of system XI is marked XI *a*, and a probable branch of that is marked XI *a m*. The lengths of the kames as given are approximate. Their total length somewhat exceeds 2000 miles. About two-thirds of this distance has been explored by the writer; the rest is mapped from what is believed to be trustworthy information. No instrumental survey of so great a field was possible and the method adopted has been by reconnoissance to interpolate the courses of the kames on the existing maps.

Names in the annexed table refer to towns or townships through which the kames pass, not to villages.

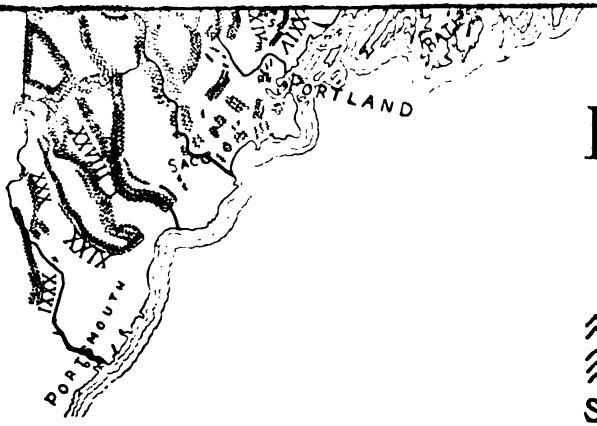
The following abbreviations are used:

v.	= valley	str.	stream	??	= possibly.
r.	= river	l. p.	low pass	? and ??,	refer to the name
l.	= lake	?	probably	preceding.	

LIST OF THE KAMES OF MAINE.

No. of System.	Length in miles.	NAME OF KAME.	COURSES OF THE KAMES.
I	60	St. Croix River System.	Crosses Chiputneticook l. to Vanceboro, along St. Croix v. to Canoose r., by l. p. to Baring? and Pembroke.
I*a*	2	Vanceboro Branch.	Extends N. W. from Vanceboro to Chiputneticook l. ??
II	150	Houlton System.	No. 9, R. 4, Littleton, Orient, under and through Grand (St. Croix) Lake, by l. p. down v. of Tomah str., Baileyville, Meddybemps, Dennysville?, Trescott.
II*a*	20	New Limerick — Amity Branch.	Linneus, Hodgdon; joins II near the S. line of Amity.
II*b*	3	Orient Branch.	Joins II in Grand Lake.
II*c*	55	Hersey — Tomah Stream Branch.	Island Falls, Haynesville, Danforth, by l. p. S. E. along E. & N. A. Railway, to join II near Tomah Station.
II*d*	6	Little Tomah Stream Gravels.	Robbins, Codyville; only in part of kame origin.
III	5	East Machias System.	Near road from E. Machias to Cooper.
III*m*	1	Crawford Kame.	Near crossing of E. Machias r. by Air Line road. Extends interruptedly to E. Machias??
IV	17	Musquash Stream System.	Along v. of Musquash str. from Topsfield to Big L.
V	25	Grand Lake — Old Stream System.	Grand Lake Stream S. to little River, then S. W., and down v. of Old Stream.
V*b*	8	Farm Cove Branch.	From Farm Cove on Grand (Schoodic) Lake by l. p. to Little River.
V*a*	17	Clifford and Chain Lakes Branch.	N. to Big Lake making Systems IV and V one ??
VI	47	Baskahegan Lake — Machias River System.	Kossuth, Pleasant Lake, W. shore Grand Lake, by l. p. to Machias lakes and down Machias r.
VI*x&y*	12	Bancroft and Hot Brook Kames.	May join VI near the N. W. angle of Baskahegan Lake.
VI*m*	5	Springfield — Junior Lake Kames.	Occasional ridges on a steep down slope.
VII	40	Sisladobsis Lake — Pleasant River System.	Only partly explored in a difficult wilderness.
VII*m*		Gravels are reported along	the West Branch of the Machias River.
VIII	130	Seboois-Springfield — Deblois System.	From Seboois Lakes S. to Seboois bridge, then E. by l. p. to upper Shin Pond, by l. p. to Patten, by l. p. to Sherman, v. of Molunkus, Prentiss, by l. p. to Nicatowas str. and to Cherryfield Plains.
VIII*a*	10	Mattakeunk Stream Branch.	From Mattawamkeag r. to Lee, by l. p. to join VIII near No. 3 Pond.
VIII*m*	17	West Branch of Union River Gravels.	From Aurora where this kame stream washed away IX to near Nicatowas Lake.
IX	130	Seboois-Enfield — Deblois System.	Valley of Seboois str., Medway, by l. p. to Chester, crosses Penobscot at S. Lincoln, Greenbush, Greenfield, S. E. by l. p. to Aurora and to the great Deblois or Cherryfield Plains.

No. of System.	Length in miles.	NAME OF KAME.	COURSES OF THE KAMES.
IX*a*	20	Staceyville—Medway Branch.	Joins IX at the Penobscot near the mouth of Pattagumpus str.
IX*aa*	8	Salmon Stream Branch.	Joins IX*a* not far north of the Penobscot.
IX*x*	3	Mattaceunk Stream Kame.	v. of Mattaceunk N. from Penobscot r. May join VIII*a* or IX.
IX*m*	6	Sam Ayer's Stream Kame.	A few miles N. of Mattamiscontis.
IX*b*	70	Katahdin Kame.	From Penobscot r. at mouth of Katahdin str., N. E. and E. to Millinockett l., S. to Shad Pond, by l. p. down v. of Seboeis str., Howland, N. E. corner of Argyle, Greenfield.
IX*bx*	2	Soper Brook Gravels.	Probably Kames, N. of Ripogenus Lake, in T. 4 R. 13.
X	20	Clifton—Hancock System.	Clifton, Otis, Mariaville? Waltham? Hancock? Has a short branch in Clifton.
XI	105	Moosehead Lake—Penobscot Bay System.	Hogback Island, Moosehead l., by l. p. in Shirley, v. of Piscataquis to Sangerville, v. of Black Brook to l. p. = "Notch" in Garland, E. Corinth, W. Hampden, Winterport, Stockton.
XI*a*	70	S. Twin Lake—Winterport Branch.	Medford, Lagrange, Orono. Bangor, Hampden. Joins XI at Ball Hill Cove.
XI*aa*	6	Schoodic Stream Gravels.	Partly kames. Join XI*a* in Medford.
XI*am*	10	Jo Mary Kame.	v. of Pratt Brook, W. of Middle of Jo Mary l. Direction nearly E.
XI*ab*	26	Charleston—Hampden Branch.	Kenduskeag, Levant village, by l. p. to Hermon Bog. Joins XI*a* near Hampden Upper Corner.
XI*b*	15	Exeter Mills—Hermon Pond Branch.	W. Levant, Carmel, Joins XI near Hermon Pond Station.
XII	30	Roach River—Pleasant River System.	Very interrupted. Reaches to Brownville and Milo?
XII*m*	5	Katahdin Iron Works Kame.	N. W. from the Iron Works along v. of a branch of Pleasant r.
XIII	25	Lilly Bay—Sebec System.	Lilly Bay by l. p. and along v. of Wilson Str. to Sebec Lake and Milo??
XIV	20	Corinna—Dixmont System.	Crosses Newport Pond at E. Newport, Plymouth, N. Dixmont.
XIV*x*	2	Kame in Troy and the western part of Dixmont.	Not fully explored.
XV	43	Hartland—Searsmont System.	Pittsfield, Unity, Thorndike, Knox, by l. p. to Montville and N. line Searsmont.
XVI	6	Montville—Liberty System.	Ends in plains near True's Pond.
XVII	32	China—Alna System.	Along China l., S. China, Windsor Village, Whitefield, Alna, Georgetown??
XVII*a*	20	Albion—Whitefield Branch.	Puddledock in Albion, by l. p. to plains near Week's Mills, then interrupted.
XVIII	50	Eastern Kennebec Valley System.	Near Kennebec r. from Somerset Mills to Merrymeeting Bay, perhaps farther.
XVIII*x*	8	Mayfield—Brighton Kames.	An interrupted series in Blanchard, Mayfield and Brighton.
XVIII*m*	10	Clinton Kame	Canaan, Clinton. Joins XVIII at Winslow.
XIX	35	Upper Kennebec Valley System.	Along Kennebec v. from near the Forks to Embden. Now mostly re-classified.
XIX*m*	10	Anson—Madison Kame.	From near Carrabassett r. in W. part of Anson, S. E. into Madison.



MAP OF THE

KAMES OF MAINE

BY

GEORGE H. STONE.

· 1880 ·

KAMES.

KAME PLAINS.

System numbers are shown by Roman numerals.
Elevations in feet above the sea by Arabic numerals.

No Syst
IX
IX
IX
IX
IX
X
X
I
I
I

No. of System.	Length in miles.	NAME OF KAME.	COURSES OF THE KAMES
XIX*t*	1	Parlin Pond Kame.	The stream which deposited this ridge may have flowed N.
XIX*v*	2¼	Kibby Stream Kame.	W of Kibby str., a few miles N. W. of Grand Falls of the Dead r.
XIX*w*	5	Dead River—Jerusalem Kame.	Along l. p. past E. base Mt. Bigelow to Kingfield. May connect with XX.
XIX*x*	3	Stratton Brook Kame.	Not far from Eustis on road to Kingfield. May also connect with XX.
XIX*xx*	3	Kame reported near the divide	between Dead r. and Arnold r., a tributary of the Chaudiere.
XIX*y*	20	Norridgewock—Sidney Plains Kame.	From S. part of Norridgewock through Smithfield and Belgrade into Sidney.
XIX*ya*	2	Short Kame in Smithfield.	
XIX*yb*	8	Mercer—Belgrade Branch.	By l. p. past E. base Hampshire Hill to join XIX*y* near Belgrade Great Pond.
XX	55	Chesterville—Lisbon System.	By l. p. in E. Livermore, Leeds, Sabattus v. Interrupted S. of Leeds Junction.
XX*a*	12	Winthrop—Leeds Junction Branch.	Broken series. Extends N. through Lake Maranocook into Readfield?
XX*aa*		West Winthrop Kames.	A local series of small ridges.
XX*ax*		Small Kame near Mount Vernon village.	
XX*x*		Androscoggin River Kames.	At long intervals from Livermore to near Lewiston, then quite consecutive to Durham.
XXI	5	Freeport System.	From near S. E. corner of Durham to Freeport or beyond.
XXII	30	Canton—Auburn System.	From Canton Point and perhaps above, by l. p. in Livermore, Turner, Lake Auburn.
XXIII	15	Buckfield—West Minot System.	By l. p. along line of R. R. Extends interruptedly toward Canton.
XXIII*a*	6	Sumner—Buckfield Kame.	Along v. of W. Br. of 20 Mile r.
XXIV	105	Androscoggin Lakes—Portland System.	Along Lake Welokennebacook, by l. p. and v. of Black Br. and v. of Ellis r. to Rumford, by l. p. to Bryant's Pond, Oxford, Poland, by l. p. to Gray, Westbrook, Scarboro
XXIV*x*	2	Kennebago River Kames.	About 12 miles N. of Lake Mooselocmaguntic.
XXIV*a*	8	Andover Plains.	Two series reaching N.W. from Andover.
XXIV*b*	8	Locke's Mills Branch.	Joins XXIV at Bryant's Pond.
XXIV*m*		Sands and gravels, partly Kames,	through Danville, at New Gloucester, Pownal and Yarmouth.
XXIV*n*	4	West Cumberland Kame Plains.	May be a delta formation thrown off by XXIV from Gray.
XXV	30	Casco—Windham System.	By l. p. from Thompson Pond, along and under Rattlesnake and Panther Ponds, Raymond, to N. Windham, then as double system to Gorham and Scarboro.
XXVI	65	Bethel—Saco River System.	By l. p. from Bethel, v. of Crooked r., W. shore Sebago Lake, then as two series in Standish, W. Gorham and Buxton. Scarboro and Saco were overrun by XXIV, XXV, XXVI and XXVII.
XXVI*a*	13	Long Pond—Naples Branch.	Extends under Long Pond as far N. as Bridgton Landing.

No. of System.	Length in miles.	NAME OF KAME.	COURSES OF KAMES.
XXVII	40	Fryeburg—Saco River System.	From Fryeburg, or Conway?, to Hollis and beyond. Found near the Saco r.
XXVIII	55	Conway-Ossipee—Mousam System.	Ossipee Lake. Balche's Pond, Shapleigh, Sanford, Wells.
XXVIII*a*	50	Great Ossipee—Kennebunk Plains.	Partly kames, partly valley drift, like all the plains in this region. Runs E. along v. of Great Ossipee to Cornish, S. by l. p. in Limerick, by l. p. in Waterboro. Alfred and indefinitely down Kennebunk v.
XXVIII*b*	24	Great Ossipee—Alfred Plains.	Great Ossipee r. by l. p. in central Parsonsfield, N. Shapleigh, by l.p. to Alfred.
XXVIII*c*	10	Great Ossipee—West Newfield Plains.	From Great Ossipee r. S. by l.p. near W. line of Maine.
XXVIII*d*	5	Little Ossipee River Plains.	From N. Shapleigh E. to Waterboro. Intersects XXVIII*a* and XXVIII*b*.
XXIX	28	South Acton—Wells System.	By l. p. in Acton and Lebanon through N. Berwick. Maryland Ridge, Wells, may belong to either XXVIII or XXIX.
XXX	7	West Lebanon System.	From North East Pond S. by l.p. to Berwick??
XXXI	15?	East Rochester—Dover System.	N. of E. Rochester lies in Maine, S. of there in N. H. A branch running N. W. in N. H. ??

GENERAL TOPOGRAPHY OF MAINE.

The state is disposed in two grand slopes.

1. *The southern slope.*—The average breadth of this slope is about 140 miles, its average fall per mile south-southeasterly at least seven feet. It is drained by numerous streams whose general courses lie somewhat east of south, yet an inspection of the map shows that they zigzag considerably and flow east or west for a considerable part of their courses. These abrupt deflections are caused by numerous ranges of hills trending eastward or northeastward. The presence of these ranges transverse both to the direction of general slope and of glacial flow must have had a great influence upon both the movements of the glacier and of the escaping waters during the final melting. After the ice had so far melted as to be not more than 400 or 500 feet in thickness, over a large part of the state, further flow unless toward the east or west would be impossible on account of these high ranges, which would then rise above the ice surface.

2. *The St. John Slope.*—This comprises the northern portion of the state. Its average breadth is about ninety miles, its slope two or three feet per mile toward the north and east. Much of the region is quite level, and even its highlands would not be very conspicuous but for the great monotonous plain around them.

KAME DRIFT COMPARED WITH GLACIAL DRIFT.

We have in Maine an excellent opportunity for a comparison of this kind, owing to the presence in the central part of the state of a great area of slate and fossiliferous rocks, bordered by crystalline areas. Thus, with the exception of one short outcrop of granite, the course of System IX lies wholly within a region of slates, conglomerates and limestones for nearly 100 miles. It then crosses the granitic range of mountains extending northeastward from Mt. Desert. For seven miles after entering the granite area in Aurora the kame is composed almost wholly of slate, although the country on both sides is covered by granitic till and shows multitudes of granite bowlders and fragments. The granite then begins to appear in the kame and, a few miles farther southeastward, the kame-plains are almost wholly composed of granite, although the underlying rock is there a micaceous slate or schist, and the till shows that kind of rock freely. These and other facts of similar import show that kame materials have been transported lengthwise of the kames and in general farther than morainal material originally derived from the same locality. In other words, kame drift is glacial drift plus a variable amount of water drift.

THE KAME STREAMS.

The sand, gravel and pebbles of the kames show plainly that they have been classified by running water and all are more or less rounded. The facts already cited prove that the currents which classified these gravels flowed lengthwise of the kames and southward, and the same is shown by the prevailing direction of stratification, by the fact that near the north end of kames the gravel is less rounded and water-worn than farther south, and by the position of the plains or delta-like enlargements of the kame. In four cases, however, the direction of the currents is still a matter of doubt.

All the longer kame systems in the state are at their northern

extremities from 500 to 1600 feet above tide water, while at the south they end in the sea or at less than 300 feet elevation. This fully accounts for the slope necessary for a stream. The kame streams resembled ordinary rivers flowing in sub-aërial channels in many, perhaps in all, respects. Their meanderings and zigzags, even when passing over a very level region, are often remarkable and plainly like rivers. The map shows that many of them come together like the tributaries of a river, and these branches at their northern ends may be many miles apart and be separated by high hills or mountains. The plains closely resemble deltas; and VIII *m*, XXIV *n*, XXV, and the many reticulated branches of XXVIII, show divergent delta streams on a large scale. Again the kames bear the same relation to drainage basins as ordinary streams. At their northern ends all, so far as now known, originate in places favorable for collecting a considerable body of water from the melting glacier. That the flow of water was swifter in some parts of the streams than in others is shown by the nature of the deposits. Short slopes do not seem to have much affected the rate of flow, but on up slopes or levels of several miles' length, the kames are usually large and of fine material, while on long down slopes, or near the height of the higher divides, or in the jaws of narrow passes, they may wholly disappear, or be represented by the larger pebbles only. Every long kame system in the state shows this alternation of finer and coarser material, varying according to the slope. The map shows how often the kames disregard the lines of natural drainage. From this it may be inferred that these rivers were contained within ice walls, and that the ice surface was far from coinciding with the underlying land surface. That these rivers for the most part flowed in superficial channels in the ice, the writer regards as a fair inference from the facts observed, but the limits of the present paper make it impossible to give details. Thus far I have found but few signs of sub-glacial streams far back from the coast. Near the coast there are evident signs of such streams and some kames may have been deposited by them.

THE EXTERNAL FORMS OF KAMES.

1. *The single ridge.*—This varies in breadth and height in different parts of its course and is sometimes 100 or more feet high. Gaps are common in the ridge, even in a level region.

2. *Reticulated kame-plains.*— Often a single ridge divides into two branches which soon come together again and thus enclose a sink or funnel, which may, if it be low enough, contain a lakelet without visible outlet or inlet. Or, a kame may expand into a series of ridges connected by cross ridges and enclosing numerous funnels or lakelets. These plains are often many miles in length and from one to five miles in breadth.

3. *Solid or continuous kame-plains.* — These are broad and massive plains, or ridges usually quite level on the top and showing few or no funnels or signs of separate ridges. They are in fact an enlargement of the kame and are often higher or lower than the narrow ridge which may be found both north and south of them. Several kame systems in the Androscoggin-Kennebec region break up into a series consisting of these broad, solid plains or ridges, separated by gaps of a mile or somewhat more. Some of these solid plains are 140 or more feet in height. They may be one mile in breadth and several miles in length. If long, they are usually not very broad.

THE INTERNAL STRUCTURE OF KAMES.

Internally kames are of two classes.

1. The stratified kame, which is the more common kind.

2. The pell-mell kame, composed of the finest and coarsest materials indiscriminately mixed. Occasionally a kame contains what appears to be upper till, as if a mass of till had slid down into the channel of the stream and defied classification by the current. In general the rounding of kame material is very conspicuous. In some cases pebbles from two to four feet in diameter have been made almost spherical. The rounding of kame pebbles or cobble stones is not like that of our ordinary streams, but resembles that of the pebbles found in pot-holes or on the sea beach. Some of the stones of the ground moraine are rounded in a very similar manner. In order to account for this extreme rounding of kame pebbles we must suppose great violence and vertical movement of the waters of the kame river, or that the movements of the glacier assisted in the rounding process, or perhaps that both of these causes were combined. A kame is sometimes found to be stratified in some parts of its course and pell-mell in others.

ACTION OF THE SEA UPON THE KAMES.

During the Champlain period the sea probably stood in the central parts of Maine, at a height of 300 or 350 feet above the present sea level, and many kames were submerged during that period. The sides of the kame that has not been under the sea usually slope at the angle of stability of loose materials in air, while marine waves and currents often reduced the kames to the form of low rounded bars, whose sides slope at the angle of stability of such materials in water. The fossiliferous Champlain clays have hundreds of times been seen overlying the kames and I have taken mya, balanus, and other marine genera from the undisturbed clay found in a depression in a kame. The difference in the physiognomy and structure of the kame which has been under the sea and that which has not, is so great as to show conclusively that the kames proper cannot have been a marine deposit.

DISTRIBUTION OF THE KAMES.

A line joining the northern extremities of the longer kames is nearly a straight line; it trends nearly northeast and is roughly parallel with the coast. North and west of this line there are occasional short ridges of kame origin, but no long systems have yet been discovered. The writer's theory as to the cause of this is, that it is chiefly a matter of slope. On a long slope northward or northeastward, the action of the melting waters of the great glacier must have been very different from what it was on a southern slope of seven feet per mile. For the full development of kames a medium slope is necessary, also that the melting proceed simultaneously over a considerable area.

TOPOGRAPHICAL RELATIONS OF THE KAMES.

We have already referred to the numerous ranges of hills in Maine, which lay athwart the course of the glacier. All of the longer kame systems cross one or more of these high transverse ranges, but never except by low passes, that is, passes not more than about 200 feet above the country lying to the northward. Hills lower than this they cross freely, and, indeed, are found in all relations to them, now turning aside as if to avoid a hill not more than 100 feet high, and soon crossing such a hill when it might avoid it by as short a course or even shorter. In some cases

where a kame breaks up into a series of disconnected ridges or solid plains, like that which passes through Portland, the gravels are found on the higher land, but this rule is not followed by some other similar discontinuous systems (XVII, XX). While following a valley the kames may sometimes be found along the axis of the valley, but more commonly approaching the bordering hills, first on one side and then perhaps on the other. After entering valleys they are not known to leave them when the bordering hills are more than about 200 feet high, and they do not ordinarily leave a broad south-southeasterly valley even when bordered by hills much lower than this. Valleys of other trends they very often enter and leave them again or cross them. This is well illustrated by the courses of systems VIII, IX, XI, and XXIV. For instance, IX runs lengthwise of the valleys of seven streams, and crosses five, besides entering the Penobscot valley twice, following it for a few miles, and then again leaving it for higher ground. The reason the kames run lengthwise of so many valleys is that they follow up a stream to its source, cross a low col and then follow down the valley of a stream flowing in the opposite direction. In general the courses of the kames are straighter than those of the longer rivers, for the latter are deflected by the east and west ranges of hills, while the kame rivers might take a direct course through some low pass, and thus often reënter the valley of the same river which they had just left. Where there are several passes the kame does not always take the lowest or most direct, in which case it is sometimes found that by so doing the kame, some miles toward the south, secures a more favorable pass than if it had taken the other route. This is the anticipatory choice of water rather than the blindness of the moraine. This relationship between the kames and the low passes is of inestimable service to the explorer when in a hilly or mountainous region. The first part of his problem is to determine where are the low passes. He may thus be able to predict the courses of the kames for many miles ahead, and will know where it will be profitable to explore. The inference of the writer is that the kames were deposited late in the ice period, when the glacier was so far melted that the higher hills rose above the ice surface, and hence the only escape for the waters southward was by the low passes.

PRESERVATION OF FOSSIL INSECTS AND PLANTS ON MAZON CREEK.
By J. W. PIKE, of Vineland, N. J.

THESE fossils are the admiration of collectors, and have a special interest for students of natural history and geology. As delicate copies of the structure of ancient plants, insects, crustaceans, etc., they are unsurpassed. There are inscribed on these beautiful "figured stones" not only the most accurate geological notices, but the finest hair lines from the leaves of ferns, the wings of insects, the limbs of crustaceans, the scales of fishes and reptiles.

Mazon Creek is a branch of the Illinois river, which it joins at Morris the county seat of Grundy county. From five to seven miles above its mouth, the waters of the Mazon have carved their channel through prairie soil, drift, and sandstone, into the blue shale bed in which the fossils are found. Below the shale is a bed of coal which has been opened, in places, by shafts sunk below the level of the stream.

Suppose we are on the ground, desirous of interpreting the geological and biological history recorded in these deposits. We must resort to the method of comparison, upon which all scientific interpretation rests. Beginning with the bed of fossil fuel upon which the shale rests, we compare it with other deposits of carbon, with lignite, peat, and the accumulations now forming in great swamps. By the method of comparison the facts of our Mazon Creek deposit are brought into relation with similar facts, as universal as the world, and scientific insight is acquired. In the underclay are the Stigmariæ with their delicate rootlets. We compare them with cedar, cypress and other roots in the swamp mud under modern peat beds, and conclude that the underclay is the muddy soil of an ancient swamp. By a similar method it becomes evident that the coal is a mass of vegetable matter which slowly accumulated in the stagnant waters of the swamp, and was subsequently pressed into a solid bed. Comparison restores the landscape: we see the half submerged shores and islands, the jungle of marsh plants, the debris of trunks, branches, vines and leaves, settling under the dark stagnant water.

The first layers of shale, that lie above the coal, mark a gradual increase of water over the surface. Compare its flattened tree stems with the perishing forests now being entombed in mud,

where the Atlantic coast is sinking. As the water deepens, the fringing swamp advances upon higher ground, and layers of clay are spread over the deposits of peat and logs. The Mazon shale must have resulted from similar conditions. Compare it with the blue clay now accumulating, where the rising water has flooded valleys and swamps transforming them into shallow bays and lakes. The clay brought down from the land by rivers and coast waves may be yellow or red. It subsides upon the bottom with decomposing organic matters which reduce peroxide of iron to protoxide, and the clay deposit becomes blue. This shale, then, is the product and record of a subsidence that buried the older swamp, with its deposit of carbon, under the waters of a lake. It formed as a soft ooze over the buried peat, and in it the remains of crustaceans, fishes and reptiles that inhabited the water, were entombed.

Compare the leaves in these concretions, or lumps of hardened shale, with those being buried in the sediment of modern lakes. As they float from the marshy shores and sluggish streams and become water-soaked, they sink flat upon the fine mud below. After successive layers have been sifted on them by the water, they are found conformable with the strata in which they are buried. So it is with the leaves in these nodules. Among a thousand specimens it is difficult to find one twisted out of conformity, even at the point, like this one. On the other hand, the extreme softness of the shale at that time is shown by the size of these base leaflets, which were thrust diagonally into it by the mere weight of the blade in water. Such was the geological page upon which these biological records were inscribed.

Comparison develops the inscription into a restored landscape. An impenetrable tangle of ferns, rushes and other lowland plants skirts the rivers and shores, stretching away in broad savannas farther than the eye can penetrate. The drooping foliage trails in the black waters of swamp-lined rivers. Creeping things of strangely generalized form, covered with scales, bristling with spines, are swarming in the murky air, on the moist ground, upon the dense foliage and in the stagnant water. There are roaches, locusts, may-flies and termites; spiders, scorpions, centipedes and caterpillars; creatures something like these, and like crabs, shrimps and craw-fishes. They float along with the fallen leaves, sink down upon the soft bottom and are buried in the growing shale bed,

which thus gains in thickness till at length it is buried under layers of sand like those now forming over peat and clay along our sinking coasts.

We next consider the incipient metamorphism by which a portion of the clay immediately around the fossils, has been transformed into ironstone nodules, or concretions. The iron held in solution in the waters that permeated, and circulated in, the pores of the clay bed, seems to have been attracted and solidified by the chemical force which had been gathered from the sunlight and stored up in the organic matter of the fossils themselves. This force may be compared to that of the galvanic battery, by which metals are deposited from solution. Observe that the nodules take the form of the fossils they enclose. A leaf has the general form of an oval, it is bent to one side, or one leaf lies across the side of another; and the resulting nodule is oval, curved, or has a lump on one side, as the case may be. It would be instructive to compare them with siliceous geodes, that have begun their growth upon fossil corals, crinoids, shells, etc.

The nodules as they lie *in situ*, in the shale, appear as though the ferrous carbonate had been attracted with such force as not only to fill the spaces between the particles of clay, but to crowd them apart and thicken the layers. Here are specimens that exhibit the continuity of the strata, from the soft outlying shale through the hard nodules, which thus appear like swellings in the clay. The layers are concavo-convex, not only in the nodule above and below the plane of the leaf, but also in the soft shale above and below the nodule. It is as though the expansion of the nodule had crowded and bent the strata into arches upward and downward.

But it is not probable that the leaf retained its chemical force till the clay was condensed to shale by pressure, nor that the strata in the nodule were expanded and thickened again after such condensation. No doubt the iron was attracted soon after the leaf was buried, when the shale was as yet soft ooze. The layers were then much thicker, even in the nodule, than now; and as the weight of overlying deposits increased, the soft outlying shale being compressed more than that in the denser nodule, the layers above and below it were bent into arches as we find them.

If we compare these biological records with the primitive inscriptions of man, both are written in the picture language of

nature, but these are incomparably more minute and accurate. The human records are found exposed on the crumbling surface of monuments; but these were sealed up in lumps of stone nearly as hard as flint and buried in beds of clay beyond the reach of wear and decay. The tablets of cuneiform from the fatherland of Abraham were done in soft clay and afterwards hardened by baking; these were inscribed on softer clay which has since been hardened, automatically, by the writing itself, or, more properly, by the types which printed the writing. Like the casts of the founder who surrounds his models with moist sand, these, too, are casts, but they are casts of such delicate things as fern leaves and insect wings, moulded in fine clay by the gentle touch of water and afterwards chemically hardened by an iron cement.

But, the age of the most ancient human writings is as a thing of yesterday—utterly insignificant when compared with the immeasurable antiquity of these inscriptions from Mazon Creek. They were formed in the incalculably remote Carboniferous Period, and while the vast Permian, Triassic, Jurassic, Cretaceous and Tertiary beds, which contain the record of nearly all animals above the grade of fish-like reptiles known to geologists, were being deposited, these engraved stones were sealed up in the shale beyond the reach of change or decay. The granite of the Himalayas, Sierras, Andes, and all the loftiest mountain chains, was, for the most part, formed out of the sediment of dried up seas after these fragile ferns and insects were embedded in the clay. In Post Tertiary time the overlying rocks were planed down by moving ice, and clay, gravel and bowlders strewn over the surface. Then came an age of shallow lakes followed by swamps, forming the black prairie soil.

Finally, the waters of the Mazon dug their channel down into the shale, and the softer clay being washed away, these nodules were left in placers in the bed of the stream, mingled with granitic gravel derived from glacial drift.

Being thus exposed to the combined action of air and moisture, the ferrous carbonate is changed to the ferric oxide of iron, and the nodules turn red like boiled lobsters. During this process they often divide spontaneously, exposing the fossils within, and thus they were found by the German[1] naturalist who made the first collection. But this change of iron from the blue to the red state

[1] Mr. Joseph Even, of Peru, Illinois.

is often attended with solution, resolidification, and final complete separation of the iron from the clay. The iron forms as a red brown shell, and the half of a nodule, with a fern on the side, becomes a geode, enclosing the clay in an ochre-like form in its interior.

These beautiful specimens being mostly exhausted from the gravel beds, the collector must anticipate the denuding forces and dig the concretions out of the shale of the river banks and bottom, and crack them for himself. Specimens got in this way show the finest details of structure, and promise to increase our knowledge of the biology of the lowlands of Carboniferous time.

The Fossil Dinocerata in the E. M. Museum at Princeton, New Jersey. By Franklin C. Hill,[1] of Princeton, N. J.

[ABSTRACT.]

The collection of the bones of the Dinocerata, at Princeton, is not large, but choice, and I hope to be able from it and the aid of my blackboard, to give a tolerably correct idea of the family structure.

Our chief pieces are two skulls, so nearly perfect that our fragments of others enable us to restore them entirely.

One of these has been already described and figured in a Bulletin from the Museum, under the name of *Uintatherium Leidianum*.

The other, a more recent acquisition, is even more perfect, the left side showing everything from the snout to the occipital condyles. We consider it as a Loxolophodon.

Beginning at the snout we notice that a slight twisting of the specimen throws the right "horn-core," so called, a little above and in advance of the left one, so that both are seen in a side elevation, which is not the case with the other pairs.

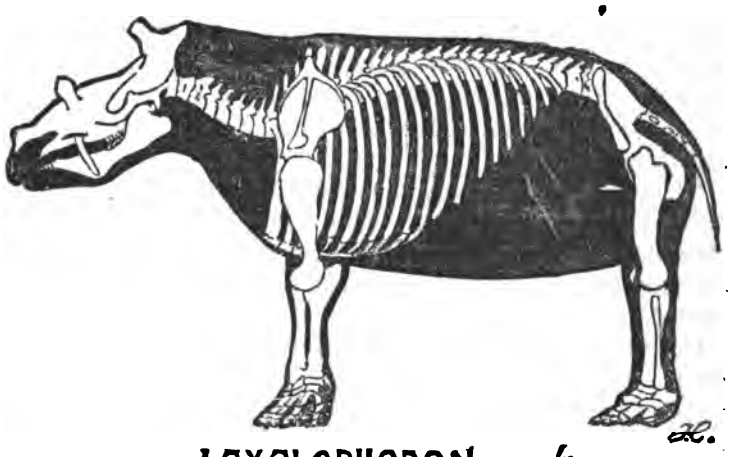
These animals have been called "Proboscidians" and have been supposed to have have had trunks, like Tapirs, if not like Elephants. But the nasal bones, as has been pointed out by others,

[1] Curator of the Museum.

make half the length of the skull. In the Elephants they are very short, and the width of the skull is greater than the length from occiput to insertion of proboscis. But here we have a length of thirty-eight inches and a width of only ten inches. Granted a proboscis — for which there is no "insertion" visible, — what power could it exert at the end of such a lever?

Again, in the proboscidians the premaxillaries are very large and carry the heavy incisors, while here we find them very small, and obviously fitted to carry a pad for the lower incisors to play against, as in the ruminants.

This skull measures nearly thirty-nine inches in extreme length, but among our fragments of other individuals we have a "horn-core" which indicates a skull sixty inches in length.



LOXOLOPHODON. 1/20

The upper canines of this specimen were in place when it was found, but were too fragile for preservation, excepting the spear-shaped points. They were curved much in the manner of the Machairodus canines, but unlike them they had a double-bayonet section and no cutting edge.

Of the lower jaw we have two nearly perfect specimens, and fragments of others, so that we have the entire dentition, — three curiously bilobate incisors, a long diastemma, and six diagonally ridged molars which fully justify Mr. Cope's name "Loxolophodon." From these specimens and Mr. Marsh's plates we are able to restore the lower jaw with confidence.

This dentition seems to indicate a diet of soft roots and stems, rather than of grass, in the collection of which food the upper canines doubtless played an important part, though it is not easy to see just what it was. But the peculiar position of the condyles, they being but little above the line of the molar teeth, and presenting directly backwards, allowed the jaw to fall low enough to clear the points of the canines and thus bring those teeth into action, as has already been pointed out by others.

The descending flap of the lower jaw seems to be for a guard for them, indicating that they were not weapons of offence and defence.

Of the cervix we have the first five bones, and they show that the neck must have been at least seventeen inches long, quite long enough, with the long head, to reach the ground.

The spines of these vertebræ are small and weak, but the size and weight of the head convince me that the first dorsals must have had large, strong spines. Our dorsal vertebræ are from the middle of the column. There were probably about twenty dorsals and three lumbars.

The pelvis and caudal vertebræ which we have belong to *Uintatherium Leidianum*, and, though not entire, are sufficiently so to show that they were similar to those of Mastodon, excepting that the caudals show a much broader and flatter tail than that animal carried.

Our ribs are mostly fragmentary but are mastodontoid in character. Their number, of course, is as indefinite as that of the vertebræ, and we must borrow the shape of the first pair from Mastodon.

Our scapula is not in good enough condition to show the very peculiar form of this bone. Mr. Cope kindly gave me a proof sheet of his plate of a perfect specimen, which I use in my sketch, leaving the description to him.

The humerus has been so well and fully described by Dr. Leidy, that I shall content myself with drawing it and quoting his remark that "it is unique," as in fact is also the ulna, which with a large olecranon has a columnar shaft, expanding very slightly at the distal end, in which it differs widely from the elephants. The radius is straight, and stands almost perpendicularly before the ulna, taking much of the weight of the humerus on its broad head, somewhat after the manner of Hippopotamus.

The hind leg is much like that of Mastodon, both in femur and tibia, though the fibula is placed rather more on the outside of the tibia, to which it joins by a small round articular facet.

We have many foot bones, with which, and Mr. Marsh's plates, I can restore his elephantine feet.

To clothe this skeleton with flesh requires, of course, some imagination. As for the so-called "horn-cores," I see no reason to suppose that they ever carried horns, and the convenient term "Dinocerata" is probably a misnomer; neither they nor the upper surface of the skull show any large channels for blood vessels, or foramina for nerves, and hence it is probable that they had a covering of thick skin only.

The enormous basin between the last pair and the occipital crest is also a problem. I cannot conceive of it as empty, I cannot imagine a filling for it.

Rejecting the proboscis I can only suggest a pair of large flexible lips, and it is probable that the throat was much like that of Rhinoceros.

Certain rugosities on the temporal bone suggest to Mr. Cope that the ear was large and elephantine.

The legs were undoubtedly large, and the tail broad and flat.

TRANSFORMATIONS OF PLANORBIS AT STEINHEIM, WITH REMARKS ON THE EFFECTS OF GRAVITY UPON THE FORMS OF SHELLS AND ANIMALS. By ALPHEUS HYATT, of Boston, Mass.

[ABSTRACT.]

THE limits of this abstract do not permit me to give an analysis of the work done by the author at Steinheim,[1] or yet of Hilgendorf's remarkable essay which led to these investigations.[2] We both agree on the theoretical presentation of the results, that is,

[1] Steinheim is a village on the eastern declivity of the mountains in the eastern part of Wurtemburg in southern Germany, and has been noted for many years for the peculiar series of fossil shells which occur in great numbers in the strata of sand and limestone in the pits close to the village. For further information see Genesis of Planorbis at Steinheim, by the author. Anniversary Memoirs of Boston Society of Natural History, 1880.

[2] Ueber *Planorbis multiformis* in Steinheimer Süsswasserkalk, Monatsberichte, K. P. Akademie, Berlin, July, 1855, p. 474, 1 plate.

in the opinion, that in this small lake in Tertiary times a large number of distinct forms of very different aspects were evolved from an older Tertiary species, the *Planorbis levis*.

Our disagreements though numerous and interesting involve minor points, except in one important particular.

Hilgendorf, whose pamphlet on these shells is said by some Germans to be the *only perfect demonstration* of the doctrine of evolution of organisms ever printed, found all the conditions of the problem in order.

He collected the parent forms in the lowest formation, and all the distinct descendant forms of the series in the successive formations lying one above another in their proper genetic order and sequence. Thus, not only did he trace several series of species, one into another, through the formations beginning with the lowest and earliest deposited, but was able to state that their relations in time or succession precisely accorded with their apparent genetic relations.

The evidence, as he first published it, and which he still asserts, after renewed investigations, to be true, is more perfect than that of any genealogical tree.

It would be similar to the genealogy of a family, if we could trace the members back into geologic times, and show not only that the existing descendants were derived from a common stock, but that these were vastly different from each other now, and from their ancestors.

My own observations refuse to be reconciled with his so far as the perfection of details is concerned, and in my opinion, though the evidence is exceptionally perfect, there are in this place, as in others, the usual gaps resulting from the imperfections and the necessarily fragmentary nature of geological history. While, therefore, I cannot join him in believing in the perfection of the record, my results after all are not theoretically contradictory, and I can only account for the appearances by the same theory of the descent of numerous specifically distinct forms from one species.[3] The results are roughly exhibited on Plate I, and may be described briefly as follows. Figs. 1, 8, 12 and 16, are the ancestors, varieties of *Planorbis levis* from the older Tertiaries of another locality, identified, named, and kindly sent to me with

[3] For a fuller account see Memoir above quoted.

eleven other specimens of this species by Prof. G. Sandberger,[4] who opposes the evolutionary conclusions of both Hilgendorf and myself.

From these four varieties spring four distinct lines of descent. Fig. 1 begins the series from 2–7 in which of course numbers of the connecting forms are not figured. Fig. 8 begins the series from 9–11 much shorter and containing fewer forms than in series 2–7. Fig. 12 also gives rise to a short series with only few forms. Fig. 16, however, is the starting-point for a compound series, or one composed of at least three sub-series, 19–20, 21–24, and 25–28.

The intermediate forms by which the gap between the four ancestors and the four first forms of each series, viz.: 2 and 9 and 17, which occur in the Steinheim basin, is very complete, but necessarily left out in this plate.

Numbering the series from left to right we see that Series I has three sub-series. Two of these show a tendency to uncoil, to become distorted and smaller than the ancestor, fig. 16, while the third decreases in size, but has a form, fig. 20, which is turreted like figs. 11 and 6.

Series II maintains size about the same throughout, but becomes flatter than the originating form fig. 12. Series III grows sensibly larger, and 10–11 are turreted shells with a more ridged and sub-angular form of whorl than the primal form of fig. 8.

Series IV exhibits not only greater increase in size, but vastly greater differences in form and in other characteristics of the shell from fig. 1, with which it started.

We can, therefore, without fear of error call series IV a highly progressive series; Series II a persistent series; sub-series 3 of Series I a partly retrogressive series; sub-series 2 of Series I, a purely retrogressive, and sub-series 1 of Series I also partly retrogressive; since, though it decreases in size and becomes deformed and uncoiled, it also has a tendency to produce a new characteristic, the transverse ribs, and also increases in size its more closely coiled forms, as in fig. 26.

There is also other testimony going to show that this classification is correct. Semper's researches on *Lymnæus stagnalis* show, that under the most favorable physical conditions, this species increases to a maximum of size and has larger whorls, while

[4] The author of the most complete Memoir on Tertiary shells in existence, Conchylien d. Vorwelt.

under less favorable conditions with relation to food and temperature, the size is very much decreased.

The immediate results of weakness, produced by wounds, are also important in this connection.

Pl. 2, figs. 21, 21*a* and 22, a diseased *Pl. oxystomus*, var. *revertens* HILG., is a diseased specimen of the same species as fig. 9, Pl. 1. Compare this diseased, partly uncoiled, shell with the species, fig. 23, Pl. 1, *Pl. denudatus* and *minutus*.

The weakness consequent upon old age is equally significant and has a similar meaning.

Pl. 2, fig. 22, represents the effect of old age in distorting the growth of the outer whorl of *Pl. oxystomus*. Compare with fig. 22, Pl. 1. These are true cases of disease of comparatively rare occurrence in *Pl. oxystomus*. I have in my collection many similar cases and have lately found some diseased shells of *Pl. trochiformis*, fig. 7, Pl. 1. These are dwarfed, and show a tendency to unwind the spiral, so that they look remotely like the beginning of a series of transition forms from *Pl. trochiformis* to *Pl. denudatus*, fig. 24, Pl. 1.

This statement is proved in my Memoir by numerous instances, and also by the Magnon Planorbis figured by M. Piret. Many of these acknowledged to be distorted are precisely similar to fig. 24. These distortions are produced at Magnon, France, undoubtedly, as in these other instances, by the unfavorable physical surroundings. I have also found near St. Johns, New Brunswick, in company with a complete series of uncoiling Valvata, like those of Planorbis on Pl. 1, Series I, sub-series 2, a number of distorted Planorbis, the counterparts of many of M. Piret's figures.

Thus we can assume without fear of error, that the increase in size and the progression of Series IV were due to the favorable conditions of the Steinheim basin, for that series; and that of Series I to the unfavorable conditions, and the mingling of characteristics or of progression and retrogression in sub-series 3 of Series I; and Series II and III to the different extent which these series are affected, either favorably or unfavorably by the same surroundings.

The effect of heredity is seen in the fact, that however great the modifications, as in fig. 7, the young are smooth and like fig. 6, though smaller of course, while the young of fig. 6 is similar to a small copy of 5, and the young of fig. 5, to a miniature fig. 4.

The young of fig. 4, however, may be said to be like fig. 2, first, and then like fig. 3 before it becomes angular as in fig. 4. There is also a very early stage in all these shells,[5] which is a mere bag and is called the ovi-shell or embryonal shell.[6] The first turn, which the young makes in growing around this embryonal shell, has a cylindrical mouth whorl like miniature fig. 8, Pl. 1.

This stage is very prolonged in fig. 3, much shortened in fig. 4, and still more restricted to the internal whorls in fig. 5, though in some specimens very much more prolonged than in others. In figs. 6–7 it remains about stationary, the size and aspect of the young being similar to fig. 8 or 9 or 2, during the earlier stages, until the shell is between one-hundredth and four-hundredths of an inch in diameter.

This is heredity with acceleration, or the inheritance of the characteristics of the original species, fig. 8, at earlier and earlier stages in successive species.

It is the same history with the characteristics which first appear in the adults of the series. The angular form of the outer whorl of fig. 4 is often in *Pl. tenuis* (fig. 4), confined to the outer whorl or whorls built during the adult period of growth. This becomes confined to the younger stages or inner whorls after a certain number of intervening intermediate generations in fig. 5, and are also left out in many specimens of figs. 6 and 7, in whose young they are replaced by the stouter form and heavier ribs and ridges of fig. 5.

The same is true of transverse ridges of fig. 25.

These with few exceptions first appear upon portions of the outer whorl.

Then in successive generations they appear at earlier stages, that is upon the internal whorls, and grow larger in the adults.

The uncoiling tends to appear in the same way first, upon the outer whorls, and it must be noticed, that in no case is the innermost of all the whorls uncoiled, or the bag-like form of the embryo changed.

Such facts show that heredity has the power, even in spite of the forces which changed the adults of the Series IV so completely in this locality, to reproduce or continually rejuvenate the ances-

[5] The centre *f'* fig. 21*a*, and fig. 8*a*, Pl. 2.
[6] For enlarged figure of this early stage see Pl. 1, figs. 29–30.

tral type of smooth, closely coiled whorls, and the original type of the bag-like embryonal shells.

Nor can we account either for the fact, that in fig. 20 a turreted form is produced with longitudinal ridges, sulcations, and all the marks of fig. 11, though in a series evidently very unfavorably affected by the surroundings, as shown by the decrease in size, nor for the persistence of Series II, without recognizing that in these series heredity was fighting a battle against the modifying or unfavorable nature of the surroundings.

It is undoubtedly true, that in many cases there is a retardation as well as an acceleration in the repetition of the characteristics of the ancestor in the young of 6–7 for example. Thus in some individuals the form of fig. 8 may be held until the shell is quite sizable, four-hundredths of an inch in diameter or even larger; but these are exceptions, which prove the rule.

They show like reversions of all kinds the tremendous forces of heredity striving to maintain invariability to reproduce like.

We might with Herr Wurtenburger say, that since some shells in the immediate children of any pair inherit characteristics later and some at earlier ages than those at which they appeared in the parents, that those inheriting them earlier were stronger and had more descendants, and therefore these having the development accelerated, were gradually replacing the other with slower development.

This sounds well, but how can distortions be of advantage, and these are transmitted also with acceleration in development as in Series I, and there ought to be some one series showing this later mode of inheritance, something I have as yet sought in vain.

Again, how can we account for the persistence of the form of *Pl. levis*, or fig. 8, in the young of fig. 7, without taking into account also the embryonal shell?

This is present in nearly all Gasteropoda and Cephalopoda, as well as in Planorbis at Steinheim, and of nearly the same form.

It is hard to refer any such widely occurring phenomenon to a question of advantage or disadvantage, unless we are also prepared to refer all vital phenomena to the same standard, and consent to the proposition, that animals grow and reproduce their own like, because it is of advantage to them to grow and reproduce in this way, that the uniform, spherical shape of the eggs of all animals is due to natural selection and not to the action of uniform mechanical force.

Natural selection certainly has either no influence or only a very remote bearing, if any, upon such facts which are so plainly the result of some uniform law which acts at all times and in all places, and forces the organization to make room for the changes which first take place in the later periods of its life by accelerating the development of those characteristics, which it has inherited from its immediate ancestors.[7]

This is not, however, the case with the differences between the distinct series.

We cannot answer the question why these four varieties of *Pl. levis* were picked out of a number of others, which migrated in company at the same time and allowed to survive in the Steinheim basin, except by the law of natural selection. All the facts support this view, as well as the dying-out of hybrids between them.

The characteristics which distinguish the series from each other are the differences of the four varieties, and it is to the preservation of these original differences, that this law can be safely applied, not to the production of similar forms in distinct series like figs. 6–11–20, or 4 and 19.

These differences are the flatter upper and lower sides of fig. 1, and its angular outer side as contrasted with fig. 8, the flatness and angularity of fig. 12, and the peculiar stout form, smaller size and rounded whorls of fig. 16.

These differences are immensely increased in the successive generations of the different series, except in the persistent Series II, and even here they are slightly increased.

These differences are apparently advantageous to the varieties possessing them, and we can understand their maintenance in these varieties and their greater increase as compared with other varieties of *Planorbis levis* on this hypothesis.

But how explain the growing smaller of Series I in all its subseries, and the origin of deformed species?

These do not die out like the disadvantageous differences, which once filled the gaps between the four ancestors of the different series. They are perpetuated and are inherited according to the law of quicker development or acceleration in the same manner as the advantageous characteristics of Series IV and III, and like

[7] These last are almost the words of Herr Wurtemberger; but that they come from my own previous publications may be seen by comparison with my first publication on this subject—"Parallelisms of Individual and Order among Tetrabranchate Mollusks." Memoirs Bost. Soc. Nat. Hist., Vol. I, 1866-7.

them they become characteristics of species, or groups of forms, which can be called species or varieties as one chooses.

Like the inheritance of wounds or diseases, they tend to appear at earlier ages in successive generations or become immediately congenital, and like them they are certainly not advantageous to the race, or at least cannot be claimed to be of the same or even similar advantages as the peculiarities of their immediate companions in Series IV.

If one is healthy, the other is not, if one is under favorable conditions, the other is not, if one is selected, the other cannot be, and yet the peculiarities of both are perpetuated alike in all cases.

Semper's researches on Lymnæus show that the shells were dwarfed when under unfavorable conditions as to temperature, food, etc., and these again may be cited here in support of the above.

This law of heredity is then certainly independent of the question of advantage or disadvantage, as well as of the physical character of the surroundings, since it takes no account of how a character becomes fixed in the organization, but only of the fact that it is fixed and proceeds to transmit it to earlier and earlier ages, making in course of time a type either wholly deformed and retrogressive, or wholly well formed and progressive with equal indifference.[8]

If this conclusion be the true one, we see at once that the differences of the four ancestral varieties are acted upon by natural selection, only so long as there are competing varieties present.

When the competing varieties ceased to have any effect upon the descendants of the successful individuals by their intercrossing with them, we find the differences like all other characteristics inherited according to the same invariable law.

The general resemblances of the forms in the different series are largely due to the tendency which each series presents of becoming more and more unsymmetrical in the spiral or turreted.

Thus figs. 24 and 7, figs. 20, 11 and 6, figs. 28 and 23, figs. 25,

[8] Physicians will at once apply this to the history of congenital diseases, and some of them may perhaps be able to cite instances where diseases have appeared in accordance with this law at later stages in the development of the descendants of a diseased ancestor. I hope some will be induced to communicate such cases, since I have been trying, hitherto, with very indifferent success, though assisted by the Mass. Board of Health, to gather statistics of all cases in which the approximate age of their first appearance in an ancestor or parent, and that of their appearance in the descendant or child, can be stated.

21, 13 and 3, present different degrees of this phenomenon, which is generally known as the appearance of similar or representative forms as they are called in distinct genetic series.

The original ancestors, it will be observed, are much flatter or more bilateral than any of their descendants, and there are some varieties of these, like figs. 24, 25, Plate 2, which are really bilaterally symmetrical, that is not in the least degree turreted, as in fact are the young of many of the lower species. Not only in each individual mollusk, which has an unsymmetrical shell in the adult, is there a change during growth, but in many groups there is a traceable gradation of forms from those which are more or less bilaterally symmetrical to those which are more or less laterally unsymmetrical.

Darwinists would say that want of bilateral symmetry or the unsymmetrical spire was of advantage to the animal, therefore it was selected and perpetuated.

Now this statement could be readily accepted, if it were not for the "therefore" implying a relation of causation.

Most of the characteristics caused by the physical surroundings are of course advantageous, but the physical forces are the causes and not the advantageous or disadvantageous nature of the characteristic.

The form of the embryonal shell is straight, bag-like or swollen, tubular; figs. 29–30, Pl. 1.

The simplest form of all is a disk with which this embryonal bag begins, but this or the embryonal shell it is not necessary for us to consider.

The coiled unsymmetrical shell (*b a*) is carried above the foot (*k*) as in the *Helix pomatia*, fig. 8, Pl. 2, and Planorbis, fig. 8 *a*, Pl. 2. This is built by the mantle or internal soft covering of the body *d*, fig. 9. The shell has been removed, and the fleshy cone of the mantle containing the stomach, intestine, etc., has been partly unrolled to show that it was originally coiled up inside of the shell. The structure of the shell can be more easily understood in forms like figs. 11, 12, Pl. 2, where the mantle is not so long and not coiled, but builds a broad, evenly balanced, conical roof above the foot as in the Patella or Limpet. If this shell be divided into halves and one half removed as in fig. 12, the structure of the shell becomes visible and the relations of the mantle, *d*, and the

mantle border, d', to each other and to the two layers of the shell, e, and e'.

This animal was once small enough to occupy only the upper part of this cone and then as it grew in size built the shell above itself.

The outer layers, e, fig. 12, the outer edges of which are seen also on the surface of the shell, ba, fig. 11, were plastered up one inside of the other by the mantle border, $d+$, which exactly fits the last one formed, and the inner, longer, layers e' which simply serve to strengthen and support these are laid on by the mantle itself, d, fig. 12.

The mantle border in the gasteropod forms a sort of collar, $d+$ figs. 8 and 12, Pl. 2, around the edge of the mantle, through which the creeping disk or foot projects when the animal is expanded.

The mantle border among the lamellibranchs $d+$fig. 18, Pl. 2, forms a wider opening[9] or slit fore and aft, but it serves the same purpose of an aperture for the protrusion of the foot, when this is used as an external organ of motion, fig. 18, Pl. 2.

Any force, which would confine or interfere with the excreting surfaces of the mantle border, would affect the form of the imbricated layers e' which determine the shape of the shell, and thus change or curve the form of the cone. The weight of the shell itself or gravitation is such a force. If we try to account for the regularity of the spirals, whether bilateral or unsymmetrical, we are struck by the fact of their great regularity of curvature, and that this regularity can only be accounted for as the result of some general and constantly acting physical force, which tends to make the bag-like, straight shell of the young bend first into a bilateral spiral and then into an unsymmetrical spiral.

The force of gravitation unless counteracted by great muscular strength, or the equilibrium of a perfectly cone-shaped, erect shell, as in some mollusks, would make the growing shell hang back and weigh upon the hinder portion of the border of the mantle, thus compressing the excreting surface in that quarter and decreasing the breadth of the shell layers built by this part.

This would make the shell assume the form of a bent cone or the bilateral spiral as in figs. 24–25, Pl. 2.

The unsymmetrical spiral would be occasioned by any additional

[9] See also fig. 8, a fresh water clam (*Anodonta*) thrown widely open.

inequality in the weight of one side over the other, which could be occasioned by the distribution of the heavier internal organs, particularly of the stomach, ovaries, etc. Any irregularities of weight on one side more than the other would, it is obvious, also compress that side as well as the back part of the mantle border, and tend to narrow the deposits.

This would occasion a deflection laterally, and we should have, what is so commonly the case, a shell bilateral in the young or at the apex becoming by growth unsymmetrical or spiral.

This explanation is obviously applicable to the regular spirals, but the test cases are the irregular spirals.

These occur through weakness occasioned by wounds, disease, fig. 21, Pl. 2, or old age, as in fig. 22, Pl. 2.

All of the distortions thus produced tend to be irregular, that is the animal becomes too weak to counteract the effects of the weight of the shell by its inherited muscular power, and it falls over more or less to one side destroying the regular curve of the spiral.

This falling over to the side of greatest weight occasions an irregularity in the deposition of the outer shell layers, and the shell becomes more and more irregular as the animal grows weaker in old age or through disease.

Another proof of the effect of gravity lies in the fact, that the irregularity of form of the shell is proportional to the extent to which it is supported and the excreting border of the mantle relieved from the effect of its weight.

Thus a perfectly regular spire in the young by being supported, is turned into an irregular meandering tube in the after growth of the same animal.

Magilus antiquus crawls freely when young among corals and has during this period a regular turreted shell, fig. 23, *ba*, Pl. 2. It becomes finally fixed in the growing coral which completely invests and supports it, and thereafter its shell is a rough irregular tube growing upwards in the direction of least resistance. The border of the mantle being free from compression on all sides deposits shell matter about equally all around in the specimen figured, and therefore grows upwards in a straight line.

The Vermitidæ are supported in various degrees in the adults, but free in the young. Their shells, therefore, though having a regular spiral in the young, are in proportion to the support

received, transformed into tubes more or less meandering after they become attached as in fig. 10 *a*, Pl. 2, or loose irregular spirals rising up like corkscrews and only supported on one side as in fig. 10.

The Ammonoids and Nautiloids are notable for the complete bilateral symmetry of their spiral shells.

Diseased specimens, which are not infrequent, however, tend to become unsymmetrical, coiling like the gasteropods, as is well known to all experienced palæontologists. These are commonly spoken of by Quenstedt, and others, as diseased or deformed or sick species.

Once at the Sorbonne in Paris, Professor Hèbert, a distinguished French palæontologist, showed me a magnificent series of uncoiled Cretaceous Ammonites, and by way of testing these conclusions, I asked this question.

"Where, M. Hèbert, is the closely coiled symmetrical form which ought to be found with these?" He turned to the other side of the room, and pointed out the required form, saying, "There it is, I found it last summer."

These and other facts of a similar kind indicate that, when physical surroundings become unfavorable to any organization, it takes on a certain series of retrograde transformations.

When they occur in the individual in the decline of life or prematurely in the course of growth, through disease, they are similar to the characteristics of whole species, and even groups of degenerate forms. As in the case described above, fig. 22, Pl. 2, taken from an aged specimen, but it is distorted in the same way as the diseased specimen, fig. 21, Pl. 2, and the retrogressive or degraded species of Series I, Pl. 1.

All the facts corroborating this assertion have already been published and are too numerous to be described in the limits of a paper like this.

The direction of the spiral is backwards[10] or away from the mouth in the Gasteropods and towards the mouth in the Lamellibranchs; in both, however, it is in the direction in which gravitation acts with greatest effect.

The Lamellibranchs have a split shell, with two valves. Each valve is unsymmetrically spiral, but there is one on each side of the

[10] In the Cephalopods it is backwards also, but with the ventral side turned out, the reverse of the gasteropod spiral.

vertical axis or *axis of gravity* so that they balance each other and together form a bilaterally symmetrical shell, figs. 15, 18 seen from the side and 16, 17 from behind; *a*, spiral of right valve and *b*, spiral of left valve.

The spiral and the outlines of the valves are equal on either side except in those forms which change the vertical axis and lie habitually on one side.

These are deformed and unequal; the deformation is in accordance with the amount of support and the resistance of the surroundings.

Thus oysters may grow to the right or left or very irregularly, fitting the curvature of surface, fig. 13, Pl. 2. In the young they are free moving, bilaterally symmetrical and for a time attached by a byssus,[11] and then lie over on one side.

Their symmetry is precisely accordant with these changes ending with having the lower valve permanently attached and larger and more concave than the upper.

Fig. 14, Pl. 2, shows the outlines of a clam shell above that of an oyster shell, and both in their real positions for comparison with each other, the beaks of the clam shell being upon the back and those of the oyster shell across the anterior or mouth end of the animal, which has built the shell. The positions of the mouth in each animal are shown at *l*, *l*.

Fig. 18, Pl. 2, shows one side of a clam supposed to be buried in the mud with the siphons extended to the surface and the mantle border or shell building organ *d* +, and the digging foot *k*, in their natural expanded condition.

What are the changes, which can take place, in a member of the oyster family by change of habit?[12] Can an animal of this family, which is always unsymmetrical when lying on one side, have a different position and thus return to the normal condition and have valves which are symmetrical and bilateral?

[11] The common mussel, fig 15, *Mytilus edulus*, remains in the condition throughout life. A' byssus.

[12] The effects of change of habit have lately been followed out by Dr. Anton Dohrn in an essay in which he shows the results of change of function induced by new conditions to be the transformations of the organs themselves. His hypothesis states that new habits bring about or induce the organs to exercise apparently new functions, which were latent or only partly developed under the original conditions of the surroundings.

See Der Ursprung und d. Princip des Functionswechsel, by Dr. Anton Dohrn, Leipzig, 1875.

This question is answered by Lima, one of the same group as the oyster, a free swimmer but also burrowing into sponges as the clam does into the earth. This change of habit produces a corresponding change in symmetry, and it becomes like the clam also perfectly bilateral, fig. 16, Pl. 2.

On the other hand, can one of the fresh-water clams, fig. 3, 17, Pl. 2, which are the reverse of the Ostreadæ, being almost invariably free moving or burrowing and habitually bilateral, become attached, and if so does its shell become distorted like that of an oyster?

The answer to this is Mulleria and genus Ægeria. Mulleria, fig. 19, becomes attached and is distorted so as to resemble the oyster and not only that, but the animal changes, since there is but one large muscle, *g*, as in the oyster, fig. 1, Pl. 2, in place of the two muscles used to close the shell in the clams *g, g;* fig. 18, Pl. 2, and the beaks of the shell have shifted from the middle of the back to the anterior end. Probably all attached animals show this tendency. Their attached and supported parts, the bases of the stems, etc., are irregular in form and growth, and their free upper parts more or less laterally symmetrical. The radiate symmetry of the soft bodies of corals and of the harder, plate-covered cups of the Crinoids, the attached parts of the Ascidians as compared with their freer bodies above; the perfect bilateral symmetry of the free moving parts of the mollusca, as in the Helix, fig. 8, Pl. 2, and Planorbis, fig. 8*a* as compared with their supported spiral shells, the same perfection in the free Eolis, fig. 2, Pl. 2, which has no shell in the adult, and in most of the Pteropods, fig. 7, Pl. 2, free swimming animals, as well as in the Cephalopods, fig. 5, Pl. 2.

One of the best proofs of this position lies where it is least to be expected. The Brachiopoda are attached by a peduncle, or fleshy stem, and the upper valve, and not the lower, is the larger, just the reverse of the oyster.

Upon examination, however, it is found that it is the upper valve which is held by the peduncle and the lower valve alone opens and closes. Then again Lingula, another type of Brachiopod, which is not fixed by its peduncle, but simply occupies a sand burrow, and can move its valves sidewise, one over the other, has equal valves. Prof. E. S. Morse's investigations have shown that the symmetry in these animals changes from a worm-like upright

young of three rings or segments, and becomes laterally symmetrical by subsequent changes in the form of the first and second segments, the third changing into the peduncle. Here then a round worm-like form exchanges its cylindrical shape for a flatter shell-covered body in two of its segments, which become bent over into a horizontal position while the third, which remains vertical, retains the original round tubular form.

The Anomia, figs. 31, 32, Pl. 2, presents a series of changes very similar in their meaning, though this animal is closely allied to the oyster.

It has the lower valve flat, resting upon and taking the form of the surface upon which it grows. The upper valve, *b*, figs. 30–31, is convex and larger than the lower concave valve, *a*, fig. 30, and is supported by a plug *h''*, passing through the lower valve. In this case in which also the upper valve is the larger, it is this which receives the direct support of the attached plug.

The young are at first free, then attached by a byssus. Figs. 26, 27, show the right and left valves at an age when the animal has fallen over on the right side, and the notch, fig. 27, begins to be formed.

Fig. 28 shows how the lower valve continues to build out around the notch towards the anterior end, and in figs. 29 and 30, it becomes complete. This greater demand upon functional activity of the lower side of the mantle, and the fact that this is the movable valve, explain why it is not larger than the upper valve as in the oyster.

As was pointed out to me by Mr. J. S. Kingsley the growth of the upper and lower valves sidewise is really an effort on the part of the animal to recover, by lateral growth in a new direction, the symmetry lost when it fell over on its side. Any one comparing figs. 28 and 31–30 will see that the long axis of the form in fig. 31 is at right angles to what it is in fig. 27.

I shall call this tendency, to equalize the form in the direction of a horizontal plane, *geomalic;* the downward tendency of the growth being designated by botanists *geotropic.*

The Anomia, when it falls over, loses its bilateral symmetry, because the right and left sides become upper and lower, and being in the vertical position, they are unequally affected by gravity and by change of function. At the same time the dorsal and ventral sides, which were before vertical, have become hori-

zontal; and the geomalic growth of the ventral side, in order to restore the lost equilibrium in a horizontal direction, at once begins. This does not attain perfect lateral symmetry. The form of the animal cannot be easily changed, and the dorsal and ventral sides are still distinguishable; therefore, it is necessary to call this tendency of the growth by a new name, so that we can speak of the dorsal and ventral as well as the right and left sides as having the same tendency to assume by geomalic growth the natural and inevitable conditions of equilibrium. The experienced observer will at once think of many apparent violations of this law.

Many larval forms of Gasteropods begin to build the spiral, while they are still within the egg, or still free swimming animals.

Balfour [13] states, in his masterly summary of Comparative Embryology that, during an early larval period, "in most Gasteropods, the shell and mantle extend much more to the left than towards the right side and that the commencement of the spiral shell is thus produced." This same explanation applies to the falling over on one side of the young Anomia and oyster, in so far as the side upon which they fall is heavier than the other or upper side.

These seem to be readily accounted for as the direct results of hereditary peculiarities which have arisen in their ancestors and become embryonic through the action of the law of quicker development or acceleration explained above, and into the same category comes also the straight anomalous bag-like shell or plate of the embryo, and the split bivalve shell of the young oyster, which according to Professor Brooks is never an embryonic plate as in other Lamellibranchs.

Any and all of these anomalies might be produced during these earlier stages, as the results of direct inheritance during the protected periods of growth in the young.

The cases of Ianthina and spiral shelled Pteropods which, being free and having the shell pendent in the water, ought to have perfectly bilateral or balanced shells, are however not explicable on this hypothesis, unless it can be shown that they are highly specialized pelagic forms descended from the ordinary creeping gasteropods or their larva, and still holding in some persistent species their modes of growth.

Balfour [14] summarizes our knowledge of these forms as follows:

[13] Comparative Embryology, vol. 1, p. 190.
[14] *Op. cit.* p. 199.

"Although many of the adult forms are symmetrical, there is very early asymmetry visible in the larva, showing that the Pteropods are descended from asymmetrical ancestors."

He writes, on page 198, with relation to the remarkable free swimming forms of the Heteropods or Nucleobranchs, that "they have larvæ like ordinary Gasteropods." Carinaria, one of these Nucleobranchs, floats in the open sea and has the shell down, but unlike the Pteropods with swimming wings as in fig. 7, Pl. 2, they have a long foot above the shell containing most of the organs of the body.

It is a very significant fact that the young shell in this and several other Heteropods is spiral before it assumes the boat-like bilateral symmetry of the later stages of growth.

Other genera show that the spiral shell may persist in the same way until quite a late period of individual development and the shell still become subsequently bilateral as in Atlanta.

The Nucleobranchs have also other peculiarities which indicate that they are degraded or highly specialized forms descended from ordinary shell-covered Mollusca.

Among Mollusca, the Cephalopods, which have very light chambered shells, have always from their first appearance in the Cambrian possessed bilateral spirals, except in diseased forms and degraded species resembling them. The Gasteropods, a crawling type with their solidly filled cones carried above, have had the shells covering the part generally deflected into unsymmetrical spirals, with the apex canted backwards, for an almost equally prolonged period. The Lamellibranchs, a burrowing type also very ancient in origin, have split and movable shells, the apices and the spiral reversed, pointing anteriorly so far as the animal is concerned, but still held in precisely the direction they ought to assume, if their growth were determined by gravitation. All of these facts are very favorable to the assumed proposition.

If we consider the growth of any body with reference to its fixed hereditary form and also to its relations to gravity, we find that effort must necessarily be in six directions.

Vertical against gravitation, and down with gravitation, laterally to the right and left for the attainment of equilibrium, and forward and backward in the direction of locomotion.

The forces being thus unequally divided must, if they have any effect, necessarily tend to produce a difference in symmetry between

the bases and the parts above; and supplement, if they did not primarily determine, the differences which have arisen between the upper and lower sides of animal bodies.

The same proposition must in some measure be true of any animal whatever in the direction of the true vertical axis which passes through its centre of gravity and the centre of the earth.

The fact will be granted that nearly all animals and plants differ in these parts above and below, whether gravitation be admitted as one of the causes of this or not.

The force of gravitation will act equally at all equal distances, to the right and left of a vertical plane passing through the centre of gravity, and the axis of the form.

The axis of the form must necessarily be the median line drawn through the morphological centre of the external outlines, and a plane passed through this axis and through the centre of gravity divides the body, in almost all free moving animals, as nearly as possible into halves.

At all equal distances from the centre of gravity on either side of this plane, the attraction ought to produce equality in the effort to use the organs of both sides, for the support or movements of the body. That this effort would result in the bilateral growth of the limbs, etc., cannot be definitely proven; but a fact most significant in this direction is the origin in pairs of so many of the external and internal organs and parts. These, which are in many cases derived from what are at first folds of the endoderm as are the glandular organs, liver, etc., or from external budding or folds, arise, like the limbs in pairs, except when they, as in the case of the median fins of fishes and batrachians, the allantois and bladder or the alimentary canal, are situated in the median vertical plane.

Bergemann and Leuckart in their compendious work "Anatom.-physiolog. Uebersicht d. Thierreichs, Stuttgart, 1852, p. 390; Chapter on "Die Vertheilung des Gewichts in beweglichen Thierkörper," though offering no explanation of the facts, describe the distribution of the organs according to their weight and their bilateral relations, and announce that this is an essential condition of all effective motion. "Eine andere Einrichtung würde einen nutzlosen Kraftverbrauch zur Folge haben, die Bewegung behindern oder gar unmöglich machen." "Another arrangement would be followed by a useless expenditure of force, and either hinder locomotion or make it impossible."

As a fact, however, exact bilaterality is rarely attained. The sides are used differently in many animals producing as in man greater growth on the right than on the left, and in the lobster, fiddler crab, etc., considerable differences between the large claws of either side.

These cases, in which the effects of effort are apparent, also help us to understand how the great want of symmetry between the anterior and posterior ends arises in all animals in which the axis of the form is at right angles or inclined to that of gravity. It is of course undeniable that, so far as gravity can have any effect, the anterior and posterior ends of any horizontal body ought to be in equilibrio as are the sides, unless functions of these two portions acted so as to produce the inequality, which in most animals, distinguishes them.

The mouth end, which is in constant use for the seeking of food, which first encounters danger, etc., naturally becomes through these efforts the seat of all the organs relating to these most important functions.

If we add to this as a result of the same cause the tendency of the limbs to be concentrated towards the head, and the inevitable increase of the power and size of the cephalic ganglia through use, we have sufficient cause for the appearance of the head in the Gasteropoda, food-seeking animals, as contrasted with Lamellibranchs, and with many divisions of sedentary animals, which like them are so organized that they also have their food wherever water flows past them loaded with minute forms of larva, Protozoa and microscopical plants.

The same effort to use the mouth perpetually in free moving animals, which brings that end forward, must eventually lead to this concentration of organs and functions, and perhaps to their origin primarily in this part of the body, as well as to the enlargement and functional perfection of the brain, and the increasing discriminative powers of the surfaces in which dwell the senses of sight, hearing, taste and smell.

When there is less functional difference in the use of the two ends as in the common earth worm and many other worms, and particularly among those spindle-shaped, globe-like and spiral-form Protozoa, some of which carry the mouth in the centre, there is a greater likeness of the two extremities, and the geomalic tendencies of the forward and hinder ends become more apparent.

The rounded and fusiform outlines of the Diatoms and the peculiar forms of Desmids and of the Protozoa show that when the two ends of an organism are not distinguishable functionally, they are apt to be in equilibrium in the same proportion as the sides; and that, as in Volvox and many other forms, there may be spherical symmetry more perfect than that of a primitive cell.

It also ought never to be forgotten that the materials in the egg arrange themselves according to the laws of gravity, and with few exceptions the germinative disk at first, whether elongated or rounded, has equal ends as well as sides.

This problem has not been approached among animals as it has among the plants by Sachs, Darwin and others, and we do not know how to distinguish between the direct effects of gravitation upon the growth of any animal at any one stated period of its life, and the effects of the proximate causes arising from the inherent tendencies of heredity.

Notwithstanding these imperfections in the evidence, and the absence of experimental proof, it has appeared to me that the discussion of this question would not be without usefulness in calling attention to what seems to me one of the most fruitful lines for experimental research. This, though now attracting much interest among botanists, on account of Sachs' experiments and Darwin's last book, is neglected by zoölogists.

In conclusion, permit me to summarize what I have endeavored to condense in this brief communication.

I have tried to show the results of the study of the history of a single species, *Pl. levis*, and its evolution into many distinguishable forms of which 14–19 may with justice be called by different names and considered as distinct species.

I have also striven to bring into comprehensible shape the following conceptions.

First, that the unsymmetrical spiral forms of the shells of these, and of all the mollusca, probably resulted from the action of the laws of heredity, modified by gravitation.

Second, that there are many characteristics in these shells and in other groups which are due solely to the uniform action of the physical influence of the immediate surroundings, varying with every change of locality, but constant and uniform within each locality.

Third, that the Darwinian law of Natural Selection does not

explain these relations, but applies only to the first stages in the establishment of the differences between forms or species in the same locality. That its office is to fix these in the organization and bring them within the reach of the laws of heredity.

Fourth, that after this is done, they are inherited according to the law of heredity with acceleration, which shows us in what manner these differences and all other inheritable characters, however originated, may with greater or less rapidity, become incorporated in the young of descendant forms and species.

Fifth,[15] that in these earlier stages they are more or less protected from change and may therefore remain comparatively invariable through long periods of time, or may be exposed to great changes in exceptional cases and modified accordingly.

Sixth, that these phenomena show, that in growth, reproduction, and heredity by acceleration, there is manifested a decided reaction of the organism, which succeeds in building up and maintaining the type structure and form, under all ordinary or normal terrestrial conditions, but in some cases fails in fully accomplishing this when exposed to exceptional surroundings, as in some cases of parasitism.

Seventh, that gravity appears to be one of the causes of the differences in effort, function and anatomy observed between the sides or ends of animal forms when in vertical relations to each other and to the earth, whether these be the anterior and posterior ends of the form, or the dorsal and ventral, or the left and right sides.

Eighth, that the bilateral or geomalic growth observed in the internal organs and the external parts of organism when their sides are in their original and hereditary positions, and the geomalic growth of the dorsal and ventral sides, when these become horizontal through change of habit, appear to be, directly or indirectly, responses to the demands of gravity.

Ninth, the origin of the limbs, etc., in pairs while mere buds, is difficult to account for, if they are not considered as the results of the efforts of the tissues of animals to maintain the equipoise of all the parts by geomalic growth in obedience to the laws of gravity.

[15] The fifth and sixth propositions are not discussed in this communication for want of space, but were given in the Memoir on Steinheim Shells above quoted and in the evening lecture of which this is an abstract.

EXPLANATION OF LETTERS.

a, right valve. *b*, left valve. *ba*, shell. *c*, hinge area. *c'*, hinge ligament. *d*, mantle. *d+* mantle border. *d'*, inner limit of mantle border. *d''*, pallial line or trace made by *d'* on the inner side of the valves. *dx*, siphon or double funnel of clam. *e*, outer layers of shell built by the mantle border. *e'*, inner layers of shell built by the mantle itself. *f*, beaks or young valves, still remaining to form the beaks of the two valves. *fa*, right beak. *fb*, left beak. *f'*, beak, apex, or young shell of Gasteropod still remaining at the centre of the spire in Gasteropods. *g*, impression or mark made by the posterior adductor muscle on the interior of the valves. *g'*, position of anterior adductor muscle, shown through the shell only by a dotted line. *h*, depression or hole made by the byssus or byssal plug in the lower valve of Anomia. *h'*, byssus of threads. *h''*, byssal plug. *k*, foot or crawling disk. *k'*, tentacles or feelers. *k''*, division between foot and head. *l*, position of the mouth. *m*, palpi, or flaps for conveying the food into the mouth. *m'*, right pair. *m''* left pair. *n*, gills. *n'*, right pair. *n''*, left pair.

PLATE 1.

The specimens from Undorf all belong to an older tertiary period than that at Steinheim. Pl. 1 is copied from Pl. 9 of Memoir above quoted, except figs. 29–30.

SERIES IV.

Fig.	1, Pl.	levis, Undorf.	
"	2, "	Steinheimensis,	Steinheim.
"	3, "	tenuis, / Steinheimensis, [16]	"
"	4, "	tenuis,	"
"	5, "	discoideus,	"
"	6, "	trochiformis, / discoideus,	"
"	7, "	trochiformis,	"

SERIES III.

"	8, "	levis, Undorf.	
"	9, "	oxystomus,	Steinheim.
"	10, "	supremus,	"
"	11, "	" var. turrita,	"

SERIES II.

"	12, "	levis, Undorf.	
"	13, "	crescens, / parvus,	Steinheim.
"	14, "	crescens,	"
"	15, "	"	"

[16] Names written in this way indicate the transition forms, as in this case the variety connecting Steinheimensis with *tenuis*.

SERIES I.

SUB-SERIES 3.

Fig. 16, Pl. levis, Undorf.
" 17, " minutus, / levis, } Steinheim.
" 18, " minutus, "
" 19, " triquetrus, "
" 20, " " "

SUB-SERIES 2.

" 21, " minutus, Steinheim.
" 22, " denudatus, / minutus, } "
" 23, " denudatus, / minutus, } "
" 24, " denudatus, / var. distortus, } "

SUB-SERIES 1.

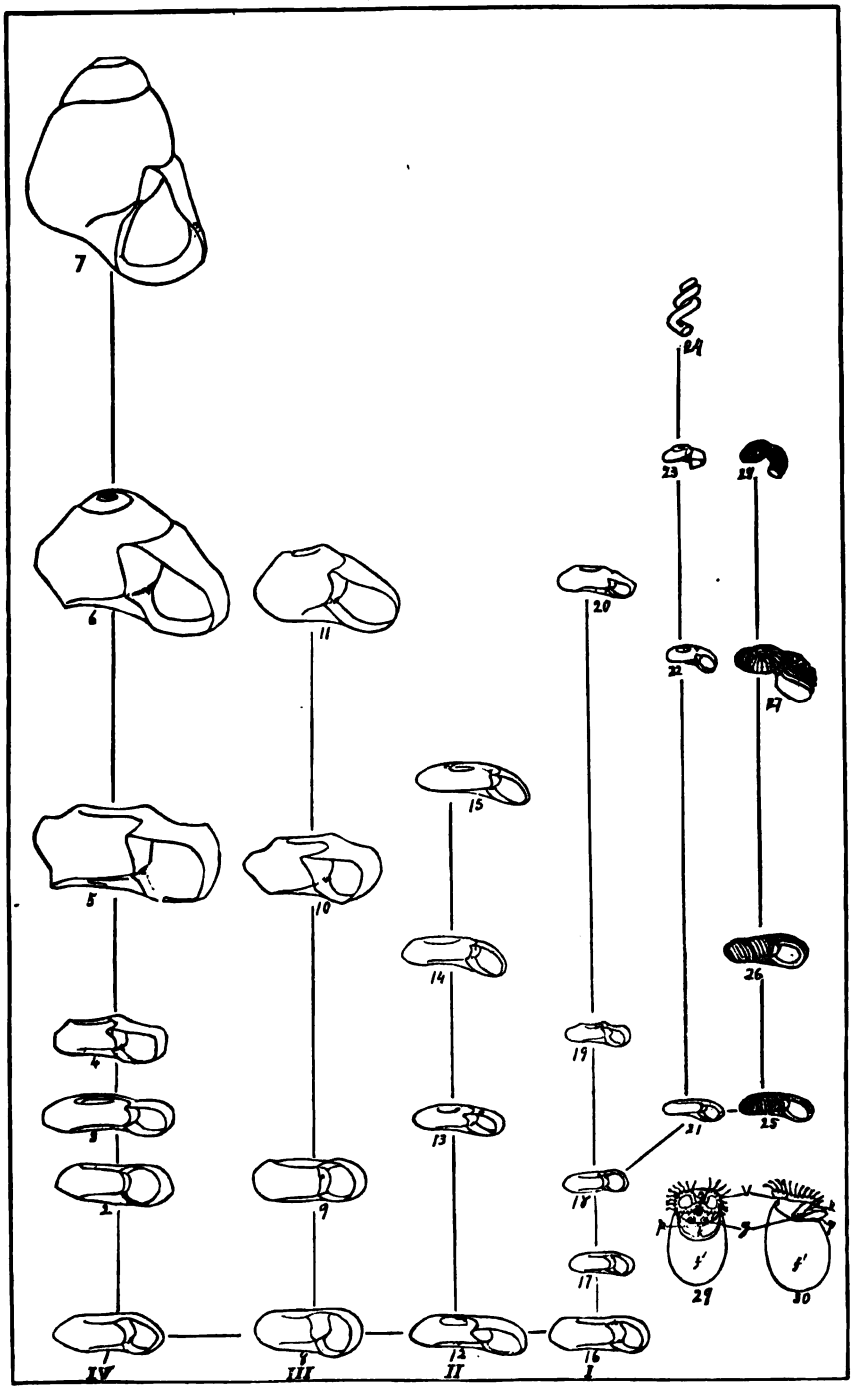
" 25, " costatus, / minutus, } Steinheim.
" 26, " costatus, "
" 27, " costatus var— "
" 28, " costatus var— "
" 29, Doto coronata? young showing the oval shell and the swimming organ or velum from above. *V*, velum, *y*, eyes, *p*, operculum, the hinged disk used in closing the opening of the shell when the velum and foot *k*, are drawn into it as the animal retracts. The mouth is indicated by the black spot near the centre of the velum.
" 30, View of the same taken partly from the side.

PLATE 2.

Fig. 1, Left or lower valve of Oyster (fig. 13*b*).
" 2, Eolis seen from above (after Adams).
" 3, Anodonta with shell open.
" 4, Young of Oyster (after Brooks), abdominal view.
" 5, Loligo pallida (after Verrill), dorsal view.
" 6, Limax, side view.
" 7, Cavolina, dorsal view (after Brown).
" 8, Helix, seen partly from the under side.
" 9, The same with the shell dissolved by acid, showing the conical bag of the mantle partly unwound.
" 10, Vermetus tricarinatus, group of.
" 10*a*, " a broken specimen winding about on a shell of Lima.
" 11, Patella, diagram showing relation of shell to the crawling disk.

Fig. 12, Same, edge of shell in section, and mantle exposed on one side.
" 13, Oyster in natural position attached firmly to a stone, but projecting without other support, for about ¾ of its own length.
" 14, Clam shell (Mya) in outline above an Oyster shell and both in the same position with reference to the structure of the two animals, if these were in their places inside of the shell.
" 15, Mytilus edulis (after Morse), common mussel attached to a stone by its byssus with the foot protruded.
" 16, Lima, view from the posterior end, showing bilateral symmetry.
" 17, Anodonta, same.
" 18, Mya arenaria. Common clam seen from the side with the siphon and mantle-rim or foot extended.
" 19, Mulleria, right or attached valve, with the solidified beak and the young shell showing at the end of the beak (after Adams).
" 20, Beak of the same showing the form of the young shells, from the dorsal side.
" 21, Pl. oxystomus, var. reverteus, spiral showing the unwinding in an adult individual, due to disease.
" 21*a*, Top view of fig. 21, and showing also the nucleus or young shell.
" 22, Pl. oxystomus, extremely old specimen showing similar unwinding of the spiral due to the weakness of senility.
" 23, Magilus antiquus, showing the regular spiral below and the irregular straight shell, above, built after the spiral shell has been buried in the coral (after Woodward).
" 24, Pl. Steinheimensis, var. æquiumbilicatus, view of section showing how perfectly balanced the two sides of the spire are in some shells during their entire growth.
" 25, External view of the same shell, seen from ventral side.
" 26, Anomia, young shell, left valve (after Morse).
" 27, Anomia, young shell, right valve (after Morse).
" 28, Anomia, young shell, right valve after the shell is turned so as rest on this valve, which is now below and is making an effort to readjust the lateral symmetry by the geomalic growth of this valve around the byssal plug (after Morse).
" 29, The same, with the same valve after it has been built around the byssal plug (after Morse).
" 30, Anomia glabra, a full-grown shell, seen from the lower, right side, with upper left valve, showing its projecting edges and beak beyond.
" 31, The same from the upper or left side, lower shell necessarily concealed.
" 32, The same from the anterior end to show distortion produced by the change in position. The lower or right valve is too concave to be visible.

[All figures not otherwise designated are original.]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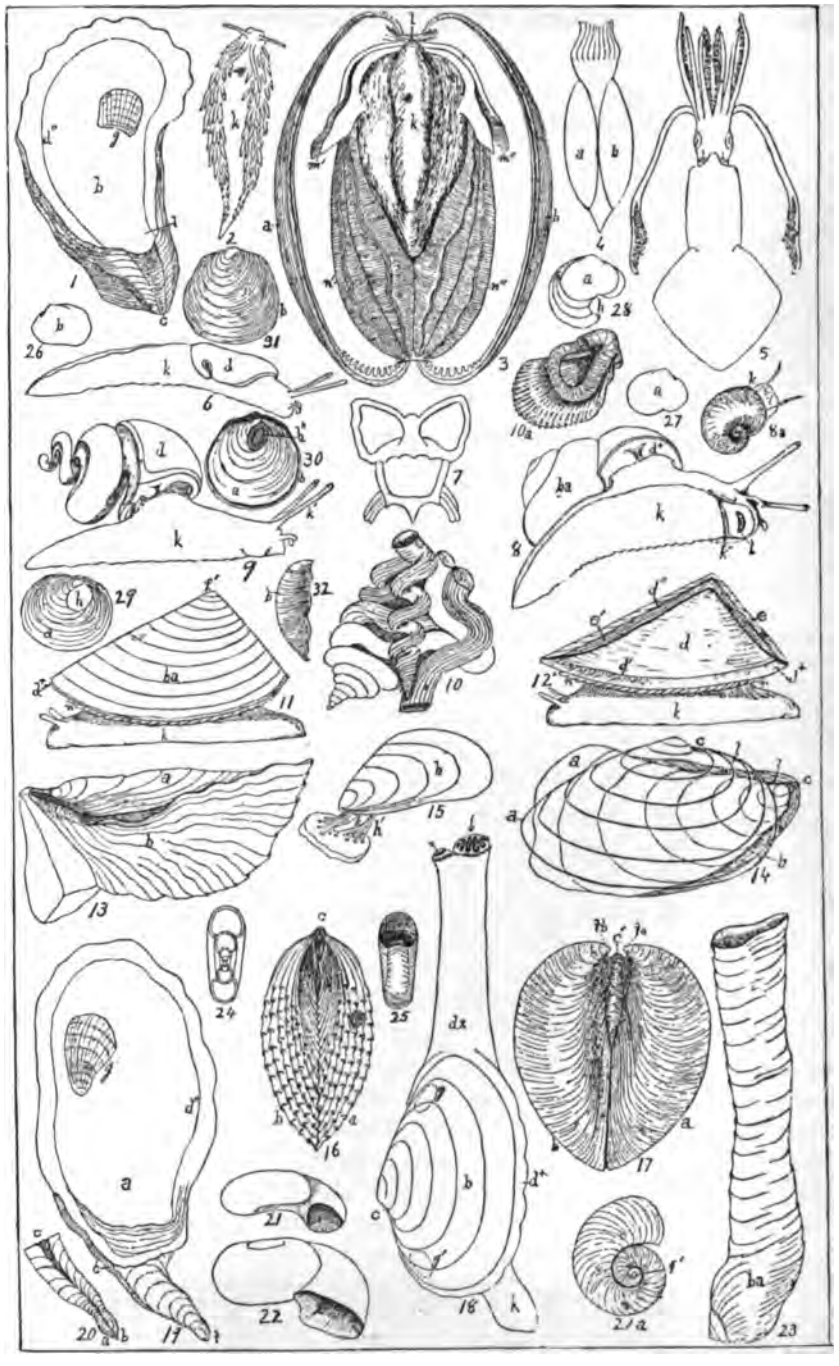

On the Study of Comparative Anatomy as a part of the Medical Curriculum.

By Harrison Allen, of Philadelphia, Pa.

If I am correctly informed, the meetings of this Association prove valuable to its members not only by reason of the facilities afforded for the announcement of discoveries, but also by the opportunities presented to teachers and others, interested in education, to discuss the best methods of conducting courses of instruction in natural science. It occurred to me in this connection that a short account of an experiment in teaching comparative anatomy to medical students might not be unwelcome, particularly since the experiment was conceived upon the didactic plan, and is now finished. I desire to emphasize the didactic nature of the experiment, for it is not likely that a similar series of lectures will again be given,—at least while the drift of opinion for demonstrative teaching conjoined with laboratory work continues to be so influential.

Comparative anatomy was founded by John Hunter, a practising physician and surgeon. The researches of this remarkable man tended at all times toward his own profession. With him the study of the structure of animals was a part of the self-imposed task, the performance of which made him not only the great comparative anatomist and physiologist, but the great surgeon as well. No name is more revered in the annals of English medicine than that of Hunter — Harvey alone excepted. It is doubtless largely owing to this circumstance that comparative anatomy has so long held a place in the medical curriculum of the British schools, and that the institutions in this country, based upon the British model, early introduced it. The University of Pennsylvania,—which imitated the University of Edinburgh,—in its early history essayed such a course of instruction, but subsequently abandoned it. The McGill University, at Montreal, naturally imbibed the English methods of teaching. It yet continues to exact of her candidates for the medical degree satisfactory proof that they possess a fair knowledge of the anatomy of the lower animals. In our colleges efforts have from time to time been made to introduce comparative anatomy but without the exercise of any great vigor. As a rule it may be said its claims have been ignored, and indeed, for various reasons actively attacked. The objections thus

far urged have come from three separate directions. First, from the teachers in the preparatory schools, who claim that the study of animal structure and habit should be completed before the student matriculates in medicine. Second, from physicians interested in comparative anatomy, who do not believe that the subject is of sufficient importance to take the time of the student who is preparing for the difficult practical art of healing, and who is already overworked. Third, from those who believe that the medical man is all the worse for possessing the knowledge, and should under no conceivable circumstances be encouraged in getting it. In the second group of the objectors, I may place the name of my friend Professor Wilder,[1] and in the third that of Professor Huxley.[2] I hope the experience to be recorded here will satisfy the objectors mentioned under the first and second heads, and possibly receive qualified sanction from the stalworth nihilist included under the third.

In 1865, Dr. George B. Wood, of Philadelphia, founded a chair of Medical Zoölogy and Comparative Anatomy in the University of Pennsylvania. It was particularly enjoined by its founder that these subjects should be taught with special reference to their medical relations. The course was not to be a part of a preparatory training to medicine but one coincident therewith. Dr. Wood had been a distinguished professor in the medical department,—having held the chairs of Materia Medica and Practice of Medicine. He wished special attention to be given to the parasites of man and to the articles of the materia medica procured from animals. Acting upon this instruction, I gave an outline of the animal kingdom, and elements of medical zoölogy in each course of lectures, giving in all between the years 1865 and 1879 thirteen courses of lectures, of thirty-six lectures in each. I made prominent in the lectures the features already mentioned — but added thereto expositions of the great generalizations that have been from time to time announced in general anatomy and biology. Thus separate lectures were given upon the subjects of Bilateral Symmetry; on the meaning of the terms "generalization" and "specialization;" on Homology; on Evolution. Attention in addition was directed *in extenso* to the tendencies exhibited by

[1] New York Medical Journ. Oct. 1877. 33, concluding paragraph.
[2] Professor Huxley in America. N. Y. Tribune Extra, 1876, No. 36, p. 10.

the hard tissues of animals, such as the bones in vertebrates, and the axial rods in various invertebrates, to undergo transverse segmentation; to the tendency of portions of such rods to be twisted upon themselves; to the causation of articulations, etc. I deemed it my province to direct the attention of medical students to the importance of these topics, and, with the object of making the series of direct value to them, drew my illustrations, whenever practicable from human anatomy, physiology and pathology.

A separate group of subjects was next treated. It embraced the themes of Variation, Teratology, Compensation, etc. In this group instances of variation in the human subject were taken up, described and explained. The object of this series was to awaken interest in the study of variations of human anatomy as met with in the dissecting room or the practice of medicine, and to enable the medical man properly to appreciate and describe the various congenital monstrosities that may come under his notice. In addition to these I usually gave one lecture on the medico-legal value of a knowledge of the skeleton (pointing out the characteristics of the bones of the different classes of vertebrates as determined by the study of fragments of bone) as well as one on medical credulity. In illustration of which subject, I pointed out fallacies in medical observation. Every physician, who has become identified with biological studies, can mention many instances in which his services as expert have been requested upon subjects as simple in their nature, as the error in observation has been gross and mortifying.

It will be remarked that the medical emphasis is everywhere insisted upon in a course of lectures such as that which has been briefly outlined. Would it not be entirely improper to attempt the delivery of such a course, before classes of young men in the literary departments of our colleges? Doubtless instruction in comparative anatomy as a branch of general education, or one preparatory to medicine, is of great value. But I am anxious that such a course should not be held to be identical in character with the one I have thus described. The student who proposes to attend, to the best advantage, a course of such lectures should already have attended a general course in biology, and have familiarized himself with the rudiments of human anatomy and physiology. It has been commonly said to me by my students—most of

whom had already studied two years—that the lectures were difficult to follow. If it so proved to advanced pupils, it is easy to foresee what would be the result of lecturing on such basis to classes preparing themselves for matriculation, or even for studies of the second year. The explanations of the generalizations of biology, the varieties of human anatomy, the abnormalities of structure — involving the most abstruse problems in biological science — cannot be essayed with advantage to other than advanced students. I have again and again stopped in the course of a demonstration and remarked that if there were any gentlemen present who felt themselves to be imperfectly prepared upon the human anatomy of the subject under consideration, such had best retire from the lecture room for it would prove for them a waste of time to remain. It was my custom to name at the close of each lecture the subject of the next — to invite special attention to the points to be therein treated—and to request my pupils to consult their text books before reporting for the lecture. By this plan I was free from the necessity of first demonstrating the anthropomorphic features before taking up the comparative.

The conviction that remains after having given much thought to the matter is, that the outline of the structure of animals as taught in our colleges of general education, or, in preparation for the special medical course, does not meet the end in view, but that a supplemental course should be given after the first or second session. If the time of the curriculum does not permit such instruction, one of two ways is open — either to make the course post-graduate or to lengthen the session. This is a matter of detail which need not interfere with the main point I advocate, *i. e.*, that the kind of comparative anatomy a student of medicine needs cannot with advantage be offered him until he is advanced in his special studies.

PLAN OF THE CEREBRO-SPINAL NERVOUS SYSTEM.[1] By S. V. CLEVENGER, of Chicago, Ill.

[ABSTRACT.]

1. *The primitive sense is tactile and all senses have proceeded from its differentiation.* For illustrative purposes let us consider energy as divided into molecular vibrations, from one ethereal pulsation in an eternity, to an infinite number of vibrations in one second. In such an undulatory series we may see, as a small division of it, all forces from sound to gravitation represented. While the protozoön may be visibly affected by every such undulation the homogeneity of its composition prevents any differential response; for instance, the tremor of a musical note, heat, light, electricity, alike produce contractions or expansions (motions) of its mass. In a higher form of life nerve tissue appears, which conveys only certain vibrations and rejects all others. Take one undulation in a second as the capacity of this nerve fibre. It is a tactile nerve. When a nerve-fibre conveys more rapid undulations differentiation begins. Sixteen to forty thousand per second begin and end the auditory vibrations. Quicker vibrations to four hundred and fifty billion per second we may view as heat appreciation, thence to eight hundred billion from red to violet light, above this fluorescent undulations, "chemical energy," electricity, to infinity. We may thus mathematically conceive an auditory sense derived from the general tactile or a special touch sense (like that of the fifth pair of nerves). An optic sense would arise from this same tactile, and we have seen it thus differentiated embryologically.

2. *Qualitative differentiation of the nervous organization proceeds dorsally, with a tendency toward the head end.* That portion of the animal which stands in most direct relation to the changing molecular movements of the environment develops the highest sensory and motor nerve-centres and projections.

3. *Repetition of parts of a system, up to a certain point, ceases; and these parts become commissurally united before another system is perfected.*

The sympathetic nervous system, consisting of the intestinal

[1] This paper is printed in full in the Journal of Nervous and Mental Disease, Oct. 1880.

and vascular or vaso-motor nerves, develops first. Schenck and Birdsall (*Archives of Medicine*, vol. I, No. 2), on the Embryogeny of the Sympathetic, consider this system as composed of masses originating in the central nervous system. This is a truth from one standpoint, and that a very narrow one. Blending the results of comparative embryology and anatomy, the sympathetic precedes the creation of other systems.

The second system to appear phylogenetically is the spinal, equivalent in the invertebrates to their "cerebral" ganglia.

The third system is the intervertebral, the swellings upon the posterior roots of the spinal nerves.

4. *The cerebellum is formed from fused hypertrophied intervertebral ganglia.*

Many sensory cranial nerves pass through this organ and by the fusion of these originally separate centres coördination occurs necessarily.

Excessive development on the one hand, or want of development on the other, places all the ganglionary tubercles and lobes of the encephalon in the third system category. Thus *the præ-frontal lobe of the cerebrum, the occipital and temporal lobes, the olivary body, the olfactory lobe, the mammillary eminence, the epiphisis cerebri, the tubercula bigemina, the petrosal and Gasserian ganglia were originally intervertebral ganglia*, and still maintain resemblance to these ganglia in many particulars.

5. *The præ-frontal lobe is the last intervertebral ganglion to develop.* It grows larger in the scale of intelligence and presses the occipital (see the brains of monotremes and marsupials) backward, downward and forward, thus forming the temporal (or what has been erroneously termed the middle) lobe.

6. The cerebro-spinal nerves, in some cases, preserve their original projections from and to muscles, but these nerves may also have not only a distribution to the viscera, as has the pneumogastric, but may also project into and from *other system-centres*. The lateral columns of the spinal cord, the tegmentum and crura cerebri in their main mass may thus be regarded as cerebro-spinal nerves of the highest series, having lower system-centres for peripheries. The præ-frontal lobes thus exert an inhibitory control over the highest centres, because such centres are peripheries for the nerves of these foremost ganglia.

Evidences of the Effect of Chemico-physical Influences in the Evolution of Branchiopod Crustaceans.

By Carl F. Gissler, of Brooklyn, N. Y.

During the winter months[1] *Eubranchipus vernalis* Verrill[2] occurs near Maspeth, Long Island, in immense numbers in large communicating ponds containing clear, yellowish fresh water. In January last I found, in a small and entirely isolated pool, less than a hundred paces from the above mentioned place, a number of perfectly colorless, smaller, but sexually mature individuals of branchiopod Crustaceans. The bottom of the pool is a white and very soft clay and the water itself is of a milky color. I collected a number, and observed the following characters:

A. Very few individuals of both sexes bearing, with the exception of transparent body, and the red furca[3] of the post-abdomen, the same characters as Eubranchipus.

B. A great number of colorless individuals from 15 to 22 mm. in length. These differ from the larger, red Eubranchipus in the following particulars: Cephalic scute[4] large and convex. Basal joint of male clasper cylindrical. Claspers crossing each other, short, tip of second joint with a blunt, minute tooth. Second joint more or less conical, tapering. A number of full-grown forms between sets A and B occurred, evidently transitory stages. I expect soon to give an account in Dr. Packard's Monograph on Phyllopod Crustacea, of the sexual organs (to be illustrated by a plate), copulation and the biology of these colorless individuals.

C. A single specimen of male Chirocephalus.[5]

D. A hermaphrodite. Sexual organs separate, both male and female claspers present.[6]

E. A single male individual with a minute tooth on the second joint of its right clasper,[7] tooth wanting on the left. Left clasper in normal position, right clasper twisted around, thus apparently

[1] From January till April.

[2] "Observations on Phyllopod Crustacea of the family of Branchipidæ with descriptions of some new genera and species," by Prof. A. E. Verrill, 1869.

[3] The normal red Eubranchipus has a white furca. Pale specimens of *Streptocephalus Watsoni* Pack. have also a red furca.

[4] F. Spangenberg's paper in Zeitsch. f. wiss. Zool., XXV, Supplem. 1875, p. 14.

[5] Proved to be a very large ♂ specimen of *Chirocephalus Holmani* Ryder. March 22, 1881, I found a large pond between Glendale and Ridgewood, Long Island, densely populated with both *Chirocephalus Holmani* Ryder and *Eubranchipus vernalis* (normal). The former is described in Proceed. Acad. Nat. Sciences of Philad., 1879, p. 200.

[6] I described it in American Naturalist, February, 1881.

[7] The claspers are the second pair of antennæ, transformed into auxiliary copulation organs.

preventing the animal from using it in copulation. The tooth is probably a substitute for the wrongly placed hook and assumes its function. This exemplifies Dr. Dohrn's theory [8] of the consecutiveness of functions in one and the same organ, brought about by evolution. As to the latter I refer to papers by Prof. Edw. D. Cope in the American Naturalist: "The Laws of Organic Development," 1871, and "A Review of the Modern Doctrine of Evolution," 1880. Dr. Moritz Wagner's migration theory [9] as well as Dr. Chas. Darwin's selection theory [10] may be employed to explain the occurrence of the above-mentioned sets, A, B, and probably C. First, I must mention that, on keeping a number of Eubranchipus, ♂ and ♀ (the latter with ovaries filled and oviducts empty) [11] together with a number of sets A and B, ♂ and ♀ (♀ in the same condition) during five days, I could never observe a single case of crossing: on the contrary the two (red and white) avoided each other, and only copulated among themselves. Now, as to set A, I consider them to be the first generation of Eubranchipus, brought along with mud into the little clay pool, by waterbirds from the neighboring larger ponds. (See Prof. J. A. Ryder's note in Am. Nat. XII, page 703). The transparency of their bodies was produced by the chemico-physical influence [12] of the little clay pool and not by "Mimicry." As the pool is an isolated one, there was no chance for the absorbing (or obliterating) influence of crossing with the original red Eubranchipus; consequently, the offspring of the new colorless *race*, influenced by different factors, was liable to submit to still further evolutionary transformations which I believe are realized in set B. The animal gradually degenerated into a much smaller one with the above-mentioned characters. The factor that produced it was a conservative one, favoring the preservation of this new species.[13]

[8] "Der Ursprung der Wirbelthiere und das Princip des Functionswechsels." Genealogische Skizzen von Dr. A. Dohrn, 1875.

[9] "Die Darwin'sche Theorie und das Migrationsgesetz der Organismen" von Dr. Moritz Wagner, 1868. The refutation of Wagner's law of migration was attempted by my former tutor, Professor Dr. Aug. Weismann ("Ueber den Einfluss der Isolirung auf die Artbildung" 1872); owing to a misconception of Wagner's paper he combined his theory with Darwin's selection theory, whilst both theories considerably deviate from each other as regards the compelling mechanical cause. See also Kosmos, IV, April, 1880. "Ueber die Entstehung der Arten durch Absonderung," von Moritz Wagner.

[10] "On the Origin of Species by Means of Natural Selection," 1859.

[11] F. Spangenberg in *op. cit.*, pages 51 to 56.

[12] W. J. Schmankewitsch in Zeitschr. fuer wiss. Zool., 1872, 1875 and 1877.

[13] Professor Huxley's "The Crayfish:" "In a strictly morphological sense, a *species* is simply an assemblage of individuals which agree with one another and differ from the rest of the living world in the sum of their morphological characters."

The factor that produced the individual E was a compelling mechanical cause, originating in a pathological condition. According to Dr. Chas. Darwin the mechanical cause enters into activity with the appearance of "favorably varying" individuals whose morphological deviations are either inherited or adapted.

As to C, the genus Chirocephalus, I have reason to suspect in it the winter form of Streptocephalus. The successive appearance of Chirocephalus and Streptocephalus in one and the same pond near Woodbury, N. J. (Am. Nat. XII, 703, J. A. Ryder), rather strengthens my assumption.

The hermaphroditic form D shows characters closely relating it to set A. From the study of comparative anatomy it follows that Hermaphroditism, *i. e.*, the coëxistence of both male and female sexual organs in one individual, is the primitive condition of sexual differentiation, which may in time be followed by a complete separation of the sexes. (See Dr. E. Haeckel's Anthropogenie, pp. 395, 681, etc). Hermaphroditism[14] and Parthenogenesis[15] can be regarded as cases of Atavism,—as a re-occurrence of former primitive conditions. Further progress in differentiation, of the sexual conditions Hæckel ascribes to division of labor (Arbeitstheilung). The bilateralism in this hermaphrodite indicates close relationship and coördination between the sexual organs and auxiliary copulation organs.

According to Dr. Chas. S. Minot's theory (Am. Nat., XIV, Febr. 1880), it is possible that a male genoblast was formed by the splitting of a neutral cell on one side, and a female genoblast in the same manner on the other side, of the post-abdomen at an early larval stage, and that then, as the animal became gradually more developed, the second pair of antennæ (not hitherto sexually distinguishable) transformed themselves symmetrically in accordance with the bilateral position of the genital glands and their exits. Unfortunately we are ignorant of the causes and conditions which impel an animal, when capable of producing genoblasts, to become either male, female or hermaphrodite.

[14] Hermaphroditism in Crustaceans occurs normally in Cirripeds and parasitic Isopods. Exceptions are recorded (*a*) In the European lobster, by F. Nicholls, Phil. Trans. Royal Society, vol. 36, p. 210, 1730. (*b*) In free-living Isopods of the genera *Cirolana* and *Conilera*, by Paul Meyer, Mittheilungen aus der zoologischen Station zu Neapel. Taf. V: Ueber den Hermaphroditismus bei einigen Isopoden. pp. 165 to 179. 1878. (*c*) In certain water-fleas, *Daphnia pulex*, *D. Schæfferi*, *Alona quadrangularis*, by Wilhelm Kurz, 49 Bd. Sitzungsberichte der k. Akademie der Wiss. I. Abtheilung. 1874: Ueber androgyne Missbildungen bei Cladoceren.

[15] Consult Prof. Dr. C. T. v. Siebold's "Parthenogenese der Arthropoden," 1871.

On Color-Blindness. By B. Joy Jeffries, of Boston, Mass.

[ABSTRACT.]

Dr. Jeffries first described the natural condition of the color sense, and illustrated some of its peculiarities as related to color-blindness. The complementary after-image of a color can be readily seen by gazing at the red setting sun, when, if we turn our eyes to the east, we shall see a green rising one. Looking steadily at a yellow spot on white, and turning away, we see a blue one, etc. After looking intently at a red or green light as used for instance on railroads and on vessels, one cannot help momentarily seeing the reverse color. The centre of the retina has the greatest power of form-perception; we must fix our eye steadily to see anything very distinctly. The same with color. All colors fade in intensity outward from the centre of the retina. In a central zone we can distinguish all three of the colors now considered primary, viz.: red, green and violet. In a zone outside of this our red perception fails, and in the outer portions still of the retina green fails, and we see blue or violet only. Now we have *red*, *green* and *violet* blindness, resembling, so to speak, the conditions of these zones. This must not, however, be too strictly construed. Color-blindness may be best described thus: Those who are red, or green (one involving the other), or violet blind see all objects having these colors as gray or grayish, in the proportion in which they are color-blind and the depth of the pigment. A color mixed with their faulty one will be, so to speak, *muddy*. Many thousands of examinations have been made all over the world, with the same result as his own. He has tested 17,695 males, finding 739 color-blind in greater or lesser degree, viz., about 4 per cent. With females it is very rare, which, however, their familiarity with the colors does not account for. He tested 13,893 females, finding only ten color-blind. Age, race, color, education, condition of civilization, all seem to have no effect, as tests have been now made from the north pole to the equator, and throughout Europe and America. It is congenital, largely hereditary and handed down through the females, though they escape it. It may be artificially produced by putting a person in a cataleptic or hypnotic state; also those color-blind who can be put in this state are temporarily relieved of their defect. It is incurable by any known means. It may be caused by tobacco and alcohol poisoning, by injuries affecting the

head and by disease. It is a symptom of some brain disease of constitutional origin.

It can be helped or palliated by gaslight, when the color-blind see better, or by looking through pale lemon-colored glass like gas light, or by looking through a solution of the aniline dye called fuchsine. All this does not cure, but simply changes the relations of light and shade for the color-blind, by which alone they distinguish their faulty colors. Dr. Jeffries briefly described how difficult it was formerly to detect the color-blind, and referred to his manual on color-blindness in explanation of how they escaped. Thanks to recent observers and workers in the field we now have methods so simple, as to be readily and quickly carried out by competent experts.

These facts have led the United States government to undertake its control in the army, navy, and marine hospital service so far as this latter affects pilots. Physical examination of seamen is not yet compulsory. Its great value to the sailor was particularly explained. Standard tests and standard powers of sight and color perceptions are not yet determined by the United States. An international commission to determine these has been proposed in a bill now before Congress. He explained the future value of such a commission, and urged his audience to assist in having this carried out.

As to the railroads of the country, Dr. Jeffries quoted from his book, now an United States manual: "The difficulties are very great. Here the interests and the safety of the community have to contend with ignorance, prejudice, pecuniary considerations and incredulity born of supposed immunity from danger." This has proved most true, and even at this date, three years and a half since he called public attention to the dangers from color-blindness, but one state, Connecticut, has passed laws controlling color-blindness and visual defects among railroad employés. And yet in that state most violent attempts have been made to prevent the action of the law in protecting the community, even politics being introduced.

The practical tests, approved of over the world and recommended at the international medical congress at Amsterdam, September, 1879, and directed by the Connecticut board of health in charge of the control, were then thoroughly shown and explained. Practical illustration was also given the audience by a color-blin

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gentleman who kindly consented to exhibit his infirmity in the cause of science. These tests were Holmgren's with the colored worsteds (used by Dr. Jeffries in our public schools and the Association), Donders and Daae's modification of this method, Stilling's pseudochromatic cards, Woinow's disk, Pflüger's letters, and finally Donder's method with reflected light and transmitted light, and Holmgren's with colored shadows,—these last two being for the purpose of determining the quantitative color-sense, so necessary in deciding the fate of a railroad employé or pilot. The simple, practical use of these methods in the hands of competent experts was shown and proved in the testing thereby the color-blind present. Dr. Jeffries explained in detail how theoretical was the attempt to decide the color-sense by lanterns and flags used on land or sea, and how readily mistakes would thus be made. The worsted test can, by competent experts, be quickly made in the paymaster's car, for instance, whereby no man is taken from his duty. The color-blind can thus be sorted out and subsequently reëxamined with these additional tests as a means of control, and to exhibit their precise defect to those in authority. During and after the reading of the paper, color-blind gentlemen present were exhibited, to their astonishment and the bystanders. All present were asked, if convinced by what was shown of the danger, to exert their influence in the cause of control of color-blindness. Dr. Jeffries stated in conclusion that he had hoped to be able to touch upon the most interesting point of the development of the color-sense and the education of our color-perception. He was, however, only able to refer briefly to the work going on of the study of color development, and particularly the contributions to it by his friend Dr. Hugo Magnus of Breslau. Dr. Jeffries' work in testing all the Boston school children showed, as has the same elsewhere, the great ignorance on the part of males of color names, all aside from color-blindness. He exhibited Dr. Magnus's color-chart for teaching children colors and their names, which received a diploma of honor from the International Medical Society at Amsterdam. The use of this he explained, and said he was engaged with Mr. Prang in its introduction into this country among our schools, both private and public. The great purpose of this special work being the teaching the child to associate with the word the color and the color sense, as well as the mere color names. If generations of teaching females colors has eliminated color-blindness from that sex, then may it not from the male?

The First Decade of the United States Fish Commission: its plan of work and accomplished results, scientific and economical. By G. Brown Goode, of Washington, D. C.

There are nine departments of the government devoted, in part or wholly, to researches in pure and applied science — the Geological Survey; the Coast and Geodetic Survey; the Naval Observatory; the National Museum; the Department of Agriculture; the Entomological commission; the Tenth Census, with its special agencies for the study of the natural resources of the country; the Smithsonian Bureau of Ethnology, and the Commission of Fish and Fisheries. The Smithsonian Institution, established upon an independent foundation, should also be mentioned, as well as the Medical Museum of the army, and the various laboratories under the control of the Army and Navy Departments.

The Geological Survey is not now carrying on any of the schemes of zoölogical and botanical investigation engaged in by its predecessors.

The work of the Entomological Commission and that of the census, though of extreme importance, are limited in scope and duration, while that of the Agricultural Department is necessarily, for the most part, economical.

The work of the National Museum is chiefly confined to the study of collections made by government surveys or individual collectors and sent in to be reported upon.

The work of the Fish Commission, in one of its aspects, may perhaps be regarded as the most prominent of the present efforts of the government in aid of aggressive biological research.

On the 9th of February, 1874, Congress passed a joint resolution which authorized the appointment of a Commissioner of Fish and Fisheries. The duties of the Commissioner were thus defined: "To prosecute investigations on the subject (of the diminution of valuable fishes) with the view of ascertaining whether any and what diminution in the number of the food-fishes of the coast and the lakes of the United States has taken place; and, if so, to what causes the same is due; and also whether any and what protective, prohibitory or precautionary measures should be adopted in the premises, and to report upon the same to Congress."

The resolution establishing the office of Commissioner of Fisher-

ies required that the person to be appointed should be a civil officer of the Government, of proved scientific and practical acquaintance with the fishes of the coast, to serve without additional salary. The choice was thus practically limited to a single man for whom, in fact, the office had been created. Prof. Baird, at that time Assistant Secretary of the Smithsonian Institution, was appointed and entered at once upon his duties.

The summer of 1880 marks the tenth season of active work since its inception in 1871. The Fish Commission now fills a place tenfold more extensive and useful than at first. The present essay aims to show, in a general way, what it has done, is doing, and expects to do—its purposes, its methods, its results.

The work is naturally divided into three sections:—

1. The systematic investigation of the waters of the United States and the biological and physical problems which they present. The scientific studies of the Commission are based upon a liberal and philosophical interpretation of the law. In making his original plans the Commissioner insisted that to study only the food-fishes would be of little importance, and that useful conclusions must needs rest upon a broad foundation of investigations purely scientific in character. The life history of species of economic value should be understood from beginning to end, but no less requisite is it to know the histories of the animals and plants upon which they feed or upon which their food is nourished; the histories of their enemies and friends, and the friends and foes of their enemies and friends as well as the currents, temperatures and other physical phenomena of the waters in relation to migration, reproduction and growth. A necessary accompaniment to this division is the amassing of material for research to be stored in the National and other museums for future use.

2. The investigation of the methods of fisheries, past and present, and the statistics of production and commerce of fishery products. Man being one of the chief destroyers of fish, his influence upon their abundance must be studied. Fishery methods and apparatus must be examined and compared with those of other lands, that the use of those which threaten the destruction of useful fishes may be discouraged, and that those which are inefficient may be replaced by others more serviceable. Statistics of industry and trade must be secured for the use of Congress in

making treaties or imposing tariffs, to show to producers the best markets, and to consumers where and with what their needs may be supplied.

3. The introduction and multiplication of useful food fishes throughout the country, especially in waters under the jurisdiction of the general government, or those common to several states, none of which might feel willing to make expenditures for the benefit of the others. This work, which was not contemplated when the Commission was established, was first undertaken at the instance of the American Fish Cultural Association, whose representatives induced Congress to make a special appropriation for the purpose. This appropriation has since been renewed every year on a more bountiful scale, and the propagation of fish is at present by far the most extensive branch of the work of the Commission, both in respect to number of men employed and quantity of money expended.

Although activity in this direction may be regarded in the light of applied rather than pure scientific work, it is particularly important to the biologist, since it affords opportunities for investigating many new problems in physiology and embryology.

The origin of the Commission, its purposes, and methods of organization, having been described, it now remains to review the accomplished results of its work. In many departments, especially that of direct research, most efficient services have been rendered by volunteers; in fact, a large share of what has been accomplished in biological and physical exploration is the result of unpaid labor on the part of some of the most skilful American specialists. Although it would be interesting to review the peculiar features of the work of each investigator, the limits of this paper will not allow me even to mention them all by name.

Since the important fisheries centre in New England, the coast of this district has been the seat of the most active operations in marine research. For ten years, the Commissioner, with a party of specialists, has devoted the summer season to work at the shore, at various stations along the coast, from Connecticut to Nova Scotia.

A suitable place having been selected, a temporary laboratory is fitted up with the necessary appliances for collection and study. In this are placed from ten to twenty tables, each occupied by an investigator, either an officer of the Commission, or a volunteer.

From 1878 to 1879, important aid was rendered by the Secretary of the Navy, who detailed for this service a steamer to be used in dredging and trawling, and this year the steamer built expressly for the Commission is employed in the same manner.[1]

The regular routine of operations at a summer station includes all the various forms of activity known to naturalists—collecting along the shore, seining upon the beaches, setting traps for animals not otherwise to be obtained, and scraping with dredge and trawl the bottom of the sea, at depths as great as can be reached by a steamer in a trip of three days. In the laboratory are carried on the usual structural and systematic studies; the preparation of museum specimens and of reports. Since the organization of the Commission, the deep sea work and the investigation of invertebrate animals has been under the charge of Prof. Verrill who had, for many years before the Commission was established, been studying independently the invertebrate fauna of New England.

In addition to what has been done at the summer station, more or less exhaustive investigations have been carried on by smaller parties on many parts of the coast and interior waters. The fauna of the Grand Banks, and other off-shore fishing grounds, has been partly explored. In 1872, 1873 and 1874 dredging was carried on from the coast survey steamer Bache, by Professor Packard and Mr. Cooke, Professor Smith, Mr. Harger and Mr. Rathbun. In 1879 Mr. H. L. Osborne spent three months in a cod-schooner collecting material on the Grand Banks, and Mr. N. P. Scudder as long a time on the halibut grounds of Davis' Straits.

A most remarkable series of contributions have been received from the fishermen of Cape Ann. When the Fish Commission had its headquarters at Gloucester, in 1878, a general interest in the zoölogical work sprang up among the crews of the fishing vessels,

[1] The number of dredging and trawling stations on record is as follows:—

1871.	Wood's Holl	345
1872.	Eastport, 200 by hand, 35 by steamer	235
1873.	Portland	149
1874.	Noank	223
1875.	Wood's Holl	169
1877.	Salem	—
	Halifax	378
1878.	Gloucester	—
1879.	Provincetown	—
	Total	1,500

The number of seine hauls is about 600.

and since that time they have been vying with each other in efforts to find new animals. Their activity has been stimulated by the publication of lists of their donations in the local papers, and the number of separate lots of specimens received, to the present time, exceeds eight hundred. Many of these lots are large, consisting of collecting-tanks full of alcoholic specimens. At least thirty fishing vessels now carry collecting-tanks on every trip, and many of the fishermen, with characteristic superstition, have the idea that it insures good luck to have a tank on board, and will not go to sea without one. The number of specimens acquired in this manner is at least fifty or sixty thousand, most of them belonging to species unattainable. Each halibut vessel sets, twice daily, lines from ten to fourteen miles in length, with hooks upon them six feet apart, in water twelve hundred to eighteen hundred feet in depth, and the quantity of living forms brought up in this manner, and which had never hitherto been saved, is very astonishing. Over thirty species of fishes have thus been added to the fauna of North America, and Professor Verrill informs me that the number of new and extra limital forms thus placed upon the list of invertebrates cannot be less than fifty.

A permanent collector, Mr. Vinal N. Edwards, has been employed at Wood's Holl and vicinity since 1871, and many remarkable forms have also been discovered by him.

No dredging has yet been attempted by the Commission south of Long Island, though much has been done in shore work, especially among the fishes, by special agents and friends of the Commission, and by the parties stationed here and there in the work of fish-culture. Mr. E. G. Blackford, of Fulton Market, New York, by carefully watching the market slabs, has added at least ten species of fishes to the fauna of the United States. Mr. F. Mather is studying the fish of Long Island and the Sound. Dr. Yarrow, Mr. Earll, and others, have collected from Cape May to Key West. The Gulf States' coast was explored last winter by a party conducted by Mr. Silas Stearns, who spent nine months in studying the food and the census. The entire Pacific Coast has been scoured by Professor Jordan for the Commission and the census, and the ichthyology of that region has been enriched by the discovery of sixty species new to the fauna, forty of them being new to science. A similar investigation on the great lakes has been carried over a period of several years by Mr. Milner and Mr.

Kumlien. The ichthyology of the rivers of the country has received much attention from the many experts employed by the Commission in fishcultural work.

In addition to these local studies may be mentioned the general explorations such as are now being carried on for the oyster, by Mr. Ernest Ingersoll and Mr. John F. Ryder, for the shad by Col. Mc Donald, for the smelt and the Atlantic salmon by Mr. C. G. Atkins, and the Quinnat salmon by Mr. Livingston Stone.

A partial indication of what has been accomplished may be found in the number of species added to the various faunal lists. Take, for instance, the cephalopod mollusks of New England. In Professor Verrill's recently published monographs, twenty species are mentioned, thirteen of which are new to science. Ten years ago only three were known.

I am indebted to Professor Verrill for the following estimate of the number of species added within the past ten years to the fauna of New England, mainly by the agency of the Commission:—

	Formerly Known.	Additions.	Now Known.
Crustacea	105	193	298
Pycnogonida	5	10	15
Annelida	67	238	305
Vermes	39	100	139
Mollusca	317	109	426
Echinodermata	47	41	88
Anthozoa	20	35	55
Tunicata	26	25	51
Polyzoa	56	91	147
Brachiopoda	5	0	5
Sponges	10	80	90
Acalephæ	102	78	180
	800	1,000	1,800

It is but just to say that many of these species were obtained by Professor Verrill in the course of his independent explorations in Maine and Connecticut previous to 1871.[2]

A similar estimate for the fishes indicates the discovery of at least one hundred species on the eastern Atlantic coast within ten years; half of these are new to science. Forty species have been added to the fauna north of Cape Cod; sixteen of these are new and have been found within three years; seventeen have been described as new from the Gulf of Mexico; sixty, and more, have

[2] A few days after the reading of this paper a new fauna was discovered about one hundred miles southeast of Newport, and several hundred numbers might now be added to this enumeration.

been added upon the West coast. The results of the summers' campaigns are worked in winter by the specialists of the National Museum, and under the direction of Professor Verrill, in New Haven.

One of the important features of the work is the preparation of life histories of the useful marine animals of the country, and great quantities of material have been accumulated relating to almost every species. A portion of this has been published, more or less complete biographical monographs having been printed on the bluefish, the scup, the menhaden, the salmon and the whitefish, and others are nearly ready.

Another monograph which may be referred to in this connection is that of Mr. Starbuck on the whale fishery, giving its history from the earliest settlement of North America.

The temperature of the water, in its relation to the movements of fish, has from the first received special attention. Observations are made regularly during the summer work, and at the various hatching stations. At the instance of the Commissioner, an extensive series of observations have for several years been made under the direction of the chief signal officer of the army, at lighthouses, lightships, life-saving and signal stations, carefully chosen, along the whole coast. This year thirty or more fishing schooners and steamers are carrying thermometers to record temperatures upon the fishing grounds, a journal of the movements of the fish being kept at the same time. One practical result of the study of these observations has been the demonstration of the cause of the failure of the menhaden fisheries on the coast of Maine in 1879 — a failure on account of which nearly 2,000 persons were thrown out of employment.

Another important series of investigations carried on by Commander Beardsley, of the navy, shows the error of the ordinary manner of using the Casella Miller deep-sea thermometer; still another series made by Dr. Kidder, of the navy, and to be carried out in future, had for its object the determination of the temperature of the blood of marine animals.

Observations have also been made by Mr. Milner upon the influence of a change from sea water into fresh water and from fresh water into sea water upon the young of different fishes.

Mr. H. J. Rice carried on series of studies upon the effect of cold in retarding the development of incubating fish eggs.

A series of analyses have been made by Professor Atwater to determine the chemical composition and nutritive value of fish as compared with other articles of food. This investigation is still in progress.

In connection with the work of fish culture much attention has been paid to embryology. The breeding times and habits of nearly all of our fishes have been studied, and their relations to water temperatures. The embryological history of a number of species, such as the cod, shad, alewife, salmon, smelt, Spanish mackerel, striped bass, white perch, and the oyster, have been obtained under the auspices of the Commission, by Messrs. Brooks, Ryder, Schæffer, Rice, and others.

The introduction of new species, in water in which they were previously unknown, is of special interest to the student of geographical distribution. Through the agency of the Commission the German carp has already been placed in nearly every State and Territory, although the work of distribution has only just begun, and the tench (*Tinca vulgaris*), and the golden orfe (*Idus melanotus*), have been acclimated; the shad has been successfully planted in the Mississippi valley and on the coast of California, and the California salmon in the rivers of the Atlantic slope. The maræna, or lake whitefish, of Europe, has been introduced into a lake of Wisconsin. It is not my purpose to speak of the great success in restocking with shad and salmon several rivers in which the supply was almost exhausted, and in planting the Schoodic salmon in numerous lakes. By an act of international courtesy, California salmon have been successfully introduced into New Zealand and Germany. The propagation work has increased in importance from year to year, as may be seen by the constant increase in the amount of the annual appropriation. A review of the results of the labors of the commission, in increasing the food supply of the country, may be found in the annual reports; the rude appliances of fish culture in use ten years ago have given way to scientifically devised apparatus, by which millions of eggs are hatched where thousands were, and the demonstration of the possibility of stocking rivers and lakes to any desired extent has been greatly strengthened. This work was for six years most efficiently directed by the late Mr. James W. Milner, and is now in charge of Maj. T. B. Ferguson, also commissioner for the State of Maryland, by whom has been devised the machinery for propagation on

a gigantic scale, by the aid of steam, which is now so successfully in use, revolutionizing the art of fish culture.

The investigation of the statistics and history of the fisheries has perhaps assumed greater proportions than was at first contemplated. One of the immediate causes of the establishment of the Commission was the dissension between the line and net fishermen of southern New England with reference to laws for the protection of the deteriorating fisheries of that region. The first work of Prof. Baird, as Commissioner, was to investigate the causes of this deterioration, and the report of that year's work includes much statistical material. In the same year a zoölogical and statistical survey of the great lakes was accomplished, and various circulars were sent out in contemplation of the preparation of monographic reports upon the special branches of the fisheries, some of which have already been published.

In 1877, the Commissioner and his staff were summoned to Halifax to serve as witnesses and experts before the Halifax Fishery Commission, then charged with the settlement of the amount of compensation to be paid by the United States for the privilege of participating in the fisheries of the Provinces. The information at that time available concerning the fisheries was found to be so slight and imperfect that a plan for systematic investigation of the subject was arranged and partially undertaken. The work was carried on for two seasons with some financial aid from the Department of State. In 1879 an arrangement was made with the Superintendent of the Tenth Census, who agreed to bear a part of the expense of carrying out the scheme in full. Some thirty trained experts are now engaged in the preparation of a statistical report on the present state and the past history of the fisheries of the United States. This will be finished next year, but the subject will hereafter be continued in monographs upon separate branches of the fisheries, such as the halibut fishery, the mackerel fishery, the shad fishery, the cod fishery, the herring fishery, the smelt fishery and various others of less importance.

Hundreds, and even thousands, of specimens of a single species are often obtained. After those for the National Museum have been selected, a great number of duplicates remain. These are identified, labelled and made into sets for exchange with other

museums and for distribution to schools and small museums. This is in accordance with the time-honored usage of the Smithsonian Institution, and is regarded as an important branch of the work. Several specialists are employed solely in making up these sets and in gathering material required for their completion. Within three years fifty sets of fishes in alcohol, including at least ten thousand specimens, have been sent out, and fifty sets of invertebrates, embracing one hundred and seventy-five species and two hundred and fifty thousand specimens. One hundred smaller sets of representative forms intended for educational purposes, to be given to schools and academies, are now being prepared.

The arrangement of the invertebrate duplicates is in the charge of Mr. Richard Rathbun; of the fishes, in that of Dr. T. H. Bean.

Facilities have also been given to many institutions for making collections on their own behalf.

Six annual reports have been published, with an aggregate of 5,650 pages. These cover the period from 1871 to 1878. Many papers relating to the work have been published elsewhere—particularly descriptions of new species and results of special faunal exploration.

AN EPITOME OF THE HISTORY OF THE COMMISSION.

1871.

The Commissioner, with a party of zoölogists, established the first summer station at Wood's Holl, Mass., other assistants being engaged in a similar work at Cape Hatteras and the Great Lakes. He also personally investigated the alleged decrease of the fisheries in southern New England, taking the testimony of numerous witnesses.

1872.

This year the summer station was at Eastport, Maine, particular attention being paid to the herring fisheries. The survey of the Great Lakes was continued. Dredging, under the direction of Professor Packard, was begun on the off-shore banks. At the instance of the American Fish Cultural Association, Congress requested the Commissioner to take charge of the work of multiplying valuable food fishes throughout the country. Work was begun

on the shad, salmon and whitefish, and the eggs of the European salmon were imported.

1873.

The summer headquarters were fixed at Portland, Maine. The opportunities for research were greatly increased by the aid of the Secretary of the Navy, who granted the use of an eighty-ton steamer.

Explorations in the outer waters between Mt. Desert and Cape Cod were carried on in the United States Coast Survey steamer Bache. Operations in fish culture were carried on upon an extensive scale.

1874—1875.

In 1874 the zoölogical work centred at Noank, Conn. The attempt was made to introduce shad into Europe. In 1875 the station was for a second time at Wood's Holl, where a permanent seaside laboratory, with aquaria, was now established. The number of investigations this year was about twenty. The increase in the propagation work was proportionately much larger.

1876.

This year the Commissioner was unable to take the fishes and useful invertebrates in behalf of the Commission field for fishery investigations, having been instructed to exhibit, in connection with the Philadelphia International Exhibition, the methods of fish culture and the American fisheries. Much, however, was accomplished by single investigators in various localities. The propagation work continued. This year the first carp were introduced from Germany.

1877.

The field of investigation was resumed at Salem, Mass., and later at Halifax, N. S. A larger steamer of 300 tons made deep sea research possible. The Commissioner and his staff served as experts before the Halifax Fishery Commission. The propagating work was on the increase, and the government carp ponds were established in Washington.

1878—1879.

In 1878 the summer station was at Gloucester, Mass.; in 1879 at Provincetown. These centres of the fishing interests were selected that more attention might be devoted to studying the history, statistics and methods of the sea fisheries; a plan for the systematic investigation which seemed yearly more necessary in view of the dissensions between the governments of the United States and Great Britain. In 1879 a combination was formed with the Superintendent of the Tenth Census, by which the Commissioner was enabled to carry more rapidly forward this branch of the work. Specialists were dispatched to all parts of the country to study the biological, statistical and practical aspects of the fisheries. In 1878 the breeding of cod and haddock was accomplished at Gloucester. In 1879 the propagation of the oyster was accomplished, by coöperation with the Maryland Commission under the direction of Major Ferguson, and the distribution of the carp throughout the country was begun.

1880.

The summer station is at Newport, R. I. The Fish Hawk, a steamer of 484 tons, constructed expressly for the work of the commission, lies at the wharf, now equipped for scientific research, later to be employed in the propagation of sea fish such as the cod and the mackerel. Over fifty investigators are in the field in the service of the commission. The season was opened by the participation of the commission in the International Exhibition at Berlin. The first honor prize, the gift of the Emperor of Germany, was awarded to Professor Baird, not alone as an acknowledgment that the display of the United States was the most perfect and most imposing, but as a personal tribute to one who, in the words of the President of the Deutscher Fischerei Verein, is regarded in Europe as the first fishculturist in the world.

The Spanish Mackerel, and its Artificial Propagation. By Charles W. Smiley, of Washington, D. C.

1. NAMES AND CHARACTERISTICS.

The fish to be noticed in this paper is a member of the family *Scombridæ*, described by Mitchill as the *Cybium maculatum*. Its general appearance is much like that of the common mackerel, *Scomber scombrus*, though it is considerably larger. Its color is greenish above, blending into ashen gray. Its sides and all beneath are lustrous white, while about twenty yellowish round spots, a quarter of an inch in diameter, are irregularly distributed along the sides. Its eyes are large: its mouth is also large and armed with acute and slightly conical teeth, which, however, are wanting in front. Its length is seventeen to twenty inches. De Kay, while describing it as *Cybium maculatum*, calls it the "spotted cybium," and applies the name Spanish mackerel to the *Scomber colias*. It is true that the *Scomber colias* formerly monopolized the term Spanish mackerel, for it was then in market and sold in considerable quantities. Great schools of these fish appeared in New York Bay in 1811 and 1813. About 1820, they were exceedingly abundant at Gloucester, and Provincetown, Mass. But it has disappeared from our coast. During a ten years' search no specimen has been taken unless a rare specimen taken by Hon. Samuel Powel, of Newport, R. I., October 22, 1875, was of this species. Mr. Powel was very certain that it was *Scomber colias*. However no specimen is known to exist in any Museum. But the vernacular still has a use for the name,—Spanish mackerel, having transferred it to the *Cybium maculatum*. The latter was first described by Mitchill more than sixty years ago. It was then very scarce in our waters, though believed to be abundant in the Gulf of Mexico and Caribbean sea. Of late years it has appeared in larger and larger numbers along our coast, extending as far north as Narragansett Bay.

2. LOCALITY AND ABUNDANCE.

In 1854, Professor Theodore N. Gill, of the Smithsonian Institution, reported it to be of very slight commercial importance. Seth

Green places its first appearance within fifteen years. Few, if any, were taken in Chesapeake Bay prior to 1870. It is now found in schools off Fire Island, the shoals annually increasing in size, one having been reported five miles square. About 1872 or 1873 there was quite an abundance of this fish in Narragansett Bay. Between three hundred and four hundred were taken at one haul of the seine, but the run was short, and the fish did not reappear. The Chesapeake Bay has been annually visited by large schools for several years where it is known as the "bay mackerel." None were known to have been marketed there prior to 1870, but in 1879, 1,300,000 of this fish were sold, and the season of 1880 is expected to yield 2,000,000. They are taken in pound-nets and gill-nets. At Cherrystone, Maryland, there are fourteen pounds, which average a catch of five hundred to a day. As many as 4,000 per day have been taken in a single pound on the eastern shore of the Chesapeake, while 2,500 is not a rare catch with one pound. The bay fish, however, are smaller and leaner than those caught farther north. The fishermen disagree in regard to the length of time in which schools have visited the bay, some asserting that there were only scattering fish prior to 1875, and others that schools have appeared for many years; but all agree that there has been a surprising increase during the last five years. The occurrence of the fish hardly ought to be judged by the amount caught, for the increase in catch has been entirely coincident with the increase of pound and net-fishing. It is well known that the fish ordinarily refuses the hook.

3. METHOD OF CAPTURE, AND SALE.

The first pound, which was really a success in the Chesapeake, was constructed by George Snediker at New Point, Va., in 1875. Now there are one hundred and sixty-four between cape Charles and the Rappahannock river. Two years later, gill-nets were introduced and came immediately into general favor. This season there are one hundred and seventy-five men, between Smith's Island and Occohamock creek, engaged in taking Spanish mackerel. A net, seventy-five to one hundred fathoms long, will average twenty to forty fish per day, and as many as five hundred have been taken in that length of time. The fish average from one and one-half to two pounds each in weight. The markets in the

order of their importance, are Baltimore, New York and Philadelphia. In the New York market the receipts, since 1878, have been as follows:

MONTH.	1878.	1879.	1880.
APRIL	.....	98 lbs.	19 lbs.
MAY	6,980 lbs.	5,974	13,938
JUNE	56,642	38,803	62,964
JULY	112,652	114,309	.....
AUGUST	72,786	98,277	.....
SEPTEMBER	21,504	46,596	.....
OCTOBER	3,699	4,451	.....
NOVEMBER	650	657	.....
DECEMBER	.....	3	.....
	274,913 lbs.	309,168 lbs.	76,941 lbs.

4. MARKET PRICES.

In New York market the price per pound ranged from eighteen to thirty cents during 1879; for May, 1880, from fifteen to forty cents, but owing to the large shipments in June the price fell to ten to fifteen cents. On special occasions the fish have sold readily at one dollar per pound. The catch of 1873 at Newport, R. I., was sold at prices varying from seventy-five cents to one dollar per pound.

5. THE MIGRATIONS.

This fish usually appears in the Chesapeake in May, when the temperature has reached 65 or 70 degrees, and the number increases until the middle of June. They remain abundant until September and diminish as the temperature of the water falls, until, in early October, nearly all have disappeared. They come in small schools, but later get scattered, and often quite isolated. Before leaving, the schools seem to be somewhat reformed. The individuals of the schools are of different sizes, the males and

females often disproportionately mingled. They seem to avoid the brackish waters and do not enter the rivers at all. They are less plentiful in the upper Chesapeake.

6. SPAWNING.

Six months ago no one knew the first particular of their spawning. The size and character of the eggs were unknown. No limit had ever been fixed for the spawning period. The shrewdest guess was that they spawned in the warm water of the south in midwinter. Their rare quality as a food-fish and the success in artificially hatching shad, salmon, cod, and other edible fish, had led the United States fish commission to desire much the lacking information.

In the winter of 1879 and 1880, by direction of Professor S. F. Baird, commissioner, Mr. R. E. Earll, for several years connected with the Propagation Department of the Fish Commission, and Col. Marshall McDonald, commissioner of the state of Virginia, made an exploring tour along the southern coast from Florida northward. To Mr. Earll were assigned the salt water fishes, and to Col. McDonald those of the fresh water. In late spring, they had passed Norfolk and entered upon the examination of the Chesapeake bay. They had been constantly on the alert for spawning mackerel and were at last rewarded about June 1st, when ripe fish were discovered by them in the lower part of the bay, and the announcement was made to the commissioner by Col. McDonald. This led to a further investigation, till upon reaching Mobjack bay, enormous quantities were found actually depositing their eggs. Mr. Earll who is a member of this Association, but unable to attend this meeting, and who has furnished all the facts concerning his work for this paper, at once telegraphed the extent of the discovery to Professor Baird, in Washington, and reported in person some days later.

7. EXPERIMENTS.

Professor Baird, who seemed more gratified with this discovery than with the world's prize just announced by cable as awarded to him personally at the International Fishery Exhibition at Berlin,

directed Mr. Earll on June 17th to return to the bay; and, by a series of experiments, to ascertain whether the fish could be artificially hatched. Professor Baird offered him the use of a steamer for the purpose, by means of which all the different kinds of hatching apparatus could be tried in the experiment. But after advising with Col. McDonald, Mr. Earll concluded to make the first attempt with such apparatus as could be prepared on the ground. June 21st, Mr. Earll started for Crisfield, Md., on the eastern shore of the Chesapeake, and during the following ten days conducted his experiments there, obtaining his supply of eggs from the pound-nets of Mr. J. E. N. Sterling, who rendered every possible assistance. Mr. Earll found the number of eggs produced by a single fish to be from 50,000 to 500,000, according to the size of the fish, the latter number having been taken from a fish weighing one and three-fourths pounds. Instead of all the eggs ripening at once, as is true in the case of the shad, only a part are thrown at a time and at intervals of a few days, probably extending through two to three months. This is analogous to the cod which deposits its eggs at intervals during five or six months. Different individuals of mackerel were found to vary in their time of spawning; some ripening a considerable time before others, and the males seeming to ripen somewhat in advance of the females. From 40,000 to 130,000 eggs were obtained at one time from a single fish. The shad however yields only 20,000 to 30,000 as its fruit of an entire season. The cod, on the other hand, are so prolific, that a twenty-one pound fish has yielded 2,700,000 eggs, and a seventy pound fish has yielded 9,000,000 eggs.

When the fish had remained in the nets several days, Mr. Earll found that the most of the spawning females had deposited all their ripe eggs. The greatest quantities were secured from individuals that had remained in the pound but a few hours. It is believed that when confined, the female presses against the netting in its efforts to escape, and produces an abnormal discharge of eggs; but it would result in the impregnation of a much larger number of eggs than would chance to be fertilized in a natural way. The males and females being caught side by side in considerable numbers, both eggs and milt would be present in the water in such quantities that they could not fail to come in contact before vitality is lost.

8. FERTILIZATION.

The eggs are easily fertilized. It is merely necessary that they come into contact with the milt of the male in any receptacle of water. A half hour after contact with the milt, the eggs swell and become too hard to be broken by the pressure of the thumb and finger. The water in which fertilization took place can then be replaced by clean water, and the eggs are ready for the hatching apparatus. Their specific gravity is now so nearly equal to that of salt water that when the water is at rest they float upon its surface, remain suspended in the water, or occasionally sink slowly to the bottom. The least current will cause them to be distributed through the liquid. Mr. Earll discovered a small oil globule in each egg which serves the purpose of buoying it. The impregnated egg is also so transparent that the fishermen, who are not usually very observing, would never suspect their presence. The eggs are smaller than eggs of almost any other species, and have an average diameter of only one-twenty-eighth of an inch. It has been estimated as will be seen that 21,952 would make a cubic inch, and a quart, 57¾ cubic inches, would hold 1,267,728 eggs.

The period of hatching is greatly influenced by the temperature of the water. The average temperature during the experiments at Crisfield was 84 degrees Fahrenheit. Ten hours after contact with the milt the outline of the fish could be discerned by the naked eye. The fish is formed with the curve of its back at the lowest point of the egg. In fifteen and one-half hours, the fish began to hatch. In eighteen hours, one-half of the eggs had hatched, and in twenty hours, all were out. Experiments in water at 78 degrees Fahrenheit showed that 24 hours were necessary for hatching. A more remarkable effect of temperature is observed in the case of the cod. In water at 45 degrees, cod have been hatched in thirteen days; but in water at 31 degrees, fifty days were occupied in hatching.

The newly hatched mackerel are about one-eighth of an inch in length, and so small as to escape through wire-cloth with thirty-two threads to the inch, and are almost colorless. The food-sac, situated well forward, is quite large in proportion

to the body, the anterior margin extending to the lower jaw. While floating on its back for several hours, during its helpless condition, it passes safely over the heads of its enemies, and is protected from being wrecked in sand or weeds. While in this almost motionless condition, it occasionally indulges in spasmodic efforts similar to those noticeable in freeing itself from the shell. After a few hours, becoming more vigorous, it gets to a depth of an inch or more below the surface of the water. After a day or two, the food-sac is less prominent and the fish experiences less difficulty in swimming at various depths. The young mackerel, hatched by Mr. Earll, were so hardy that forty were confined in a goblet without change of water for two days before the first fish died; others placed in water which was allowed to cool gradually, and immediately transferred to water ten degrees warmer, were not injured in the least. In fresh water they slowly sank and died in a few hours.

Mr. Earll also found that a fair percentage of eggs could be hatched in still water with but one or two changes during their development. Eggs taken at 6 P. M. and allowed to remain in a basin of water till morning, when another change was made, hatched with very small percentage of loss. Samples of all the different stages of development were preserved in alcohol and glycerine for the National Museum. Over half a million were hatched by the various methods and at various times.

9. APPARATUS.

The apparatus used in these experiments being made on the spot, though very primitive, was as successful as could possibly be desired. The apparatus consisted simply of floating-boxes with bottoms made of wire-cloth. The cloth was plated with nickel to prevent injurious action of the salt water and contained thirty-two wires to the inch. After it was found that a lot of fish had escaped through it, only the shells remaining to prove that hatching had taken place, the wire and each aperture were covered with coarse cotton cloth. The boxes were provided with covers for protection against storms, or wind or rain, but were provided with openings on the sides to admit fresh water from above. The box giving best results was arranged on a principle similar to that employed by Captain H. C. Chester in the experiments with codfish

at Gloucester, in the winter of 1878. It had openings on two sides even with the water line. Just below these on the outside of the box were wooden floats about three inches wide. These were placed at an angle with the surface so that the ordinary wave-action would cause the water to run up the slight incline, and after reaching the highest points, passed down through the wire-cloth into the box, thus giving a constant change of water without the aid of tide or currents. Other boxes were arranged in the ordinary way, to utilize the action of both tides and currents. These set obliquely in the water, so that the motion would force it through the bottom. In addition each had a large opening covered with wire-cloth below the surface of the water on its front side to give additional circulation. The boxes were moored in the harbor where the tide was strongest, and by means of them a greater part of the eggs was successfully hatched. The loss in one instance was less than fifteen per cent.

10. CONFIRMATION AND PRACTICAL RESULTS.

After the experiments of Mr. Earll were over, very gratifying confirmation of the results reached by him was had in the microscopic studies of Mr. John A. Ryder, of the Fish Commission. The fish-hatching steamer "Lookout," under the direction of Major Ferguson, going down the bay, Mr. Ryder improved the opportunity to hatch a small number of fish and to make drawings of the embryo in different stages of development. He found that when the embryo was freed from the shell, the changes occurred rapidly. In seven hours he detected the outline of a fish; in seventeen, the head and eyes, and in twenty a free fish. He discovered the oil-globule attached to the lower end of the umbilical sac, which he pronounced to exist in no other fish which has been artificially propagated. At four days from the time of hatching, the globule having been absorbed, he observed the fish swimming vigorously about with open mouth, as if in search of microscopic food, and actually discovered traces of food in the stomach. Major Ferguson, the assistant commissioner, was so much pleased with the series of discoveries by Mr. Earll, and the confirmatory observations of Mr. Ryder, that he transported some of the young to Saint-Jerome Creek to watch their subsequent growth. Professor Baird has traced the stages of the investigation with the

greatest interest and has furnished every possible facility. He will probably enter upon systematic hatching of the mackerel next summer with a view to stocking other waters. It is hoped that at least Narragansett bay may be reached as a northerly limit. The artificial propagation is entirely feasible and the season being in mid-summer would not conflict with the shad season of the spring, the salmon season of the fall, or the cod season of the winter. The eggs are much more abundant, and hatch more easily, as well as more speedily, than any other fish now propagated. During the four days consumed in hatching a lot of shad, five lots of mackerel could be hatched, and during twenty-four days necessary to hatch one lot of codfish, thirty-two lots of mackerel could be produced. Mr. Earll selected a suitable locality for a hatching station at Cherrystone, Md., on the eastern shore, preferring this to Mobjack bay at which the pounds are most numerous, on account of an excellent harbor. The fishermen are also very kindly disposed, and offered to render every assistance. Thus most important facts are added to the natural history of the fish and the prospect is bright for a great commercial success.

ANTHRAX OF FRUIT TREES; OR THE SO-CALLED FIRE BLIGHT OF PEAR, AND TWIG BLIGHT OF APPLE, TREES. By T. J. BURRILL, of Urbana, Ill.

THE widespread and disastrous disease of the pear tree, commonly called Fire Blight and that no less prevalent, at least in the western states, usually known as Twig Blight of the apple tree, are due to the same immediate agency. They are identical in origin and as similar in their pathological characteristics as are the trees themselves, or differ no more than one familiar with the histological structure of the trees might indicate, prior to observation upon the course and consequence of the malady. The quince and probably many other plants, among which we now name the butternut, the Lombardy poplar and the American aspen, suffer from the same disease. Judging from published descriptions only,

it is also very probable that the "Yellows" of the peach will be found due to a similar cause.

The immediate and exciting cause of the disease is a living organism, belonging to a group of minute beings called *bacteria* which produce butyric fermentation of the stored carbonaceous compounds in the cells of the affected plants, and especially in those of the bark outside of the liber or fibrous inner layer. This organism, if indeed specifically different, is closely allied to the "*vibrion butyrique*" of Pasteur[1] and the *Bacillus amylobacter* of Van Tieghem,[2] now known to be identical with the preceding. The proof of these assertions is the burden of this paper.

HISTORICAL.

This disease of pear and apple trees has been known in this country over one hundred years, in varying degrees of virulence and remarkably irregular occurrence, throughout the United States east of the great plains bordering the Rocky mountains, sometimes doing inconsiderable damage and again killing such disheartening numbers of trees that planters have not dared to risk the cultivation of these favorite fruits except upon a limited scale. The planting of pears has been practically abandoned throughout large areas of the country, solely on account of the ravages of the disease in question. Apple trees, though as often attacked in the western states and much injured, do not so severely suffer, seldom dying outright from this cause alone. The quince is scarcely less injured than the pear.

No topic has been more fully discussed in the meetings of the horticulturists, and none has a fuller literature in the periodicals devoted to rural affairs. Theory after theory has been advanced as to the cause and the cure, only to be overturned by subsequent observation and experience. Because an insect was sometimes found destroying a twig or branch, and an account of its operations published, many persons supposed that insects were the depredators and the sole cause of the mischief. But no competent entomologist now attributes the devastating "fire blight" to the direct work of insects. Downing[3] argued — and he had many

[1] *Comptes Rendus*, t. 52, p. 344, 1861.
[2] *Comptes Rendus*, t. 88, p. 205, 1879 and t. 89, p. 5, 1879.
[3] Fruits and Fruit Trees of America, 2d ed., p. 646.

supporters — that the difficulty originated in the freezing of the sap, and its subsequent corruption in the tissues. Others believed they had traced the origin of the disease to the absence of essential elements in the soil, while still others attributed it to organic debility of the trees. Finally, when all visible or tangible agents were given up, and all theories proved inadequately supported by facts, the idea of destruction by fungi was theoretically advanced.

In 1863, Dr. Salisbury[4] of Ohio published an illustrated account of his microscopical observations, concluding that "blight" was due to a fungus which he named *Sphœrotheca pyri*. Later investigations have by no means sustained this opinion, yet by many this paper is considered the most important one in the literature of the subject. Dr. Hull[5] of Illinois in 1868 and '70 gave accounts of observations and experiments, proving as he thought the accuracy of the fungoid theory. One of his statements is of special interest here. He had received through the mail a blighted apple twig and remarks: "We cut from it several small slices of bark, going deep enough to include a thin slice of wood; with these, we inoculated several succulent pear shoots by tying in the pieces of bark as in budding. * * * . After a lapse of thirty-four days * * * we found them all blighted." He also adds that the conditions of the weather were not the most favorable, for though warm, a drought was prevailing and he believed a humid condition of the atmosphere is required.

Many others have stated, in a less definite way, that blight is communicable from diseased to healthy trees, but there is no precise account of carefully conducted experiments upon the subject. Professor Turner[6] of Illinois speaks of its dissemination by the pruning knife, and cure by turpentine and lampblack. In 1868 Dr. Hull thought he had induced disease similar to the "fire blight" by grinding in water fibres of fungi found upon rotting roots.

In an address before the American Pomological Society at St. Louis, Thomas Meehan gave his reasons for holding theoretically that the malady is due to fungous parasitism and, through the columns of the Gardener's Monthly, he has often pointed out the

[4] Transactions Ohio Agricultural Society, 1863, p. 450.
[5] Transactions Illinois State Horticultural Society, 1868, p. 35; 1870, p. 220.
[6] Transactions Illinois State Horticultural Society, 1878, p. 81.

fallacy of other opinions and the accumulating evidence in favor of this. At his instance Dr. J. Gibbons Hunt, of Philadelphia, examined, by the aid of the best microscopical equipments, blighted limbs of pear, and having found a fungus which he supposed caused the black color of the branches, he says[7]: "It attacks the bark and outside of the leaves and young fruit first, causing changes in the cells, in these locations resembling much those pigmentary cell-changes which differentiate the negro from the so-called white man. The cell contents, normally transparent, are changed into extremely minute pigment granules which fill the cells and give that characteristic color and smell which mark the disease. Moreover, minute drops of viscid offensive liquid come out on the surface. * * * From the cambium layer the fungus travels towards the interior of the stem through the medullary rays chiefly, and here I find those round bodies which in our ignorance we often call spores. The ducts which ascend the stem are often obstructed with similar bodies and aggregated pigment granules. This is all I know about the subject. I cannot venture to name the fungus."

I have quoted this account at some length, for according to my observations it is the nearest approach to the solution of the cause of the difficulty founded upon direct investigation that had up to this time found its way into public print. Still it seems impossible to admit in full the correctness of the interpretations. What the round bodies called spores were, it is difficult to perceive. The dark color of blighted parts certainly does not come from pigment granules. The notice of the "offensive liquid" is a capital point, but this seems to have escaped careful examination.

In 1876, Thomas Taylor of the Department of Agriculture, Washington, D. C., exhibited drawings at the Centennial Exposition showing[8] "the effects of the chemical changes which take place in the interior structure of the tree under the attacks of the fungus to which this disease is due."

In 1877, the writer[9] presented to the Illinois State Horticultural Society the results of microscopical observations, in the account of which occurs the first published notice of the minute moving

[7] Gardener's Monthly, 1875, p. 245.
[8] Report Department of Agriculture, 1876, p. 75.
[9] Transactions Illinois State Horticultural Society, 1877, p. 114.

bodies always present in the portions of trees suffering from the malady of which we treat. I quote: "The cambium [a term here too loosely used] of the blighted branch, when trouble first shows itself, and for some days thereafter, is filled with very minute moving particles. * * * . Not unfrequently a thickish, brownish, sticky matter exudes from affected limbs, sometimes so abundant as to run down the surface or drop from the tree. This proves to be identical with that noticed in the cambium and unquestionably has the same origin. The sticky, half-fluid substance thus exuding is entirely made up of these minute oscillating particles." In a subsequent discussion in the same society I am credited with the following:[10] "If we remove the bark of a newly affected limb and place a little of the mucilaginous fluid from the browned tissues under our microscope, the field is seen to be alive with moving atoms, known in a general way as bacteria. * * * . A particle of this viscous fluid introduced upon the point of a knife into the bark of a healthy tree is in many cases followed by blight of the part, but with me not in every instance. * * * . If we look once more to the affected branch, we find the disease spreads more or less rapidly from the point of origin, and upon examination the moving microscopic things are discovered in advance of the discolored portions of the tissues, but not very far ahead — an inch perhaps. [They are now known to advance several feet in certain cases, ahead of the black colored parts.] Does it not seem plausible that they cause the subsequently apparent change? It does to me, but this is the extent of my own faith; we should not say the conclusion is reached and the cause of the difficulty definitely ascertained. So far as I know, the idea is an entirely new one — that bacteria cause disease in plants — though abundantly proved in the case of animals."

This is a very meagre sketch of the literature upon the subject, but nothing has been omitted which to my knowledge has special bearing upon the essential point of the present paper. After all the investigations that had been made and all that had been said — a kind of ferment of itself — editors[11] were "obliged to reply," to quote from the American Agriculturist, "as we have many times in the past that the 'cause' of the blight is not known. It is supposed to be due to fungi, but this cause has not been

[10] Transactions Illinois State Horticultural Society, 1878, p. 80.
[11] American Agriculturist, Vol. 37, p. 356, 1878.

demonstrated. * * * Though pear blight is a topic discussed at pomological meetings more than any other, scarcely any positive knowledge has been thus far elicited." This was literally true and has been repeated in substance hundreds of times until this day. Can it be hereafter written with equal truthfulness?

EXPERIMENTAL.

After further researches upon the presence of bacteria in the tissues of dying limbs and the scarred blotches upon the trunks of trees, I began, July 1, 1880, a series of experiments with a view to determine whether these organisms were really active agents in the observed changes, or simply accompanied other causes of destruction. Other interests at the same time so engaged my attention and time, that the work was not so extensively prosecuted as I heartily wished it had been, after becoming gradually convinced of the possible complete demonstration of the perplexing problem. However, the results are sufficiently clear to warrant their announcement and to establish the aggressive activity of the organisms. Experiments were continued and results carefully noted up to the 14th of August, 1880. Blight was during the time more or less prevalent in the vicinity. On the isolated acre of ground where most of the experiments were made, there are ninety-four pear trees of different ages and kinds, some in cultivated soil, others in sward. There are twenty apple trees and one quince tree. Of the pears, seventy-two are three years old from the bud on pear roots. They were transplanted in the spring of 1879, and though culls from a nursery were in good thrifty condition, making a growth this year of about three feet. These were Bartletts and Clapp's Favorite. The other pear trees were of bearing age and mostly laden with fruit. Blight occurred spontaneously, *i. e.*, without conscious introduction by myself, on four of the large pear trees and upon one only of the seventy-two young ones, upon which the experiments were tried. This one stood within two feet of one allowed to die from artificial inoculation.

The weather during the time was favorable for the operations, being with the exceptions to be stated remarkably uniform, of moderate summer temperature, clear and fine. Much rain had previously fallen, and a heavy thunder shower occurred on the last

day of June. During the few first days of July, the air was moist and the temperature stood in daytime, about 90° Fah. Other storms with thunder occurred July 9th and August 11th, with, however, but little rainfall.

The term inoculation is used to denote the introduction of the virus by a wound in the bark. External applications are not included under this designation.

The inoculations of July 1st and 10th were made by cutting pieces of diseased bark freshly taken from the tree, and inserting them after the manner of budding, as practised by nurserymen, without, however, taking the precaution of tying. These pieces of bark were about three-sixteenths of an inch by two-thirds of an inch. Those made after the date mentioned were performed with a sharp-pointed knife, or needle dipped in the exuding virus of diseased trees. This was usually collected in the morning and placed in a vial with a little distilled water. Requisite care was always taken to cleanse the instrument thoroughly or to choose a new one when changing the infecting material. Usually, shoots of the current year's growth were chosen for inoculation and the operation performed near the middle, longitudinally. But in the apple after the first few trials the wound was generally made near the apex. Experiments, thirty-three to forty-two inclusive, were made in the growth of last year of thrifty apple trees in nursery the third season, using virus from pear in part, and in part from apple. None of these were successful. Twenty-one experiments were made by simply applying the virus by means of a brush to the uninjured epidermis of growing shoots or leaves. No one of these was apparently successful up to the 14th of August.

RESULTS OF EXPERIMENTS.

Of the inoculated pear trees, *sixty-three per cent. became diseased*, exhibiting all the characteristics externally and internally of "fire blight." Eighteen of these inoculated trees stood in a row of fifty-five, two feet apart, and were selected here and there along the row. Of the thirty-seven not inoculated by myself in the row, all save one remained free from the malady up to the end of the experiments. Seventeen others of the same age and kind, growing within a few feet or rods, also continued in health. Thus while sixty-three per cent. of the inoculated died, less than

two per cent. of those with the former, under the same conditions but not consciously inoculated, became infected with the disease. Of the pear trees inoculated with virus from diseased pear, fifty-four per cent. received the disease, while of those inoculated from blighting apple, *seventy-two* per cent. became as thoroughly infected, and the parts as speedily died. This greater per cent. of infection from the apple virus does not necessarily prove that it is more destructive, for another set of experiments may reverse the percentages; but I think the result may be taken as showing the identity of the disease in the two trees. All other experiments agree in this respect.

The four inoculations of the quince with pear virus were all successful. There is but one quince tree or rather bush upon the experimental grounds. Its low head furnishes many branches—thirty to forty—similar to those chosen for experiment. Blight occurred only in the four inoculated. These having been removed, the bush is (Aug. 14) apparently perfectly free from the disease.

Turning to the apples experimented upon, we find the results less positive. The ten nursery trees previously mentioned, a half mile from the principal experimental plat, inoculated in year old parts, five with pear, five with apple virus, gave no affirmative results. No special reason can be assigned for the failure, unless it may be the age of the parts inoculated. This needs repetition. The other trees were of the variety known as Grimes' Golden Pippin, eight years from graft. This variety is not peculiarly subject to blight, but several affected branches not intentionally inoculated were trimmed away from time to time. From ten inoculations with a needle in the tender bark of young shoots, six, or sixty per cent., gave evidence of infection; but, except in two cases, the subsequent development of the disease was feeble compared with the experimental pears. Application of virus with a brush was made upon the bark of the current season's growth of four pear trees and six apple trees. In some cases these were bound with a moistened cloth to prevent too rapid desiccation of the virus. All proved unsuccessful. In a similar manner virus was applied after six o'clock, P. M., to a large number of leaves upon five pear trees and two apple trees. The most perfect and apparently healthy leaves were selected, and an account kept as to the application to the upper or under surface. No certain infection followed, though microscopical and other examinations

show that leaves upon healthy shoots may perish with the disease. Had some of these leaves shown infection, it would not have proved that this came from external application, for the epidermis might have been freshly ruptured by insects or otherwise and *inoculation* secured. But the experiments do tend to show that the virus is harmless on the outside of the epidermal surfaces. Neither does it appear to gain access to the inner tissues through the stomata.

In experiments nine and ten, large limbs with the disease in active progress were cut and bound in among the branches of healthy trees. No injury ensued. This was upon the first day of July when the earth was wet and the air moist from the heavy rain of the day before. Another storm occurred eight days afterward, the dead branches remaining on the trees. Here again infection might have followed from inoculation by insects, etc.; but the failure further supports the idea of the non-communicability from without.

Of the total number of experiments of which records were kept including application in every way, thirty-four and seventy-eight hundredths per cent. were effective, while of the whole number of inoculations fifty-two and seventeen hundredths per cent. unmistakably communicated the disease.

Thrown into tabular form the per cents. in the foregoing are as follows:

Number of Experiments	Kinds of Trees.	Virus from Pear.	Virus from Apple.	Successful Inoculations.	Successful Applications all kinds.
36	Pear	54.00	72.00	63.00	38.80
29	Apple	30.00	None	30.00	20.69
4	Quince	100.	None	100.	100.
69	Total				34.78

The close daily observations required for the work, as now described, convinced me that two popular opinions concerning the progress of this disease are not founded in fact. It is usually supposed that the destruction of affected parts of trees is very sudden. In a recent paper by an accomplished scientist [12] I find

[12] Professor W. S. Barnard, First Report Cornell Experiment Station, p. 28.

these words: "In all *pear-blights* there appears a *fungus*, but its development can only be occasioned by some injury, as from excessive freezing or drying, and from the ravages of animals. When due to climatic influences usually a large part or all the tree is affected at once." Now as the pear blight of which we write is so often attributed to "climatic influences," this expression gives voice to a very commonly entertained supposition. I find the march of the destroyer very irregular in different parts of the same tree at the same time, or in the same part at different times, but always *slow*. In experiment seven, visible indications of the disease spreading both ways from place of inoculation were observed after seventeen days through the distance of eighteen inches, but this is altogether exceptional. Infection rarely shows at all under nine days and after this time an inch per day is more than the average rate. But if guided solely by the change of color in the leaves, other conclusions would often be reached. Ordinarily, according to my observations the leaves do not become black until about two weeks after the infection of the bark upon the shoot which bears them, except in the case of very young portions near the extremity. On the pear shoot just mentioned every leaf was still green and fresh in appearance at the close of the seventeenth day. The bark of older trees has been found unquestionably affected throughout a length of four feet while all the leaves were still green. In some cases this may continue during the whole summer and recovery may sometimes occur.

On old limbs and trunks covered with rough bark, the progress of the disease cannot be observed from external signs; but on the growth of the current season at least, this is not the case. The color becomes darker, a watery appearance is presented and, especially in the early morning, little beads or drops of white gummy matter may be seen. Wet with rain or dew, this exudation spreads over the surface or runs down the bark, dripping from projecting points.

Again, there has been a prevailing opinion that thunder storms in some way accelerate the disease. It seems to me some explanation can now be given for this common but fallacious notion.

The green leaves of the shoot or tree become, from the checking of the transpiration, gorged with water and the fermentation already in progress in them quickly destroys their vitality. However this may be, I have not been able to detect any certain varia-

tion in the advance of the disease in the tree, corresponding with changes in the meteorological conditions. Doubtless there are some such variations, but not such as are appreciable on account of passing storms. Even winter weather does not offer complete immunity to the trees. During two winters I have watched the slow progress of the disease in the branches of the same tree and am positive there was no mistake as to the nature of the phenomena. In the first case the gradual destruction was continued during the spring; in the second, progress immediately ceased upon the expansion of the buds.

In many parts of the country, patches of bark upon the trunks of apple and pear trees, often near the ground, are found diseased or dead. Far too often these extend around the tree and put an end to its existence. This is ordinarily supposed to occur in winter. Is this another form of the blight of which we treat?

MICROSCOPICAL.

The organism to which we attribute the death of our fruit trees is so minute that a magnifying power of two hundred diameters is necessary to make out its outline at all, and one thousand diameters are required for careful study. During the course of its development, it assumes various shapes, the different forms usually appearing together in the field of the microscope. What seems to be the most characteristic form consists of two oblong joints with rounded ends, which have a transverse diameter of about $\frac{1}{1000}$ of a millimetre. The two articles usually seen together have a length of about $\frac{3}{1000}$ of a millimetre. They are comparatively thicker than *Bacterium termo* and their motions are less rapid. They slide forward with a slightly undulating motion, they turn over and on end, but never glide across the field. It is impossible specifically to identify these creatures by form alone. So far as we now know, this may be the same as a common omnivorous little agent[13] which converts sugar, amylaceous matter, lactic, tartaric, citric, malic and music acids and albuminous substances into carbon dioxide, butyric acid and hydrogen, whenever and wherever the conditions permit. Should this prove to be the case, it would not necessarily, if presumably, invalidate its agency in pro-

[13] Schutzenberger, on Fermentation, p. 209.

ducing this disease of the pear: but it might render less hopeful the discovery of remedial treatment.

The most conspicuous change that can be observed with the aid of the microscope, in the tissues affected with blight, is the disappearance of the stored starch. The cell walls are not dissolved, or in any way altered except the staining by oxygenized material in later stages of the disease. The protoplasm of young cells remains, until death takes place. In older cells the thin protoplasmic lining of the walls can be made out after the starch has all disappeared.

Unless the Bacterium does dissolve for itself a passage way through the cell wall, it cannot possibly gain entrance in the adult state: yet they are found swarming in cells absolutely closed so far as a power of one thousand diameters can reveal. In studying this I have used a Spencer's one-tenth of recent construction and have been unable to detect the least corroding of the cellulose wall. It is scarcely necessary to add that the young tissues affected by the blight have in their cell walls no open pores, such as occur in the cells of older wood; neither are there ducts or other channels for their passage.

Accepting Nägeli's[14] theory of the molecular construction of the cell wall, we can only understand how these organisms pass from cell to cell in their deadly work by supposing their germs are less than the molecular openings, hence ultra-microscopical, and that in this condition they pass in water the cellulose barrier, developing into visibility within; or, that in the germ condition, they are of such plastic consistency that, amœba-like, they are able to creep through the narrow spaces between the molecules. It is well known that the spores of many parasitic fungi, *e. g.*, *Peronospora infestans*, causing the potato rot, send through the epidermis of the leaf an exceedingly fine tube through which the contents of the spore pass and accumulate in the swelling end of the penetrating tube. From the latter the growing mycelium itself makes its way through the walls of the inner tissues. The minute opening in the epidermis becomes closed, and all indications of the entrance are obliterated. If anything of this kind occurs in the penetrating of our Bacterium it is upon too small a scale to be made out.

[14] Sachs' Text Book of Botany. Eng. trans., p. 388.

After a cell has been invaded by the moving particles, the first thing noticeable is the agitation of the starch granules. They swing to and fro like the Brownian movement in small particles. By passing under the microscope a thin fresh section from the healthy to the diseased parts, one can witness the gradual diminution of the granules of starch until they disappear entirely from the cells.

There is absolutely no trace of other fungous growth in the tissues examined by me until after death has taken place; neither have any been found for several days after this in sections of twigs or diseased parts of trees left in the meantime out of doors. In culture baths, ordinary molds soon appear on the affected parts. If the death of the trees were due to the larger fungi like that of the potato rot and wheat rust, surely some one would have found it. None are too small for the modern microscope and its masters, nor too inscrutable to be past finding out!

In very young tissues such as the tips of apple tree shoots, all parts except the epidermis seem to be equally affected; but in older limbs the chlorophyll-bearing parenchyma of the bark is the first and usually the chief seat of the disease. The bast is not affected and not unfrequently a concentric layer of bast cells divides the healthy from the diseased parts. Sometimes invasion through such a close layer is gained by way of the medullary rays. Contrary to the usual opinion, the cambium is by no means the seat of the disease. This layer of thin walled cells often retains its vitality when all outside of it perishes, in which case a fresh layer of new bark is sometimes formed and the limb or tree survives. When the disease proceeds upward, as it often does, the xylem or wood inside the cambium is usually stained by the ascending water colored in its passage through the dying and brown parts. The staining is especially noticeable in the fibro-vascular bundles which turn out into the leaves, just where the water rises to supply the transpiration from the leaf surfaces. This stained wood may be still healthy in every particular; there is no evidence of the progression of the disease in the wood itself.

The leaves however are invaded from the bark and doubtless may be on the other hand the starting point of the infection. I have no direct experiments upon this point, but have found infected and dying leaves on healthy stems. During dewy nights

infected leaves become smeared with the exuding virus which upon drying looks like varnish. This dried material retains its vitality for some time, but how long is not known. When moistened with water, the bacteria move as usual and otherwise exhibit every characteristic of life.

CHEMICAL.

My colleague, Professor H. A. Weber, kindly undertook to determine for me the kind of fermentation which takes place in the diseased bark. He found, first, that carbon dioxide is abundantly given off, and that in this respect the affected bark differs wholly from that in the healthy condition. Secondly, butyric acid was determined as a considerable product of the fermentation. This turned my attention to the published accounts of the butyric fermentation of organic products. Among the earliest and best known are those of Pasteur[15] of France, upon sugar and calcium lactate. He traced the changes observed to the action of a minute organism, which received the name of *vibrion butyrique*.

In 1850 Mitscherlich[16] of Germany communicated to the Academy at Berlin his studies upon the fermentation of cellulose and expressed the opinion that it was due to a particular living atom which he found in the fermenting material. Fifteen years afterward, Trecul of France found in macerated vegetable substances a living organism which he believed arose spontaneously by a direct transformation of the protoplasm of the closed cells. To this he gave the name of *Amylobacter*, after assuring himself that in a certain stage of its existence it stored starch within itself. In 1877, Van Tieghem[17] of France transferred this living thing to the genus Bacillus, naming it *Bacillus amylobacter*. From observations upon it he satisfied himself that it could and did penetrate the cell wall in the adult condition, thus overthrowing as he thought the theory of spontaneous origin. Here the battle wages. Trecul[18] still holds his first opinion and criticises Van Tieghem's proofs.

In June, 1879, Prazmowski[19] of Leipzig announced that this Bacillus amylobacter produced, in the fermentation of carbonaceous substances, butyric acid, and forthwith concluded that it was iden-

[15] Comptes Rendus, t. 52, p. 344 (1861) and Schützenberger, on Fermentation, p. 213.
[16] Comptes Rendus, t. 88, p. 205, t. 61, pp. 156 and 436, t. 65, p. 513.
[17] Bulletin de la Societé botanique, March 23, 1877. Comptes Rend., t. 88, p. 205.
[18] Comptes Rendus, t. 88, p. 401, 1879.
[19] Comptes Rendus, t. 89, p. 5, 1879.

tical with the Vibrio described by Pasteur. Van Tieghem confirms this, and it now appears probable that the butyric fermentation of at least all dead carbonaceous compounds, soluble or insoluble, is directly or indirectly produced by the same wonder-working little creature. Does it now extend its dominion, and conquering the life forces of our trees seize upon their treasured products and ruthlessly appropriate them to uses of its own? Whether this microscopic organism of the pear blight proves to be the same as that described or not, the evidence now given of disease *in plants* produced by bacteria contributes something to the germ theory of disease *in animals* and may lead to very important scientific and practical results.

In closing, I wish gratefully to acknowledge valuable suggestions from and assistance by Dr. H. J. Detmers, commissioned by the department of agriculture at Washington to investigate the diseases of domestic animals, and who is the author of a most important report upon Swine Plague or Hog Cholera.

FIELD WORK BY AMATEURS. By ELLEN HARDIN WALWORTH, of Saratoga Springs, N. Y.

IT is announced, I believe, that one of the aims of this Association is to make natural science popular, to encourage its pursuit among all classes of people. It is because I have such an understanding of its aims, that I presume to speak a word in behalf of the class who love science, yet can give to it but a limited portion of their time and thoughts.

Such a class of persons are important factors in the development of every department of knowledge and art. The professor, the artist, the specialist may have higher aims, they certainly do more thorough work, yet they would scarcely be understood, appreciated and encouraged if there did not exist the intermediate class who admire, applaud and exhibit the work they cannot themselves perform.

I therefore deprecate the scorn with which the professional too

often contemplates the dabbler in his specialty, as he will perhaps designate the amateur. "A little knowledge is a dangerous thing," only when it is pretentious. A mere elementary knowledge of any natural science is a proposition from which reason starts; it is a foundation on which thought builds, and a height from which imagination takes its flight. It is an educator in all other knowledge because it demands attention, observation and accuracy with well defined expression.

How can the popular interest in science be stimulated and increased? A majority of educated people shrink with aversion from the memory of tasks performed at school. The bare mention of a natural science recalls pages of unpronounceable words and incomprehensible classifications; yet if a practical geologist or botanist will take any three of these individuals into the field with him and beguile them into breaking rocks or gathering flowers scientifically, two out of every three will be delighted with the occupation and will strive to recall the classical names which inspired them with disgust while they were merely theoretical. It is then only while science is an abstraction, that it repels; render it practical and it invariably attracts.

In every city and village of our country we find numerous clubs and societies devoted to special objects of literature and art, and a few to science. These last are rare,— they would be numerous and active if slight encouragement were given to them by those who have the ability to guide and direct. Such clubs and associations should begin with a short and well directed course of reading, accompanied if possible by a few interesting lectures as a preparation for field work, which should not be delayed through timidity or a feeling of ignorance. A few visits to the field, by a geological club, will serve to arouse enthusiasm and inspire a desire for research which months of reading would not accomplish. It cannot be urged that many live in localities where there is nothing to study, for I believe it may be safely stated that uninvestigated scientific facts lie over and under every square mile of the United States. Yet I have heard the members of a geological club, who studied exclusively in the class room, make such a plea. When visiting their city I said to one of them, "What rocks have you in this vicinity?" The person addressed looked at me with unqualified surprise and answered, "We have none." I exclaimed, "You have a river, and hills, and many railroad cuttings; the

foundation of things must be visible somewhere." But this individual insisted that there was absolutely nothing to examine within walking or driving distance of that city. There is, of course, a difference in varying localities. In Davenport, Iowa, where there is now a well established Academy of Science, located in its own fine building, and displaying a great museum, a few years ago there were but half a dozen persons who met in a hired room to talk informally about science. They soon, however, began collecting and investigating in the suburbs of their city; it is, as you know, the region of ancient mounds. Their discoveries have been remarkable and valuable.

In Saratoga Springs where I reside, we have, in a limited way, an interesting geological region, and we have an active field club. It labors under disadvantages, having had no regular instruction, and no course of lectures, but it has been assisted by two gentlemen who have had some experience in geological research. As this club is now established upon an apparently permanent basis, some accounts of its efforts may not be amiss. There are between thirty-five and forty members, the larger number studying geology, a few botany, and others who are studying art accompany these to sketch from nature. This community of interest, among those who are pursuing different studies, has the advantages of economy in the hiring of vehicles and in the purchase of instruments like the microscope, which can be used in common. The Saratoga Field Club make excursions into the country every Saturday when the weather is favorable for field work; they also have in-door meetings once a week, to compare and examine specimens; papers are then read on subjects relating to special objects of study, and discussion and conversation concerning them are encouraged. Meetings are also held during the winter preparatory to the summer work.

We have in Saratoga the rocks of the Laurentian, the Lower Silurian, of the drift, the Champlain and Hudson river periods. But in the Laurentian granite alone there is an endless variety for those who are interested in minerals. Then, too, one experiences a certain awe in handling the oldest rocks that formed a boundary of the world's first continent. The gloom of that almost lifeless age seems still to creep along the dark scant foliage that strives to cover the baldness of these venerable rocks. Worn and ground by the action of ages they display few picturesque forms, but

strength and endurance seem moulded into shape among their rounded hills; while nestling among their unattractive gray shadows are found the garnet, the chrysoberyl, the tourmaline and other beautiful gems. The Potsdam Sandstone lying above the granite, shows great variety and beauty of color, and Ruskin says very justly "that nature tempts us, like foolish children as we are, to read her books by the pretty colors in them." The ripple marks and glacial scratches of this rock are also countless and interesting. The calciferous sand rock coming next in succession, and upon which the western half of our village rests, is in many places brilliant with crystals and finely marked with fucoids; it bears also whole acres of the marvellous concentric stromatopora which is peculiar to this vicinity. The Trenton limestone next above this is, as usual, rich in fossils, and an afternoon amid its quarries will render the members of the Field Club oblivious of heat or cold or fatigue, in their search for crinoids, brachiopods, and trilobites. Such interest is scarcely diminished in their laborious wanderings in other directions among the Hudson river slates and shales for the rarely found graptolites. The moraines and pebble-laden hills of the drift period are sought out and discussed. The sands of the Champlain and the terraces of the Hudson river periods are subjects for thought and surmise, as we ride over the country toward some definite object of investigation. The great geological fault which has given birth to our justly renowned mineral springs, coming forth as they do from the hidden fossil oceans of the buried centuries, stimulates us to ponder and to inquire.

Yet for years most of the members of this club walked blindly through these treasures, seeing but not observing, knowing but not seeing. A new world has been opened to them, and this world of nature and of science would be a revelation to hundreds of others if they were induced to engage in out-of-door studies. The public mind has been awakened to an interest in science by means of the popular lectures delivered by men of acknowledged fame, and also through numerous popular publications. These have been a preparation for field work which can now be pursued with enthusiasm and profit. A search for geological facts in the fields affords an admirable means of self-discipline. In the beginning each one sees all that he seeks, and believes that all he sees is of immense value, or he goes to the other extreme

and pronounces everything worthless. He will be vexed with himself, amused and mortified by turns, but each blunder will be an important lesson, and soon he will begin to discriminate, to learn, and to search until he finds himself like the hunter in pursuit of game, eager, excited, and ever ready for a new chase.

When the guidance of a professor of geology cannot be secured, much may still be done with the use of proper text books, and the state geological surveys; especially if there are a few men and women in the association who have some experience in field work. In every community a few gentlemen will be found who possess such knowledge. It is a deplorable fact that few women possessing such knowledge can be found in any community; except, of course, in Boston. I say deplorable, because scientific training is, of all others, that which women need to correct the defects which as a class they display; defects which have become inherent through continuous superficial training. In this case like must cure like, for it will require several generations of women gradually trained to scientific methods of thought and investigation to eradicate the slip-shod mental habits of the women of to-day. A few are struggling toward better and clearer ways, but the difficulties to be overcome prove the low standard of their starting point. Is it right that woman should be ignorant of the scientific facts embodied in the useful and beautiful things she handles? If these facts are of value to the world, they are of value to women individually.

Invite women, then, to enter upon this field of labor, and science will gain thereby. Enlist the enthusiasm, the self-sacrifice and vitality of women in the cause of science, and a new principle will stir the remotest members of the body of scientific knowledge.

The effect of this labor upon the lives of women is beyond calculation. Where they are now weak both physically and mentally they will become vigorous and strong, where they are complaining and sentimental they will grow cheerful and wise. Their restless longings will move into healthful channels, and they will learn to think, to observe, and to perform, with accuracy and deliberation. They will discover that the ability to learn and to do is not a mere knack to be caught, but that it is the result of continuous and painstaking labor.

Believing as I do, that a practical knowledge of natural science will do more for the advancement and emancipation of woman

than any laws that can be made, or any rights which can be granted to her, I appeal to the learned gentlemen of this association to invite and encourage woman to labor in the various departments of scientific investigation. The progress of woman depends on the exercise and discipline of her mental powers, and the proper expenditure and economy of her physical powers. Both are to be obtained mainly through a knowledge of the natural sciences, and these will take and retain their hold upon her most readily by means of out-of-door work.

There are also many men engaged in the professions and in business who would make time for open air excursions, if they thought a study of natural objects feasible.

In such studies of local geology the amateur may, by chance, make valuable discoveries, and he may in time become enlisted as an enthusiast and specialist.

Goethe says, that in science "treatment is nothing, all effect is in discovery; every new phenomenon that is observed is a discovery, and every discovery a property." If then it is allowable and desirable for amateurs to study science practically, it is important for them to receive suggestions and instruction from professors and specialists. In botany, several American publications have been issued which serve as admirable guides for such persons. In geology, I know of but one popular book on field work; that is an English publication. We need one especially adapted to American geology. A series of articles published in one of the popular magazines and bearing a name of authority would give a wholesome impetus to this work, and would reach many persons who desire information concerning it. These vague desires and feeble reachings after such knowledge should be noticed and cherished, for in these there may exist some of the future discoveries and triumphs of science.

TITLES OF OTHER PAPERS READ IN SECTION B.

THE PULMONATES OF THE PALÆOZOIC PERIOD. By J. W. Dawson, of Montreal, Can.

IRON MINES OF ORE HILL, CONN., AND VICINITY, AND THE MAKING OF PIG IRON. By W. A. Stearns, of Fishkill-on-the-Hudson, N. Y.

THE EXCAVATION OF THE UPPER BASIN AND CLOVE OF THE KAATERSKILL, CATTSKILL MOUNTAINS, N. Y. By Alexis A. Julien, of New York, N. Y.

CLASSIFICATION OF MOUNTAINS. By W. H. Niles, of Cambridge, Mass.

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NOTICE OF A COMPLETE BIBLIOGRAPHY ON PLATHELMINTHS. By Charles Sedgwick Minot, of Boston, Mass.

ORIGIN AND SUCCESSION OF FELIDÆ. By E. D. Cope, of Philadelphia, Pa.

THE METAMORPHOSIS OF ACTINOTROCHA. By E. B. Wilson, of New Haven, Conn.

NOTES ON THE MEDUSÆ OF BEAUFORT, N. C. By W. K. Brooks, of Baltimore, Md.

THE RHYTHMICAL CHARACTER OF SEGMENTATION. By W. K. Brooks, of Baltimore, Md.

DISTINGUISHING SPECIES OF POPULUS AND JUGLANS BY THE YOUNG NAKED BRANCHES. By W. J. Beal, of Lansing, Mich.

AN INVESTIGATION OF THE PEACH YELLOWS. By B. D. Halsted, of New York, N. Y.

EVOLUTION OF PARASITIC PLANTS. By Thomas Meehan, of Germantown, Pa.

INCOMPLETE ADAPTATION AS ILLUSTRATED BY THE HISTORY OF SEX IN PLANTS. By Lester F. Ward, of Washington, D. C.

PERMANENT

SUBSECTION OF ENTOMOLOGY.

ADDRESS

OF

MR. SAMUEL H. SCUDDER,

AS PRESIDENT OF THE ENTOMOLOGICAL CLUB OF THE ASSOCIATION, AFTERWARDS THE SUBSECTION OF ENTOMOLOGY.

LADIES AND GENTLEMEN:—

IT is the good fortune of your President on this occasion to welcome you to his native heath, where our favorite science has been longer, more uninterruptedly, and, perhaps more zealously cultivated, than anywhere else in the new world. Here, in the last century, Peck studied the canker-worm and the slug-worm of the cherry, and, in late years Rhynchaenus, Stenocorus and Cossus —all highly destructive insects. Here lived Harris, who cultivated entomology in its broadest sense, and whose classic treatise was the first important government publication on injurious insects Here, to-day, we have two associations for our work, consisting, it will be confessed, of nearly the same individuals, and not many of them, but meeting frequently—one in Boston, the other in Cambridge. Harvard acknowledges the claims of our study in supporting not only an instructor in entomology at its Agricultural School, but a full professor of the same in the University at large.

Harris attributed to Peck his special interest in entomology, and his first paper, that on the salt-marsh caterpillar, appeared in the *Massachusetts Agricultural Repository* only four years after Peck's last, in the same magazine, on cherry and oak insects. How many of us have drawn our first inspiration from Harris? Yet probably not one of our local entomologists ever saw him. The

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[illegible] comparisons). But at the best it is so only in
descriptive work we deal with simple facts; in comparative
work we deal with their collocation. "Facts," said Agassiz, one
day, "Facts are stupid things, until brought in connection with
some general law."

It is to this higher plane that concerns itself with general laws
that I would urge the young student to bend his steps. The way
is hard; but in this lies one of its charms, for labor is its own

ward. It is by patient plodding that the goal is reached; every step costs and counts; the ever-broadening field of knowledge exhilarates the spirit and intensifies the ambition; there is no such thing as satiety — study of this sort never palls.

It is hardly necessary to point out that so-called systematic work never reaches this grade unless it is monographic; unless it deals in a broad way with the relationship and general affinities of insects. It is not my purpose to call attention here to the needs of science in this department, as they are too patent to escape observation; but if one desires a model upon which to construct such work, one need not look further than the Revision of the Rhynchophora by Drs. LeConte and Horn. Rather than linger here, we prefer to pass directly to some of the obscurer fields of study.

When we compare the number of insect embryologists in America with that of their European colleagues, the result is somewhat disheartening and discreditable; although perhaps the comparison would not be quite so disproportionate were some of our students to publish their notes. But take all that has been done upon both sides of the water, and what a meagre showing it makes. Of how many families of Coleoptera alone have we the embryonic history of a single species? Of two of the four families of butterflies, the fertile eggs of which are perfectly easy to obtain, nothing is known. In short, one may readily choose numbers of typical groups whose embryonic history would be a great acquisition to science. Here is a broad field. From the special range of my own studies let me recommend to any one eager for this work to choose the eggs of our common copper butterfly, which she will lay to order on sorrel, and the earlier stages of which can be obtained from the parent at two or three different times of the year; or the eggs of any of our common skippers, which deposit on grass, and which are equally easy to obtain, although only once a year. Or, if we turn to Orthoptera, the eggs of our common Oecanthus, concealed all winter in raspberry twigs, are more transparent and more easily obtained than those of any other cricket; and our knowledge of the embryology of any of the Gryllidae is very fragmentary, and of this particular tribe, *nil*. Better still, perhaps, would be the choice of our common walking-stick, as it belongs to a bizarre and isolated type, now known to be of very ancient an-

general direction of Harris's studies doubtless arose from the predilections of his instructor; and the unprecedented growth of economic entomology in this country, where it flourishes as nowhere else, must be credited primarily to the influence of Harris's work. With every temptation which the wealth of new material about him could give, or which a very extensive correspondence with naturalists devoting themselves almost exclusively to systematic work, like Say, would naturally foster, he wisely followed the bent given his studies by his early training under Peck, and left a better example and a more generous and enduring influence.

In our own day, the spreading territory of the United States, the penetration of its wilds, and the intersection of its whole area by routes of travel, the wider distribution and greatly increased numbers of local entomologists, as well as the demand for our natural products abroad, have set also before *us* the same temptation to study only new forms and to cultivate descriptive work, to the neglect of the choicer, broader fields of an ever-opening science. It is to this danger I venture briefly to call your attention to-day, not by way of disparaging the former, but rather in the hope that some of our younger members, who have not yet fallen into the ruts of work, may be induced to turn their attention to some of the more fruitful fields of diligent research.

We should not apply the term descriptive work merely to the study of the external features of insects. The great bulk of what passes for comparative anatomy, physiology and embryology, is purely descriptive, and is only to be awarded a higher grade in a scale of studies than that which deals with the external properties, when it requires a better training of the hand and eye to carry it out and greater patience of investigation. We pass at once to a higher grade of research when we deal with comparisons or processes (which, of course, involve comparisons). All good descriptive work, indeed, is also comparative; but at the best it is so only in the narrowest sense, for only intimately allied forms are compared. In descriptive work we deal with simple facts; in comparative work we deal with their collocation. "Facts," said Agassiz, one day, "Facts are stupid things, until brought in connection with some general law."

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cestry, and of whose embryonic history nothing has been published. I have, indeed, a few incomplete notes upon this insect, but they relate wholly to a late period of development, and were made before the time of the microtome, when work over such coarse-shelled eggs was very difficult and unsatisfactory. The eggs may be readily procured, the insects being abundant in scrub-oak fields; the mother drops the eggs loosely on the ground, and from imprisoned specimens I have procured scores in a single season. Any one who will glance over the history of what has been done in insect embryology will be able to select a hundred examples as important and as easy to obtain as those already named, and by concentrating his work upon them will do better service than in aimless selection of what may come to his hands.

In following the post-embryonal history of insects there is work for all. While allied forms have in general a very similar development, there are so many which are unexpectedly found to differ from one another, that every addition to our knowledge of the life histories of insects is a gain, and they are to be praised who give their close attention to this matter. Here is a field any entomologist, even the most unskilled, may cultivate to his own advantage and with the assurance that every new history he works out is a distinct addition to the science. The importance of an accumulation of facts in this field can hardly be overestimated, and those whose opportunities for field work are good should especially take this suggestion to heart. Nor, by any means, is the work confined to the mere collection of facts. How to account for this extraordinary diversity of life and habits among insects, what its meaning may be, is one of the problems of the evolutionist. There are also here some specially curious inquiries, to which Sir John Lubbock and others have recently called attention, and to which Mr. Riley has contributed in this country by his history of Epicauta and other Meloidae. I refer to the questions connected with so-called hypermetamorphosis in insects. In these cases there are changes of form during the larval period greater than exist between larva and pupa, or even between larva and imago, in some insects. There are also slighter changes than these which very many larvae undergo; indeed, it may safely be asserted that the newly-hatched and the mature larvae of all external feeders differ from each other in some important features.

The differences are really great (when compared to the differences between genera of the same family at a similar time of life) in all lepidopterous larvae, as well as in all Orthoptera which have come under my notice. No attempt to coördinate these differences, or to study their meanings, or to show the nature of their evident relationship to hypermetamorphosis, has ever been made.

Not less inviting is the boundless region of investigation into the habits of insects and their relation to their environment. The impulse given to these studies by the rise of Darwinism, and the sudden and curious importance they have assumed in later investigations into the origin and kinship of insects, need only to be mentioned to be acknowledged at once by all of you. The variation in color and form exhibited by the same insect at different seasons or in different stations, "sports," the phenomena of dimorphism, and that world of differences between the sexes, bearing no direct relation to sexuality; mimicry also, phosphorescence and its relations to life, the odors of insects, the relation of anthophilous insects to the colors and fructification of flowers, the modes of communication between members of communities, the range and action of the senses,[1] language, commensalism,—these are simply a few topics selected quite at random from hundreds which might be suggested, in each of which new observations and comparative studies are urgently demanded.

The fundamental principles of the morphology of insects were laid down by Savigny in some memorable memoirs more than sixty years ago; the contributions of no single author since that time have added so much to our knowledge, notwithstanding the aid that embryology has been able to bring. Nevertheless there remain many unsolved problems in insect morphology which by their nature are little likely to receive help from this source. Let me mention three:

The first concerns the structure of the organs of flight. The very nomenclature of the veins shows the disgraceful condition of our philosophy of these parts: the same terminology is not employed in any two of the larger sub-orders of insects; names without number have been proposed, rarely however by any author with a view to their applicability to any group outside that which

[1] Notice Meyer's beautiful studies on the perception of sound by the mosquito.

formed his special study; and a tabular view which should illustrate them all would be a curious sight. A careful study of the main and subordinate veins, their relations to each other, to the different regions of the wing, to the supporting parts of the thorax and to the alar muscles, should be carried through the entire order of insects; by no means, either, neglecting their development in time, and possibly deriving some assistance in working out homologies by the study of their hypodermic development.

The second concerns the mouth parts. The general homologies of these organs were clearly and accurately enough stated by Savigny, though one may perhaps have a right to consider the last word not yet said when one recalls Saussure's recent claim to have found in Hemimerus a second labium. What I refer to, however, is another point: it relates to the appendages of the maxillae and the labium. Considering the labium as a soldered pair of secondary maxillae, we have at the most, on each pair of maxillae, three appendages upon either side. These appendages, as you know, are very variously developed in different sub-orders of insects, or even in the same sub-order; and it has at least not been shown, and I question if it can be done, that the parts bearing similar names in different sub-orders are always homologous organs. Here is a study as broad and perhaps as difficult as the last.

The third is the morphological significance of monstrosities, especially of such as are termed monstrosities by excess. The literature of the subject is very scattered, and the material much more extensive than many of you may think. At present this subject is, so to speak, only one of the curiosities of entomology, but we may be confident that it will one day show important relations to the story of life.

After all the labors of Herold, Treviranus, Lyonet, Dufour, and dozens of other such industrious and illustrious workers, some of you would perhaps ask: Is there anything important remaining to be done in the gross anatomy of insects? Let the recent work of some of our own number answer, which has shown in the Hemiptera and Lepidoptera the existence of a curious pumping arrangement by which nutritious fluids are forced into the stomach. It is certainly strange that after all that has been said as to the mode in which a butterfly feeds, that no one should have dissected a

specimen with sufficient care to have seen the pharyngeal sac which Mr. Burgess will soon show us. No! the field is still an open one, as the annual reviews clearly show. The curious results of Flögel's studies of the brain, the oddly constructed sense-organs found by Graber and Meyer (early noticed briefly by Leydig) in the antennae of Diptera, the important anatomical distinctions discovered by Forel in different groups of ants, the strange modification of the tip of the spiral tongue in Ophideres, which Darwin, Breitenbach and Künckel have discussed, and, above all, the extensive investigations of the nervous system in insects, generally, which Brandt has recently undertaken, and the keen researches of Graber in various departments of insect anatomy, show, by what has been accomplished, how many harvests are still unreaped. The microtome, too, has put a new instrument of precision into the hands of the investigator in this field.

We might in the same way point out some of the special needs in the study of the finer anatomy or histology of insects, but the pressure of other duties forbids a further pursuit of the subject. Enough surely has been suggested, even in this hasty sketch, to show that we cannot yet rest upon our oars, but must push forward undaunted into still unknown waters. If these few words shall arouse in any one a higher ambition leading to better work, their aim will have been accomplished.

PAPERS READ.

Further notes on the Pollination of Yucca and on Pronuba and Prodoxus. By C. V. Riley, of Washington, D. C.

At the Dubuque meeting of the Association, in 1872, it was my privilege to lay before you the substance of a then unpublished communication to the Academy of Science of St. Louis, embodying sundry facts regarding an interesting and anomalous little moth (*Pronuba yuccasella*, fig. 1), and its connection with the pollination of our Yuccas. The observations were made in the year 1872 and are recorded, with additional facts ascertained in 1873, in the Transactions of the above-named Academy (Vol. III, pp. 55–64 and 178–180), the *American Naturalist* (Vol. VII, Oct. 1873) and my 5th and 6th Reports on the Insects of Missouri. As the facts have never been published in our Proceedings, I will present the more important of them by brief recapitulation.

The moth (fig. 2, *b*, *c*,) has an immaculate white upper surface, and fuscous under surface in both sexes, and rests during the day for the most part within the half-closed flowers, the protective coloring of which serves to conceal it. The male possesses no very marked characters, but the female is most anomalous; *first*, in possessing a pair of prehensile, spinous, maxillary tentacles (fig. 1, *b*) found, so far as we now know, in no other genus of Lepidoptera; *second*, in possessing a long, horny ovipositor (fig. 1, *j*, and fig. 3), adapted to piercing and penetrating — a structure equally exceptional among Lepidoptera: "When the ovipositor is entirely withdrawn, the tip of the abdomen [fig. 3, *a*] presents a truncate appearance, the terminal joint being bluntly rounded at tip, with a slight projection both above and below, and a corrugated ridge dorsally a little in advance of the tip. This terminal joint is very much compressed from the sides, with a few stiff hairs around the terminal borders. The ovipositor issues from the middle of the truncate end, is very fine, tubular, the basal

joint [fig. 3, *b*] beautifully imbricato-granulate, the terminal joint [fig. 3, *d*] perfectly smooth, long and peculiarly constructed at tip, the extreme tip being notched or serrate, and a dorsal membrane, also finely and sharply serrate, running anteriorly from it—the whole recalling in form the caudal and second dorsal fins of the Lamprey (*Petromyzon*). Ventrally along the terminal joint is seen a membranous duct which broadens just in front of the tip and has an outlet from which a soft and extensile oviduct can be extruded [fig. 3, *e*]. The whole structure is, in fact, admirably adapted to cleaving through the fruit of the Yucca and then running into the ovarian cavity."—*Am. En-*

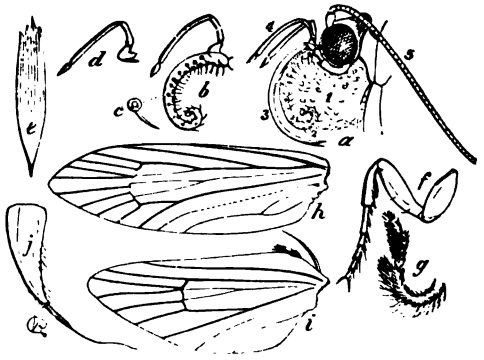


FIG. 1.—PRONUBA YUCCASELLA: Generic characters—*a*, side-view of head and neck of female denuded, showing how the collected load of pollen (1) is held by the tentacles (2); *b*, maxillary tentacle and palpus; *c*, an enlarged spine; *d*, palpus separated; *e*, scale from front wing; *f*, front leg; *g*, palpus; *h*, *i*, front and hind wings denuded; *j*, anal joint of female with ovipositor.

tomologist, III, p. 182.—Within the abdomen the ovipositor divides into four stout, horny rods which extend nearly its whole length, the upper pair being continuations of and aiding in the protrusion of the terminal joint of the ovipositor, the lower pair connected with the basal joint thereof. A careful study of the ovipositors, in the different species of *Pronuba* and *Prodoxus*, characterized in this paper, shows that they are all formed upon the same plan and that their diverse characters are but modifications of this typical form. They all agree in forming a solid piercing organ, which shields the exsertile, membranous oviduct. The nearest approach to such a piercing ovipositor in this Order will probably be found

in some of the *Pyralidæ;* for, in a moth of this family (*Clydonopteron tecomæ*, MS. mihi)[1] the ovipositor is slightly horny and flattened and apparently fitted for slipping under the skin of the pods of *Tecoma* in which its larvæ live. A somewhat similar structure is observable in some species of Pempelia and in *Pyralis olinalis*. In all other Lepidopterous insects known to me the ovipositor, when at all specialized, presents merely a horny sheath, either uniformly horny and cylindrical or having the dorsal and ventral edges more or less membranous and the sides more or less produced and sometimes pointed. In Paleacrita it is extremely elongate and fitted for secreting the eggs under the scales of bark. In some Noctuids, as in Leucania, it is very much compressed from the side, to fit it for secreting the eggs between blades of grass (a form repeated in the Pyralid, *Nomophila hybridalis*); while in others it is produced to a tolerably sharp point to enable the eggs to be thrust in between the scales of a bud, the terminal leaflets of a stem, or into the corolla of a flower. The more typical form of this kind of horny and exsertile ovipositor in the Order may be found in the Ægeriidæ, in which it is admirably adapted for probing the crevices of the bark of the plants in which the larvæ burrow. In the Zeuzerides, as in certain large Australian species, it is flattened to permit the thrusting of the eggs underneath the scales of bark of Acacia and Eucalyptus. In the Castniides (as in *Synemon sophia*, White) we find, again, a special modification, the terminal joint being armed with retrorse spines and having a rather sharp, blade-like tip, compressed laterally; but I know of no other Lepidoptera beyond those mentioned in this paper which have piercing and sawing ovipositors, with an extensile and elastic oviduct issuing from an orifice near and beneath the tip, and the highest living authority on European

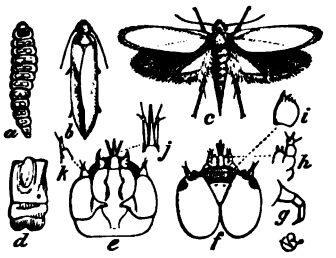


FIG. 2.—PRONUBA YUCCASELLA: *a*, larva; *b*, ♀ moth with closed wing; *c*, do. with wings expanded—nat. size; *d*, side view of larval joint; *e* head of larva, beneath; *f*, do. above; *g*, thoracic leg of same; *h*, maxilla, *i*, mandible; *j*, spinneret and labial palpi; *k*, antenna—enlarged.

[1] Figured and described since this paper was read, in the *American Entomologist*, III, p. 287.

Tineids, Mr. H. T. Stainton, wrote me some years ago that there was no European genus in the family at all analogous to Pronuba in this respect.

The internal organization will doubtless show peculiarities corresponding to the external structure; and one feature is worthy of mention, because very noticeable even through the sides of the body when that is rendered in any way transparent. It is a pair of brown, chitinous, radiate structures, each with a darker, circular, central ring, and extending across the third abdominal joint. More carefully studied, the dark circle is seen to represent the end of a hollow, though shallow, cylinder from the sides of which the

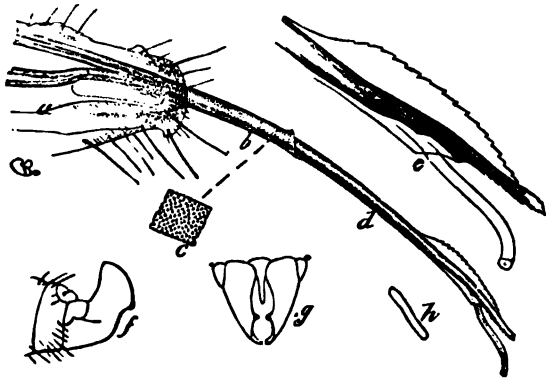


FIG. 3.—Genital characters of Pronuba yuccasella: *a*, tip of ♀ abdomen rendered somewhat transparent; *b*, basal joint of ovipositor; *c*, its sculpture; *d*, terminal joint of same; *e*, tip still more enlarged; *f*, genitalia ♂ from side; *g*, do. from above; *h*, egg.

filaments or spicules radiate as spokes from a hub. These radiate objects seem to distend the walls of a sack, which is evidently the *bursa copulatrix*, as Dr. Hagen has clearly made out in his recent studies of *Prodoxus decipiens*, in which the radiating spicules are fewer, shorter and thicker than in *Pronuba*.

The egg of *Pronuba* (fig. 3, *h*), which is very soft and plastic, is thrust through the pistil or young fruit and into the ovule. The larva (fig. 2, *a*), which is peculiar in possessing no prolegs, feeds upon the seeds, issues from the capsule as it ripens, and hibernates in a tough cocoon underground. The chrysalis (fig. 4) offers characters which enable us to separate the sexes in this state,—

a very unusual occurrence — and is admirably adapted, by means of spade-like dorsal spines, for forcing its way up through the ground, which it does to give forth the moth at the time of the flowering of its food-plant.

In regard to the habits of the female moth, which are more particularly interesting, I shall take the liberty of quoting the original language used:

"Before she [the female] can carry out the maternal task of continuing her race, she must act as foster-mother to the plant in order to insure a proper supply of food to her larvæ, which feed on its seeds. With her maxillary tentacle, so wonderfully modified for the purpose, she collects the pollen in large pellets, and holds it under the neck and against the front trochanters. In this manner she sometimes carries a mass thrice the size of her head. Thus laden, she clings to the top of the pistil, bends her head, thrusts her tongue into the stigmatic nectary, and brings the pollen mass right over its mouth. In this position she works with a vigor that would indicate combined pleasure and purpose — moving her head and body from side to side, and apparently making every effort to force the pollen into the tube. Such is the method by which our Yuccas are fertilized."[2]

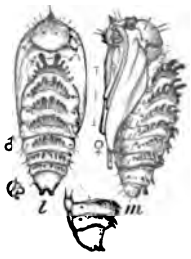


FIG. 4. — PRONUBA YUCCASELLA: *l*, male; *m*, female chrysalis.

Or, again, from the observations of 1873:

"The Yucca flowers are fully opened and perfect during a single evening and night only, and it is during this, the first night of blooming that eggs are consigned to the somewhat prismatic pistil. The pollen grains are not so often expelled, to fall on the inside of the flower, as I had been led to suppose; but almost always remain in an entire lump on the contracted and curled anthers. The moth, consequently, has no difficulty in accumulating her little load of pollen, for a single anther furnishes nearly the requisite amount.

"Once equipped with this important commodity, she may be seen either crawling over or resting within the flower. From time to time she makes a sudden start, deftly runs around and among the stamens, and anon takes position with the body between and the legs straddling some two of them—her head turned towards the

[2] 5th Rept. Ins. Mo., p. 152.

stigma. As the terminal halves of the stamens are always more or less recurved, she generally has to retreat between two of of them until the tip of her abdomen can reach the pistil. As soon as a favorable point is reached—generally just below the middle—the lance-like sheath of the ovipositor, which consists of four converging corneous bristles, is thrust into the soft tissue, held there a few seconds while the egg is conducted to its destination, and then withdrawn by a series of up and down movements. So intent is she upon this work that after the ovipositor once penetrates the pistil the whole perigon may be detached, some of the encumbering petals and stamens removed, the insect brought within the focus of a good lens, and all her movements observed to the greatest advantage, without disturbing her. In this way, I have been able to watch the consignment of hundreds of eggs, and to admire the delicacy and elasticity of the ovipositor proper, which issues from the setaceous sheath in a silk-like thread, almost invisible to the naked eye, and as long as the terminal abdominal joint; and which stretches and bends according as the body is raised or lowered.

"No sooner is the ovipositor withdrawn into the abdomen than the moth runs up to the top of the pistil, uncoils her pollen-bedecked tentacles, thrusts them into the stigmatic opening, and works her head vigorously as I have previously described — the motion being mostly up and down and lasting several seconds. This carrying of the pollen to the stigma generally follows every act of oviposition, so that where ten or a dozen eggs are consigned to a single pistil, the stigma will be so many times be-pollened. The ends of the tentacles, which are most setose and spiny, and which are always curled into the pollen-mass when not uncoiled, must necessarily carry a number of pollen grains each time pollination takes place; and I have noticed a gradual diminution in the size of the collected mass, corresponding, no doubt, to the work performed, which is indicated by the rubbed and worn appearance of the individual — the freshest specimens always having the largest loads.

"While oviposition generally takes place in the manner described, the moth head outward and straddling two stamens, an entirely opposite position must sometimes be assumed, since larvæ and punctures are not unfrequently found in the upper part of the fruit, especially where a single one is stocked with ten or a

dozen larvæ, as is sometimes the case. As the fruit enlarges, the mouth of the puncture forms a slight, discolored depression, more noticeable in some varieties than others; but the passage-way becomes obliterated."[3]

The above are some of the leading facts in the natural history of *Pronuba yuccasella* that can be verified by any one who cares to take the trouble to observe them.

The insect offers, however, such a remarkable instance of special modification of parts to a particular end, and there is such a striking interdependence between it and its food-plant, that Hermann Müller, who, from his extensive studies of the relations between plants and insects, is most competent to speak on the subject, avowed it to be the "most wonderful instance of mutual adaptation"[4] yet detected. The original observations have, as a consequence, been widely commented on. My conclusions have not unfrequently been criticised, and too often, I regret to say, by those who seem not to have taken the trouble to read the original articles. With a view of emphasizing the facts already published and of presenting others recently ascertained, let me briefly consider the criticisms that have been made:

1. Capsules are often found uninfested with the *Pronuba* larva, and several writers have assumed that such could not have been fertilized by the moth and that this last is not necessary to produce fertilization.

The following quotations from the original articles offer a sufficient reply to this objection:

"It is quite possible that the moth may, at times, introduce the pollen into the stigmatic tube without consigning any of her eggs to the fruit, and we should naturally expect to find some capsules uninfested with her larvæ. But I have this year examined hundreds of capsules around St. Louis, and some in South Illinois, and not more than four or five per cent. were uninfested. Sometimes every pod on the same plant had its worms, while at others half the pods on a given panicle would be free from them. From the very large percentage of infested pods, I conclude that oviposition naturally and immediately follows fertilization, unless the moth be disturbed."[5] "While oviposition is generally followed (and not pre-

[3] 6th Rept. Ins. Mo., pp. 133–4.

[4] In correspondence with the writer. See also *Zoologische Garten*, Frankfort, Oct. 1874.

[5] Trans. Ac. Sc. St. Louis, III, p. 62.

ceded, as I formerly supposed) each time by pollination, yet the former sometimes takes place twice, thrice or oftener without the latter being performed; and I suspect that the converse of this is equally true."[6]

2. Yucca occasionally and exceptionally seeds in Europe, and several horticultural writers arguing from this fact, and doubtless without informing themselves of what I had really written, have repudiated any necessary connection between the moth and the flower. A single illustration from a recent editorial in a leading horticultural journal, the London *Garden*, and approvingly quoted in *Vicks' Illustrated Monthly Magazine*, for March, 1880, will suffice:

"I fancy insect agency is talked of in a very unscientific way by too enthusiastic followers of Mr. DARWIN. I remember reading Professor RILEY's dogma that the Yucca could only possibly be fertilized by a certain American insect, and being amused at it because I had seen it fruiting well at the south of Europe, and also in France. Probably we should see it oftener if we had a better climate. I now note what Mr. ELLACOMBE, writing to a contemporary, says: 'There can be no doubt that the Yucca can be fertilized by other means than by the agency of the Yucca moth, *Pronuba yuccasella*. I have more than once had well formed fruit of *Y. recurvifolia*, but the seeds did not come to maturity.' Dr. ENGELMANN, in his *Notes on the Genus Yucca*, says: 'In the botanical garden of Venice I gathered the pulpy pods from a large *Yucca aloifolia*, about fifteen feet high. This was the only Yucca fruit seen by me in Europe, though I have since learned that in other instances, also, though only exceptionally, fruit and good seed have been produced there, principally by the same species, and very rarely by others.' I remember the late Mr. BARILLET telling me he had raised a great many varieties of *Yucca gloriosa* from seed saved in France. However, this mistake on the part of so good a man as RILEY is good sense compared with what we read on this side as to the influence of insects on the color and odor of plants. The statement by Mr. WALLACE, for example, that showy flowers are scentless, because from their color they are sufficiently attractive to insects, may pass for science with some innocent people, but it seems foolish to those who know even only a few garden plants."

Again let my original language do duty in reply:

"Our Yuccas, on the contrary, *seem* to depend for assistance, *so far as we now know*, on the single little Tineid which I have described, and, for this reason, are among the most interesting of entomophilous plants. At least such is the case with the capsule-bearing species, *i. e.*, those which have dry, dehiscent pods; and I will here premise that my observations have been made upon a filamentose-leaved species in common cultivation about St. Louis, and which Dr. Engelmann takes to be *Y. puberula* or *Y. glauca*.

[6] *Am. Nat.*, Oct. 1873.

"The fructification of such Yuccas as bear fleshy, pulpy fruit, of which *Y. aloifolia* may be taken as the type, has not been studied; but, even with this last mentioned species, the facts, so far as known, strongly indicate that *Pronuba* is principally, if not solely, instrumental in bringing it about. Its seeds are infested with our *Pronuba* larva, though not to the same extent as those of the dehiscent species. It would be premature to speculate until we have further facts; but it is not at all unlikely that the seeds of the fleshy pods are less congenial to the larvæ, and that a smaller percentage [of these] is produced from the eggs consigned to such pods by the moth."[7]

Those parts of the quotation which I have emphasized show little inclination to form sweeping conclusions, not warranted by the facts observed. Again, after bringing together such facts as were then obtainable respecting the seeding of Yucca in different parts of America and Europe, and citing the case of the seeding of *Y. filamentosa*, observed by Mr. T. Smith at Newry in England, in 1868, I wrote:

"These extracts prove that the Yucca moth occurs on Long Island, and around New York, and indicate that other insects occasionally pollinize the flowers. The experience of Mr. Smith, in England, is as interesting as it is exceptional; but until we learn whether or not the work of the larva was manifest, no safe conclusions can be drawn. *Other insects may have been the pollinizers, or Pronuba may have been locally introduced with seed from America.* This last view may not appear very plausible, but if both sexes of the insect were, by some chance, introduced into a locality where Yuccas of blooming age were growing, there is no reason why they should not multiply; and such chance introduction is not impossible, since the larva not unfrequently remains in the capsule after the seed is ripe, where it fastens a number of the riddled seeds together into a sort of cocoon, which might easily pass unnoticed in gathering seed; and, if buried in the ground with such seed, would in time give forth the moth."[8]

And after a second summer's investigation, I further wrote:

"My observations this summer might be extended much in detail. They have convinced me more than ever that *Pronuba* is the only insect by the aid of which our Yuccas can be *fully*

[7] Trans. Ac. Sc., St. Louis, III, pp. 58-9.
[8] 5th Rep. Ins. of Mo. (1872) p. 159.

fertilized: for I have studied this fertilization diligently night after night, without seeing any other species go near the stigma. The stigmatic opening closes after the first night, and I know of no crepuscular or nocturnal species which could collect the requisite amount of pollen and bring it so to bear on the stigma that each ovule would receive the influence of a pollen grain. The species already enumerated as frequenting Yucca[9] are mostly diurnal, and have nothing to do in the work; and wherever I have excluded the moth from the flowers, by enclosing the latter with netting, no fruit has been produced. *I am, therefore, led to believe that the few rare instances of yucca fertilization, in localities where* PRONUBA *may be presumed not to occur, have been brought about by another insect accidentally, or by the stamens reaching an exceptional length, and the anthers being brought into contact with the stigma by the conniving of the closing petals.* I have found the stamens of varying length in the flowers on the same panicle, and in some instances almost as long as the pistil."[10]

The accompanying figure (fig. 5) illustrates a few pistils from the same species of Yucca, and indicates (*a*) a deformity of tolerably common occurrence in which the anthers may easily be brought in contact with the stigma, and also (*b*, c, *d*,) varying length of the stamens. I also figure (fig. 6) a flower of *Yucca*

[9] I take the opportunity in this connection to elaborate the list given in my 5th Mo. Report (p. 154) of insects known to me to frequent and infest Yucca. COLEOPTERA. Frequenting the flowers and evidently feeding upon the pollen: *Carpophilus yuccæ* Cr., *C. pallipennis* Say, *Euphoria melancholica* Gory, *E. fulgida* Fabr., *Trichius delta* Forst., *Chauliognathus americanus* Forst., *C. marginatus* Fabr., *Strangalia strigosa* Newm., *Typocerus zebratus* Fabr., *Hymenorus densus* Lec.. *Anthonomus signatus* Say and many other species of less frequent occurrence. Living in the fruit: *Silvanus quadricollis* Guér., *Carpophilus mutilatus* Fabr., *Epurœa luteola* Er. and a Trogositid larva, presumably of *Trogosita yuccæ* Cr. Living in the dead flower stem: *Hypothenemus hispidulus* Lec. Living under the bark, or boring in the root-stem of the plant: *Carpophilus melanopterus* Er., larva of *Trogosita yuccæ* Cr., larva of *Elaphidion* sp. (probably *tectum* Lec.), *Scyphophorus yuccæ* Horn and *Yuccaborus frontalis* Lec. Dr. Horn mentions *Rhigopsis effracta* as feeding on Yucca (Proc. Am. Phil. Soc. XV, p. 37) but the particular part of the plant is not given. LEPIDOPTERA. Living in the flesh of the fruit: an unbred Tortricid larva and exceptionally, the larva of *Prodoxus decipiens* Riley. Feeding on the seeds: larva of *Pronuba yuccasella* Riley and presumably of *P. maculata* Riley. Living in the fresh flower-stems: larva of *Prodoxus decipiens* Riley and presumably of *P. intermedius* Riley, *P. marginatus* Riley, *P. cinereus* Riley, *P. œnescens* Riley. Boring in the root and the root-stem: *Megathymus yuccæ* Boisd. and the variety *coloradensis* Riley and presumably *M. cofaqui* Strecker. Mr. V. T. Chambers mentions that *Holcocera gigantella* occurs on Yucca in Colorado. HETEROPTERA. Puncturing and sucking the fruit: *Lygus robiniæ* Uhler, *Orthotylus discoidalis* Uhler, *Cyllocoris scutellatus* Uhler and *Theognis phyllopus* Uhler (*albicinctus* Say).

[10] *Am. Nat.*, Oct. 1873.

aloifolia in which species the stamens reach nearly to the stigma, and in which, were it not for their recurved ends, some of the pollen might fall on the papillose apex of the stigma while the flower hangs or droops.

It is so very plain from the above quotations that, while I have held and still maintain that it is the rule for our Yuccas to be pollinized by *Pronuba*, I have nevertheless admitted that the rule is not without exceptions. The rarity of yucca-fructification in those parts of America or in other countries where the plants are not indigenous and *Pronuba* may be presumed not to occur; the uniform failure to fructify whenever the moth has been excluded from the flowers both in my own experiments and those of Mr. J. M. Milligan[11] and of Mr. Meehan; and the non-fructification, even where the moth exists, of those species which bloom either

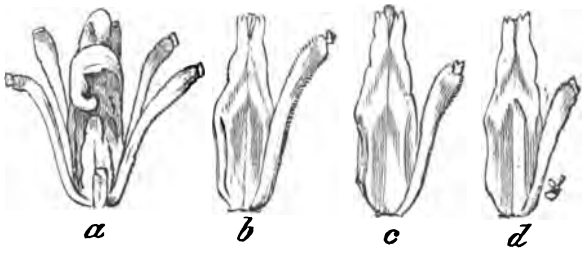


FIG. 5.—Young fruit of Yucca filamentosa, showing (*a*) a deformed pistil and (*b*, *c*, *d*) varying lengths of the stamens.

before or after she appears—all serve to emphasize the rule, and the few exceptions confirm it all the more markedly that (as Mr. Meehan has shown us) the plant is so easily fructified whenever the pollen is brought to the stigma or even to the papillose apex thereof.[12] It would be wonderful indeed, if the plant were not fructified occasionally in some one of the exceptional modes here indicated and more particularly by some of the pollen-feeders like Chauliognathus.

[11] *Am. Nat.*, VIII, p. 749 (1874).

[12] I do not doubt the correctness of Mr. Meehan's observations; yet it is rather singular that the only other recorded experiments of the same kind made in this country, viz., those of Mr. Milligan (see the paper above cited) should give quite different results from those obtained by Mr. Meehan. Mr. Milligan says: "In the three experiments where pollen was used, the pollen was placed as nearly as possible in the same position in each flower, viz., just at the entrance of the stigmatic tube; the result confirms the view taken by Dr. Engelmann in relation to the difficulty of this flower being pollinized by other natural agencies than that afforded by the Yucca moth." In two instances I have tried to fructify *Yucca aloifolia* artificially without avail.

The chances that *Pronuba* may occur in some parts of the world where it had not before existed have also increased by my having, during the years 1873 and 1874, sent to correspondents a number of larvæ in their cocoons with the request that they be buried in the ground near Yuccas. Of those thus sent were 100 to Mr. Meade Woodson, Kansas City, Mo.; 150 to Dr. Asa Gray, Cambridge, Mass.; 100 to Professor J. E. Planchon, Montpellier, France; 250 to Hermann Müller, Lippstadt, Germany; 100 to Chas. Darwin, Down, near London, Eng.; and 50 to H. T. Stainton, Mountsfield, near London, Eng.

FIG. 6.—Flower of Yucca aloifolia fully opened.

3. The accuracy of my observations has been called in question, and notably by Mr. Jacob Boll[13] of Dallas, Texas, who made some himself which seemed to conflict with mine;[14] while much doubt was thrown upon the utility of *Pronuba* to the plant by certain statements made in a paper read by Mr. Meehan at the last meeting of this Association.[15] As I have elsewhere

[13] Deceased since this was written.

[14] *Stettiner Entomologische Zeitung*, 1876, pp. 401-5.

[15] Mr. Meehan's paper "On the fertilization of Yucca" is not published in the Proceedings, but was printed in the *North American Entomologist* for November 1879 (vol. I, p. 33). It is an attempt to rebut the evidence existing as to the benefits derived by plants as species from cross-fertilization, Yucca being used as an illustration. The author lays stress on the *supposed* rarity of the seeding of Yucca in Colorado and Utah. I say *supposed* because my own experience with Yucca in Colorado (during three

replied to Mr. Boll's strictures,[16] and have more recently considered Mr. Meehan's statements in an article in the *American*

different years), as well as that of others, is in direct conflict with it and shows that the seeds are very generally found wherever a flower-stalk has been thrown up and it has not been prematurely browsed by cattle or cut by man. Mr. Meehan's statements of facts in this matter of insect fertilization have frequently greatly differed from those of others and led him to views in opposition to those generally accepted. In some instances they are pure misstatements or misapprehensions, as in the famous one made before the Association in 1876, viz.: that bees and insect fertilizers of flowers were rare in the Rocky Mountain region where bright flowers abound ("On self-fertilization and cross-fertilization of flowers" *Penn. Monthly*, 1876, vol. 7, p. 834). It was my privilege in answer to this statement to show that the insect fauna of that region was peculiarly rich in pollinizers whether among Hymenoptera, Lepidoptera, Coleoptera, or Diptera, and the experience of extensive collectors in that region, and of Messrs. J. D. Putnam, E. A. Schwarz and H. K. Morrison more particularly, has fully corroborated this view. Dr. LeConte's published papers (Coleoptera of the Alpine Rocky Mountain region; Hayden's Bull. U. S. Geol. and Geog. Surv., IV, p. 447, and V, p. 499), also confirm it. I am not aware, however, that Mr. Meehan has admitted the error whether in the misstatement or the conclusions drawn therefrom. I feel, therefore, that it is desirable to point out the erroneous statements in the article "On the Fertilization of Yucca" to facilitate the doing of which I quote from it the following passages:

[1] "It may be remembered that at our meeting at Buffalo I produced three capsules that had not been produced by this elaborate process, but simply by mere touching of the papillose apex with one of the flower's own polliniferous anthers. Professor Riley was so sure that the seed-vessels could not have been produced in that way, that there must have been some insect agency unknown to me in addition to my work, that at the conclusion of my paper he asked permission to cut open the capsules, sure of being able to show the larvæ in the fruit; but he found them not. I recall these matters to show that I have not misapprehended the position our friends take on this question."

There is, notwithstanding, both grave misstatement and misapprehension. Having known a few pods to develop from the last and topmost flowers of *Y. angustifolia* at St. Louis with evidence that *Pronuba* was the fertilizer (see Trans. Ac. Sc. St. Louis, III, p. 570), I stated the fact and made the point that the pods which Mr. Meehan exhibited might after all have been fertilized by *Pronuba* and that I should not be surprised to find its larvæ within them. It never entered my mind to be "sure" that the flower could not be pollinized through human instrumentality, or to be "sure" of finding the larvæ in the pods, and the misconstruction of the remarks is quite unjustifiable.

[2] "At the conclusion of this address, delivered at the Saratoga meeting of the American Association, Professor C. V. Riley made some remarks which unfortunately I did not hear. The newspaper reports make him say that I was mistaken in the insect I found in *Yucca angustifolia*, that it was not *Pronuba yuccasella*. I have called Professor Riley's attention to this and have asked for a correct note of what he did say, but have only the reply that he is 'not answerable for a newspaper report.' It remains then only for me to say in reply to the "newspaper report" that at the outset of my observations on *Yucca angustifolia*, I sent one of the insects caught to Professor Riley asking: 'Is this certainly *Pronuba yuccasella?*' and he replied that it was."

It remains only for me to remark on the above that I was never guilty of the discourtesy to my friend implied in the first quotation from my correspondence, but that in stating that I could not be answerable for newspaper reports which I had not seen, I also expressed my willingness, in order to avoid misapprehension, to write out my criticism of the paper if the author would send me the MS. In reference to my determination as *Pronuba yuccasella* of a specimen sent me by Mr. Meehan, it was not preserved because pressed and spoiled in the letter, so that I cannot now verify my determination. *Prodoxus decipiens* being unknown to me then, my determination may have been erroneous, or there may have been some error in the fact stated by my correspondent which, in this instance, was not our author but his son, Mr. Jos. T. Meehan. That there was an error somewhere is rendered probable by the facts recorded in the present paper.

[16] Trans. Ac. Sc. St. Louis, III, p. 571; *Stettiner Ent. Zeitung*, 1878, p. 377.

Entomologist for June last (vol. III, p. 142) entitled "The true and the Bogus Yucca Moth," it suffices here to state that the strictures and statements of both gentlemen resulted from mistaken identity, and that the conflicting experiences are explained by the existence of a moth which I have named *Prodoxus decipiens* and which, while bearing a very deceptive general resemblance to *Pronuba yuccasella*, yet differs essentially in structure and habit.

Prodoxus decipiens[17] was first announced by me before the Entomological Club of the Association at its Saratoga (1879)

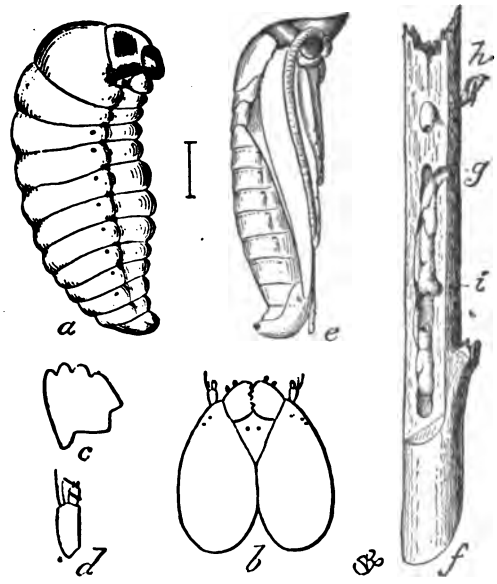


FIG. 7.—PRODOXUS DECIPIENS: *a*, larva; *b*, head from above; *c*, *d*, left jaw and antenna; *e*, pupa; *f*, infested stem cut open to show the burrows, castings, cocoons and pupa shell (*h*)—all enlarged but *f*, the hair line between *a* and *e* showing natural length.

meeting. It breeds either in the flower-stem or in the flesh of the fruit of different species of Yucca and is common through the Southern states, both on *Yucca aloifolia*, *Y. rupicola* and *Y. gloriosa*. In the West it is found on *Y. angustifolia*, while at Washington I have found it not uncommon on the different cultivated forms of *Y. filamentosa*. It also occurs around Philadelphia and has been bred by Dr. H. A. Hagen and by myself from larvæ

[17] *American Entomologist*, III, pp. 153 and 182.

infesting the flower-stems of *Y. filamentosa* grown in Mr. Meehan's grounds, thus furnishing strong presumptive evidence that the moths which Mr. Meehan assumed to be *Pronuba yuccasella*, and which he found upon *Y. angustifolia* long before the flowers of *Y. filamentosa* had opened, were in reality the *Prodoxus* or Bogus Yucca Moth, as I surmised would be the case in my comments upon his paper at the meeting of the Association a year ago. Its occurrence there weakens the inference which Mr. Meehan drew in his paper that, because *Pronuba* did not pollinize *Y. angustifolia*, therefore it did not pollinize *Y. filamentosa* and entirely nullifies the conclusions in the following paragraph from that paper in which, moreover, as in the affirmation (without a

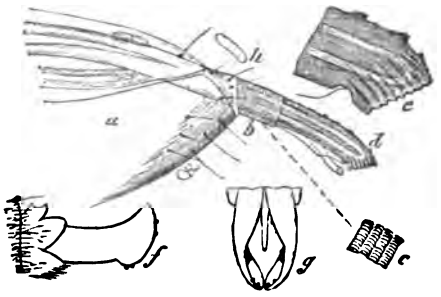


FIG. 8.—Genital characters of Prodoxus decipiens: *a*, tip of ♀ abdomen rendered somewhat transparent; *b*, basal joint of ovipositor; *c*, its sculpture; *d*, terminal joint of same; its tip more enlarged; *f*, genitalia of ♂ from side; *g*, do. from above; *h*, egg.

particle of proof) that the moth feeds on other flowers, there are some very unscientific statements.

Pronuba yuccasella, the Yucca Moth, has for years abounded on my flowers of the *Yucca filamentosa*. It has not been known to visit any other plant than Yucca. *Yucca angustifolia* begins to flower from three to two weeks and its blossoming is all over before *Yucca filamentosa* begins to open. The facts now adduced show that the moths exist weeks before the flowers bloom with which they have been so intimately connected, feeding of course on other flowers, and would perhaps make use of other fruits as depositaries for their eggs if Yucca should not exist. At any rate the facts weaken any belief we may have that the Yucca and Yucca Moth, through the long ages, have become mutually adapted to each other through a fancied mutual benefit.

The accompanying figures will sufficiently indicate the structural peculiarities of *Prodoxus decipiens* and how markedly it differs from *Pronuba yuccasella* in the larva (fig. 7, *a*) having no legs (lacking even the thoracic legs which belong to the latter); in the pupa (*e*), among other differences, lacking the series of

dorsal spade-like projections; in the form of the claspers in the male moth (fig. 8, *f*, *g*); in the much stouter and differently shaped ovipositor of the female (fig. 8, *b*, *d*); and especially in her lacking the maxillary tentacles. Oviposition has not yet been observed, but stems that have been badly infested are usually covered with small elongate-oval scars or swellings with a depression along the middle as at fig. 9, *a*. These are undoubtedly the scars of the wounds made by the ovipositor, though comparatively few of them show evidence of having led to an egg or a larva. I cannot better set forth the differences in the two insects, whether in structure or in habit than by quoting the concluding portion of the article in the *American Entomologist* above alluded to:

FIG. 9.—Flower stem of Yucca, showing scars made by female Prodoxus decipiens in ovipositing (*a*, *a*) and pupal exuvia protruding (*b*, *b*, *b*,).

"The larva of *Prodoxus* never quits the stem in which it lives. It eats comparatively little, packing its pale, buff-colored excrements very tightly in its burrow, and spinning as winter approaches a neat cocoon of white silk covered on the outside with its castings [fig. 7, *f*]. Prior to forming its cocoon, a passage way is always made to the outside of the stem, leaving but a very thin covering [fig. 7, *g*]. In issuing, the chrysalis pushes half way out, very much as is the case with all other Lepidopterous endophytes [fig. 7, *h*; fig. 8, *b*]."

* * * "Who, studying these two species in all their characters and bearing, can fail to conclude that, notwithstanding the essential differences that distinguish them not only specifically but generically, they are derivations from one and the same ancestral form? *Pronuba*, depending for its existence on the pollination of the flower, is profoundly modified in the female sex in adaptation to the peculiar function of pollination. *Prodoxus*, dwelling in the flesh of the

fruit or in the flower-stem, and not depending upon the fructification of the plant, is not so modified, but has the ordinary characters of the family in both sexes. In the former, the larva quits the capsules and burrows in the ground: it has legs to aid it in its work, while the chrysalis is likewise beautifully modified to adapt it to prying through the ground and mounting to the surface. The latter, on the contrary, never quitting the stem, has no legs in the larva state, and in the chrysalis state is more particularly adapted, by the prominence of the capital projection, to piercing the slight covering of the stem left ungnawed by the larva. The former is very regular in its appearance as a moth at the time of the flowering of *Yucca filamentosa*, a fact which would indicate that it was modified while living upon that species and had a range coëxtensive with it and other species blooming simultaneously. The latter appears earlier, as the food of its larva is earlier ready. Which of the two insects is the older in time, or whether the divergence from some archetypal form has been simultaneous, is a matter of opinion which those interested in evolution will decide for themselves one way or the other, or according as knowledge increases. That other species of both these genera will yet be discovered, there can be but little doubt."

Since the preceding quotation was written I have detected a species of *Prodoxus* which is still more closely related to *Pronuba yuccasella*, and have obtained another species of *Pronuba* and several of *Prodoxus*, collected upon Yucca (species unknown) at Caliente, Kern Co., Cal., by Mr. H. K. Morrison. The insects seem to be not uncommon there, but Mr. Morrison searched in vain for other species.

PRONUBA MACULATA n. sp.—Expanse, ♀ 21.5 mm; ♂ 17.8 mm. *Head* white, eyes black, mouth parts yellowish but very sparsely clothed with white scales; antennæ yellowish, the tips fuscous, sparsely clothed with scales on the basal half only; maxillary tentacle (fig. 10, *mt*) yellowish, the base green, large, more swollen and with the notch or elbow near the base more distinct than in *yuccasella;* maxillæ bristly (in *yuccasella* they are nearly smooth). *Thorax* white: primaries (fig. 10, *pr*) with the upper surface opaque white, bordered with from 10–12 black spots (when all are present) of varying size and running from just beyond the middle of the wing to about the inner angle; the disk with 5 spots, 4 of them in a line from base to apex, the 2d and 3d rather farthest apart, the 5th, or middle one, below and nearest the 3d from the base; under surface deep fuscous, intensified around the borders and with the spots of upper surface barely indicated; fringes white: secondaries transparent, being

very slightly covered with elongate fuscous scales, thickening on the borders, especially toward apex; fringes faintly fuscous: legs white, tarsi very sparsely clothed and yellowish except at extreme tips which are more fuscous. *Abdomen* white beneath, fuscous above. Integument of head yellowish, of thorax and coxæ black or blackish. Terminal abdominal joint in ♀ short and thick, obliquely truncate from beneath (fig. 10, *a*) bare, honey-yellow with its base black; ovipositor stout, decurved, the terminal, horny joint (fig. 10, *t j o*) broad and rounded at the tip and its edge finely denticulate. Claspers of ♂ short and abruptly curved upward as in *yuccasella*, but armed with only one tooth and this tooth greatly lengthened and preceded by a deep excavation (fig. 10, *c*.).

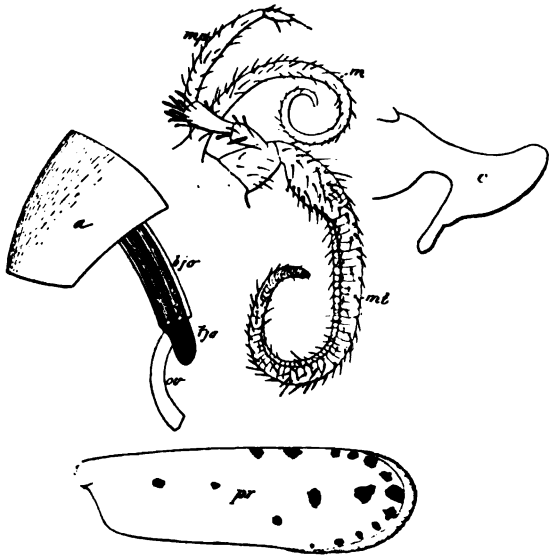


FIG. 10.—PRONUBA MACULATA: *a*, tip of female abdomen; *b j o*, basal joint of ovipositor; *t j o*, terminal joint do.; *ov*, oviduct; *m*, maxilla; *mp*, max. palpi; *mt*, maxillary tentacle; *c*, clasper of male from side; *pr*, front-wing showing arrangement of spot—hair line showing nat. size.

Examination of the venation in one male shows that marginal veins 7 and 8 arise from the transverse vein independently of the discal branch of the subcostal vein and that the upper fork of the independent discal veinlet is wanting.

In one of the male specimens only 4 of the spots on the border of the primaries are present, and in this specimen as well as in one other male and one female, the second discal spot is absent.

Described from 2 ♀♀ 5 ♂♂, collected in California by Mr. H. K. Morrison.

Easily distinguished from *P. yuccasella* by the smaller size, the spots

on primaries, the broader maxillary tentacles and shorter, blunter ovipositor; and by the male claspers being deeply excavated before the tooth which is greatly elongated.

PRODOXUS INTERMEDIUS n. sp. ♀.—Expanse 28.5 mm. Coloration as in *Pronuba yuccasella* and immaculate specimens of *Prodoxus decipiens*. Basal joint of the maxillary palpi not provided with a tentacle, but having a slight tubercle at the tip. Apical joint of the abdomen (fig. 11, *a*) shaped as in *Pronuba yuccasella*, but slightly deeper and possibly a little thicker. Ovipositor (fig. 11) shaped as in *Pronuba yuccasella*, but slightly stouter, more acuminate at tip and with the serrations of membrane finer and more numerous. The sculpture of basal joint shows like very fine punctations in regular rows.

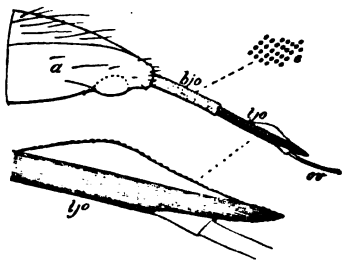


FIG. 11.—PRODOXUS INTERMEDIUS: *a*, anal abd. joint of female; *bjo*, basal jt. ovipositor; *s*, its sculpture; *tjo*, terminal jt. do.; *ov*, oviduct.

Described from 2 ♀ ♀ from Texas (Boll), and 1 ♀ from Ute Pass, Col., July 18, 1877 (Riley).

In the specimen from Ute Pass there is a short rounded process just before the apex of the abdomen on the under side. As this, together with the space just above it, is very thin I consider it to be the result of some injury, such as a pinch of the forceps, which the specimen may have suffered while fresh. (This flattened portion is indicated by a dotted line in the figure.)

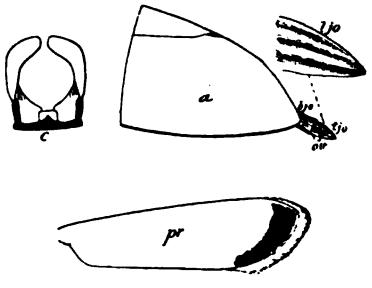


FIG. 12.—PRODOXUS MARGINATUS: *a*, anal abd. jt. of female; *bjo*, basal jt. ovipositor; *tjo*, terminal jt. do.; *ov*, oviduct; *c*, claspers of male from above; *pr*, front wing—hair-line showing nat. size.

PRODOXUS MARGINATUS n. sp.—Expanse 10 mm. Color white, the clothing of body very sparse, especially on the abdomen, and showing more or less distinctly the integument which is dark brown often with an æneous tinge. Antennæ bare towards tip and yellowish. Primaries (fig. 12, *pr*) satiny white with a terminal black band of varying width; under surface fuscous, almost black on costal and posterior borders; fringes white. Secondaries white with a broad costal and apical fuscous shade; under surface concolorous; fringes white. Apical abdominal joint of ♀ blackish, slender, very obliquely truncate above, the tip blunt-pointed (fig. 12, *a*). Ovipositor short and stout; the horny terminal joint gradually narrowed to the pointed tip, the upper edge very finely and acutely

serrate (fig. 12, *t j o*). Claspers of ♂ (fig. 12, *c*) not dentate, their form much concealed by scales, the large arms broad and of nearly uniform width; large upper basal piece obtusely angulated in the middle.

Described from 3 ♀♀ and 4 ♂♂ collected in California by Mr. H. K. Morrison.

PRODOXUS CINEREUS n. sp. ♂.—Expanse 12 mm. Head, thorax, legs and apical ventral joints whitish. Primaries ashy; secondaries and under surface of all the wings brown with a cinereous reflection. Tip of maxillary palpi and apical third of antennæ black; the integument of body black. Claspers (fig. 13, *a*, *b*,) dark testaceous, similar in form to those of *decipiens* but without teeth; large upper basal piece broadly rounded or subtruncate; the smaller piece beneath it of similar shape.

FIG. 13.—PRODOXUS CINEREUS: male claspers (*a*) from above; and (*b*) from side.

Described from 7 ♂♂, collected in California by Mr. H. K. Morrison.

PRODOXUS ÆNESCENS n. sp.—Expanse, ♀ 14.7 mm.; ♂ 11.2 mm.—14mm. General color bronzy, the primaries with a distinct purple reflection. Under surface of thorax, the coxæ and the femora clothed with white scales. Head whitish, with scattered black hairs; labial palpi with black hairs; 4th and 5th joints of maxillary palpi and the antennæ, except at the base, black. Integument black. Apical abdominal joint ♀ (fig. 14, *a*) swollen as in *P. decipiens* and obliquely truncate from above, but the tip is also truncate from beneath and the lower border is slightly excavated. Ovipositor short and stout, very broad; the upper

FIG. 14.—PRODOXUS ÆNESCENS: *a* anal abd. jt. of female with ovipositor retracted; *bjo*, basal jt. ovipositor; *tjo*, terminal jt. do.; *ov*, oviduct; *c*, claspers of male from above.

border of the horny terminal joint (fig. 14, *t j o*) thin, arched and finely serrate, the tip obliquely truncate beneath and at the base of the truncation forming a small, thin tooth, the base beneath forming a blunt tooth, the border between these teeth retuse. Claspers of ♂ (fig. 14, *c*) with no teeth on the arms beneath; the arms more slender than in *cinereus* and narrowed more abruptly near the base; the broad basal part with a small tooth at the apex within. Large upper basal piece forming a stout process at the apex.

Described from 3 ♀♀ and 8 ♂♂ collected in California by Mr. H. K. Morrison.

From the above descriptions it is evident that we already have represented, in the few species so far known, various gradations between the more typical ovipositor of *Pronuba yuccasella* on the one hand and that of *Prodoxus decipiens* on the other. It would also seem that the curious maxillary tentacle possesses simply generic and not Family value. *Prodoxus intermedius*, agreeing so closely with *Pronuba* in all respects except in lacking these tentacles, shows us how insects of different genera may become similarly modified in special parts by similarity of function of such parts; for we may with tolerable safety assume that *intermedius* breeds in the fruit of the plant and probably in the seed as does *yuccasella*, existing largely through the intervention of this last since it has itself no special means of pollinizing.[18] The male of this species is not positively known; but two specimens taken at Ute Pass, Col., on the same day with the female, and two others taken at the Monument Park the next day, differ from the ♂ of *Pronuba yuccasella* only in the somewhat greater expanse.

Hitherto rather than hastily found a family on a single species even where there are good structural characters not covered by former characterizations of the Family, I have been unwilling to separate *Pronuba* from the TINEIDÆ in the group TINEINA, with which family it has most affinities in its venation and well developed maxillary palpi. But with the addition of other species and another genus science will be best served by the establishment of a distinct family which I would call PRODOXIDÆ. The family characters will then be:

Imago with the head rough; labial palpi of moderate size and curved upward; maxillary palpi long, elbowed, 5-jointed, the basal joint either protruberant or modified into a prehensile tentacle; anal joint of female compressed from the sides, bare and horny; ovipositor extensile, the terminal joint horny, in one piece and adapted to piercing and sawing; egg very soft, elongate and flexile; larva

[18] The assertion of Dr. H. A. Hagen in a recent number of the *Canadian Entomologist* (July, 1880) that moths which he bred from the flower-stem of Yucca were typical specimens of *Pronuba yuccasella* and possessed its curious maxillary tentacles, might strengthen the idea that these organs were structures of dimorphic value only, but Dr. Hagen has since admitted to me that he was entirely wrong and that his moths were typical specimens of *Prodoxus decipiens*. I need only record the fact, without further comment. Indeed Dr. Hagen has made some beautiful studies of the anatomy of *decipiens* which strengthen and emphasize the differences between the two insects.

either without prolegs or entirely apodous; chrysalis with a strong capital thorn.

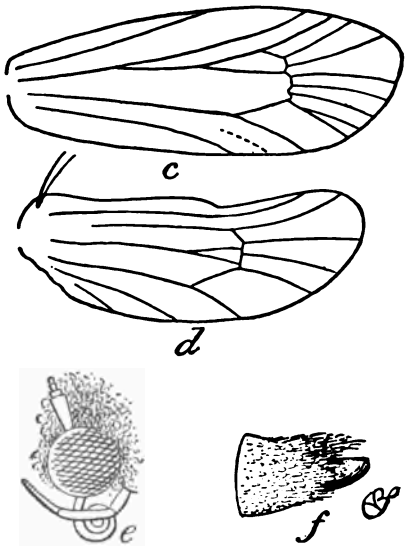


FIG. 15.—HYPONOMEUTA MALINELLA: *c*, *d*, venation of female front and hind wings; *e*, head with tongue, labial palpus and part of antenna; *f*, tip of abdomen.

From the HYPONOMEUTIDÆ with which it has strong general resemblances, the family is at once distinguished not only by the anal characteristics, but by the different venation and by the possession of maxillary palpi. In the typical *Hyponomeuta* the antennæ are more serrate, the head more finely and closely scaled, the palpial joints differently proportioned, the tip of abdomen does not extend beyond the scales, the front wings lack the forked disco-longitudinal vein and the internal vein is simple, not forked at base. The hind wing is usually notched on costa and shows the frenulum in the ♀ more distinctly, in two bristles. The accompanying figures will show (fig. 15) the pterognostic characters, trophi and anal abdominal joint of a ♀ *H. malinella* Zeller, a common European species feeding on Apple, and (fig. 16) the slightly varying venation of *H. multipunctella* Clem., one of our own species. The larval habits are also quite different. From the GELECIDÆ the PRODOXIDÆ are readily distinguished by the rougher head the presence of maxillary palpi, the wing characters and the larval characters and habits.

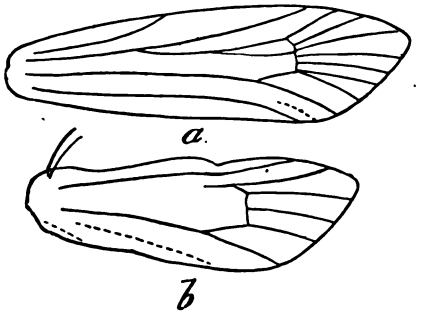


FIG. 16.—HYPONOMEUTA MULTIPUNCTELLA: *a*, *b*, venation of front and hind wings.

We have thus:

Family PRODOXIDÆ.

Genus PRONUBA.[19]

P. YUCCASELLA Riley, Trans. St. L. Ac. Sc., III (1872), p. 56. *Tegeticula alba*[20] Zeller, Verh. d. zool.-bot. Ges., XXIII, (1873), S. 232 (Sep. p. 32).

P. MACULATA[21] n. sp.

Genus PRODOXUS.

P. DECIPIENS Riley, American Entomologist, III (1880), p. 155. *Hyponomeuta 5-punctella*[22] Chambers, Canadian Entomologist, VII (1875), p. 7.

Hyponomeuta paradoxica Chambers (Journ. Cincinnati Soc. Nat. Hist., Oct., 1878, p. 9.

P. INTERMEDIUS n. sp.

P. MARGINATUS n. sp.

P. CINEREUS n. sp.

P. ÆNESCENS n. sp.

[19] This name seems to be preoccupied in Coleoptera, as I am informed by Dr. Geo. H. Horn that it is used by Thomson in his Classification of the Longicorns, 1860, p. 241. In this event Zeller's *Tegeticula* will take precedence.

[20] Zeller described from the ♂ only, but I have examined his type in the Cambridge Museum of Comp. Zool. and verified the fact that it is ♂ *P. yuccasella* and not *Prodoxus decipiens*.

[21] This is quite different from the supposed spotted specimens of *Pronuba yuccasella* which Mr. Chambers found in Colorado (See *American Entomologist* III, p. 143), all of which that are in existence and which I have carefully examined being spotted forms of *Prodoxus decipiens*. The maculation in these last, though variable, is on a different pattern.

[22] A recent reëxamination of the type in the Cambridge Mus. of Comp. Zool. leaves no question as to this being a 5-spotted form of *Prodoxus decipiens*. The figures, given in the Journal of the Cincinnati Soc. Nat. Hist., are somewhat inaccurate in detail and are turned completely around, so that what is given as the costa is the inner border. I would retain my own name for the immaculate form and relegate *5-punctella* as a variety name to the 5-punctate form which seems to most prevail in parts of Texas.

neglected meadows where the insect has opportunity to multiply unnoticed by man and undisturbed by cattle.

Connection of wet and dry Seasons with Army Worm Increase.—The past history of Army Worm years shows conclusively that dry seasons are favorable to the multiplication of the insect. Fitch's theory that such excessive multiplication occurred only during a wet season following the dry ones, required that the Spring of the present year should be a wet one, whereas, throughout the Atlantic States where the worm has been destructive, the exceptional drought has been proverbial. We must, therefore, believe that the Army Worm is most likely to appear in destructive numbers after dry seasons, regardless of the wetness of the season in which it does so appear; though moist cloudy weather, rather than hot dry weather, is without doubt favorable to the development of the species.

SOME RECENT PRACTICAL RESULTS OF THE COTTON WORM INQUIRY BY THE U. S. ENTOMOLOGICAL COMMISSION. By C. V. RILEY, of Washington, D. C.

THE Cotton Worm (*Aletia argillacea* Hübn.[1]) has already formed the subject of two papers read at recent meetings of the Association. A year ago I presented some of the facts and conclusions regarding the habits and natural history of the insect, that had up to that time grown out of the investigation begun by me three years ago under the Department of Agriculture and continued under the auspices of the U. S. Entomological Commission. It is my purpose in the present short paper to present a few of the more practical results obtained up to the present time.

PRINCIPLES ESTABLISHED.

While experience and methods must vary, there are certain underlying facts and principles which we have fully established, and which should everywhere guide, since they obtain all over the cotton belt. Among the more important of these that have a practical bearing are the following:

[1] *Anomis xylina*, Say.

1. The first worms appear in small numbers, much earlier than has been hitherto or is usually supposed, and generally in the same particular spots year after year, in hibernating centres or regions where the parent moth survives the winter; for this fact of survival in the imago state in the southern portion of the belt is now established upon substantial evidence.

2. These first worms appear much earlier than the so-called "first crop," which attracts the attention of the planter, and they may be looked for early in May, or even during the last days of April. While young their presence is most readily detected by the pale, translucent spots they make on the leaves, which spots are a sure indication either that the worm is present or that it has been; for these early worms are frequently swept entirely off by their enemies.

3. The eggs in early summer are laid on the underside of the leaves, and very uniformly on the older leaves about the middle or the lower third of the main stem, and the young worms feed there for a few days, producing the spots above described, before ascending to the more tender leaves.

4. The parent moths, while fond of all sorts of sweet exudations, the nectar from flowers and the juices of fruits, obtain their chief nourishment in early summer from the glands which occur on the underside of these leaves and on the involucre, and which exude a sweet liquid. They are able to suck up solid particles, and may be killed by poisoning the sweets upon which they feed.

5. The eggs of the Boll Worm are, also, mostly laid in similar situations and rarely on the squares or the bolls, and the young Boll Worms likewise feed upon the leaves for some days before entering the squares or the bolls.

From the above well-proved truths it follows that poisons applied to the under surface of the leaves will accomplish more good than when thrown on the upper surface, as is the common custom. They will thus more surely kill these young worms before these do any damage; they will tend to kill the moths, and they will likewise kill the young Boll Worms.

My efforts the present season have, therefore, been principally directed: 1. To discovering feasible means of thus poisoning; 2. To improving present methods by diminishing the quantity necessary to an acre and the labor necessary to apply it; 3. To the discovery of cheaper, safer and more satisfactory substances.

THE BEST POISONS.

The experience of the year has so far given us nothing superior to the substances previously tested. We have over five tons of extracts and decoctions of various native plants centered at Selma, Ala., made either by Professor R. W. Jones, of the University of Mississippi, or by Mr. James Roane of Georgetown, D. C., agents of the Commission. But two or three, so far, give any promise, and these not much. Yeast ferment or beer mash, which Dr. Hagen, of Cambridge, so strongly recommended, has proved entirely useless. Of the various arsenical poisons, Paris green still proves the best, so far as efficacy and harmlessness to the plant are concerned, but the use of this and of different preparations of white arsenic is to-day so well understood that they need no further mention.

LONDON PURPLE.

Of this arsenical refuse from the manufacture of aniline dyes, which I introduced a year ago with a good deal of hope as a cheap substitute for Paris green, it will be well, however, to say a few words. The testimony in regard to it is very generally favorable the present year, as I anticipated would be the case from the experiments we made a year ago. But some reports are less favorable, and such mostly come from parties who have not understood how properly to mix and use it. Pound for pound, it should be made to go twice as far as Paris green, *i. e.*, a pound of the purple is sufficient to eighty or even one hundred gallons of water, and if used dry should be in proportion of one to forty parts of the diluent.

It should be borne in mind that great care is necessary in mixing it in water to prevent its forming lumps, and that it acts more slowly than Paris green. To this last fact is due most of the unfavorable experience and judgment. If a rain follow too soon after an application, the purple kills comparatively few worms. Its good effects are fully seen only under favorable circumstances on the second or third day, while the green shows its good effects a few hours after application, and particularly the day following. In the early use of the green the same diversified experience was had, and from defective methods or adulterated material unfavorable results were quite frequent. One source of failure with both these materials in liquid is the lack of provision to keep them

stirred up and well suspended; another, in not bearing in mind that the poison has greater specific gravity than the water in which it is carried; so that in poisoning many rows at a time the finer spray falls on the furthermost rows with little or no poison.

London purple is exceedingly fine and sifts through the slightest crevice. This is an advantage to the planter who uses it on his cotton, but necessitates great care in shipping. The manufacturers have shipped it for the most part in barrels, which have permitted it to leak and stain other goods, as well as the vehicles of transport, thus doing more or less injury and prejudicing freight agents against it. This defect should be remedied. Some of the unfavorable results with this purple, I am constrained to believe, have resulted from adulteration. Experience would indicate that it is less dangerous to use than the green.

PYRETHRUM.

This powder, of which, since last year's experiments, I have had great hopes, fully warrants them. No other vegetable substance approaches it. Last year while it was found by Prof. E. W. Hilgard, of California, that an alcoholic extract of any part of the plant possessed the insecticide property, I had serious doubts whether it could ever be successfully used in the cotton field because of its cost. The simple powder, mixed with flour as a diluent, could then be made to go over more ground than the alcoholic extract. The present year we have found that a simple fluid extract, made after the ordinary formula of the pharmacopœia will go much farther, and that the extract from a pound kills all the young worms when diluted in 120 gallons of water! Nay, more, one of the most important discoveries is that it acts equally well or even better when simply mixed with water, and even one pound to 150 gallons is effective, and one pound to 200 gallons will cause the destruction of most young worms. Its action is really marvellous; but as it kills by contact, its effects are not lasting as in the case of arsenical poisons, which act through the stomach. It produces convulsions and paralysis; so that all young worms in contact with it soon writhe to the ground. Larger worms are less easily affected, but they soon writhe to the ground, from which they rarely recover, even if the pyrethrum fails in the end to kill; for once on the ground and enfeebled, a host of enemies are ever ready to finish the work begun by the powder. This insecticide

acts quite differently on different insects, but Aletia is one of the most susceptible to it. It acts equally as well in rainy weather, and I have not a doubt that when the plant is grown in this country so that the cost of the powder will be nominal, Pyrethrum will be extensively employed by planters, and to this end I have taken steps to have it introduced and cultivated. Its harmlessness to man, the small quantity necessary, and the fact that it may be grown by the planter himself, will offset the greater permanency of arsenical powders.

OILS.

Nothing is more deadly to the insect in all stages than kerosene or oils of any kind, and they are the only substance with which we may hope to destroy the eggs. In this connection the difficulty of diluting them from the fact that they do not mix with water has been solved by first combining them with milk, either fresh or spoiled, to form an emulsion, which is easily effected, while this in turn, like milk alone, may be diluted to any extent by water, the particles of oil being held homogeneously in suspension. Thus the question of applying oils in any desired dilution is settled, and something practicable from them may be looked for.

IMPROVED APPLIANCES.

Planters will apply poisons either in liquid or in powder according to circumstances and conveniences. The wet method, according to present practices, is the more expeditious, and the safer so far as injury to man and stock is concerned. It acts less favorably in wet weather, the first outlay in appliances is greater, and they are often useless where the soil is heavy and wet. The dry method can be most advantageously used in wet weather, and the application is most persistent: the cost of diluents has heretofore been great; there is more danger to the operator, and an acre is poisoned less quickly.

Important advances have been made the present year in both methods. The perforated sprinkling nozzles have given way wherever they have come into competition with those which spray by obstruction against an inclined plate or by pressing the water through a crescent-shaped slit. The simplest and cheapest nozzle, and that which least clogs, will give most satisfaction, for which reason one which I introduced the present summer, and which is

known as the Riley sprinkler, has been largely used. It is a simple, semicircular piece of metal, about three inches in diameter, with a narrow strip of the straight edge bent upward at an angle of about 45°, and the nozzle so adjusted that the jet strikes the angle of the bend at its middle.

Experiment shows that there is a limit to the subdivision of the liquid beyond which it cannot practically be carried, both on account of the greater tendency of the nozzle to clog and of the greater specific gravity of the poison compared to water in fine spray; so that in attempting to throw a fine spray over ten or twelve rows, the outer rows, as already stated, receive no poison. This last obstacle applies less to pyrethrum, which has least specific gravity. Nevertheless, experiment has demonstrated that forty gallons of poison may be successfully made to go over three acres of cotton instead of one, which is a great gain. In using the poisons dry, it does not seem possible to advantageously diminish the amount per acre by any present appliances, but I have reason to believe that a diluent of simple earth well dried and pulverized may be used with as much advantage as any more costly.

POISONING FROM BELOW.

Now the throwing of the poison from below will enable us to diminish much further the quantity to be thrown on the plant in either method, and one of the ideas originated and now being tested by the Commission is to throw the poison upon the plants by means of rotating brushes. This method promises full success and is probably the simplest and cheapest by which the principles mentioned in the beginning of this paper can be satisfactorily applied. It has these further advantages: the amount of the poison is easily regulated; it is uniformly applied; it remains longer and is not so easily washed off by rains, and finally, the plan does away with pumping and stirring. The brush is made to revolve in a box or trough, which is supplied with fluid poison on the principle used in automatic inkstands, or with powder from a hopper having slits from which the bristles brush out the powder. The spray may be produced by the centrifugal force from rapid rotation, in which case the box is only open on the side from which the spray is desired, while the slow turning of a very stiff brush against an edge will throw either a spray or a cloud of powder by the elasticity of the bristles as they spring out from

under the edge. It is useless to go into details, and we have not yet decided whether steel, whalebone, bristle or broom corn, or some other material will prove best. Prof. W. S. Barnard, of Ithaca, N. Y., is now working out the details of the gearing, and will continue to experiment with different devices and on different principles. He has already made some very important discoveries which will be duly recorded in the next report of the Commission on the subject. Another simple plan now being tried, for applying wet poison to both sides of the leaves, without involving machinery, is by a long, heavy fringe upon which the poison-fluid drips, and from which it is wiped off by the leaves as they are pulled through its pendent parts, which hang down among the branches while it is carried over the row. Another principle will be practically applied and worked out, if possible, by Mr. J. D. Cross, of Alabama. It is to throw the powder behind a cultivator by means of a revolving fan, worked by a spring to be wound up at intervals, so that the plant may be poisoned without extra labor when it is being for the last time cultivated or laid by. It is unnecessary to allude here to experiments that have given no practical results.

EARLY POISONING.

The importance of poisoning early, or when the very first appearance of the worm is manifest, cannot be too strongly insisted on. Wherever it has been done early and judiciously the crop has been saved, and the contrast between the poisoned fields and those not poisoned is most marked. The latter have been defoliated over a month, and will yield, on an average, but one-third as much cotton as the former, and that of an inferior quality.

WEATHER: CONCLUSION.

The weather throughout most of the cotton belt has been unfavorably wet, and, indeed, the persistent rains have greatly interfered with the experiments and the work of the Commission. Yet we have reached a stage in the management of the worm when there is no longer any excuse for loss by its ravages, and with the results of this year's labors of the Commission, I feel that I have measurably accomplished what I set out to do three years ago, viz.: to get thorough and accurate knowledge of the habits of the principal insects injuriously affecting the cotton plant — a knowledge

which did not then exist — and to learn best how to control them. The Caterpillar and the Boll-worm, the two worst of these enemies, will soon cease to be a cause of anxiety to intelligent and enterprising planters. How best to overcome in this connection the negligence and indisposition of the more careless and ignorant of the cultivators, of whom there are so many among the freedmen, is a question which I may consider at some future time.

THE HITHERTO UNKNOWN LIFE-HABITS OF TWO GENERA OF BEE-FLIES (*Bombyliidæ*) By C. V. RILEY, of Washington, D. C.

[ABSTRACT.]

THE paper gives the life-history of *Systœchus oreas* O. S. and of *Triodites mus* O. S. and shows that their larvæ have the same habit of preying on locust eggs. It calls attention to the parallelism in the life-history of the Bee-flies and of the Blister-beetles. The Bombyliidæ (accepting the more recent expansion of the family), so far as their habits have been hitherto recorded, were known to prey parasitically in the larva state upon the larva either of burrowing bees (genera *Anthophora*, *Andrena*, *Halictus*, *Colletes*, etc.), of mud-daubing wasps (*Trypoxylon*, etc.), or on the pupæ of certain Lepidoptera (*e. g.*, *Limacodes*). Yet certain genera develop in the egg-masses of the Acrididæ, feeding upon the eggs. So the habit of the Meloïdæ, so far as known up to the year 1877, was to live parasitically in the cells of either burrowing bees or mason bees, though certain genera, *e. g.*: *Epicauta* and *Macrobasis*, were then shown to prey on locust eggs. (See Proc. of the Association for 1878, B., p. 18.) The abundance of both the Bombyliidæ and Meloïdæ in the western country is referred to as directly connected with the prevalence of locusts there, and the facts of retarded development in the early stages of both families are recorded and explained as a characteristic beneficial to the species which must depend on such uncertain food as the eggs of insects like the migratory locusts, which in some years prevail in great abundance and in others become scarce or are not found at all, in given localities.

On Lightning Bugs. By John L. Leconte, of Philadelphia, Pa.

Since the publication of my synopsis of Lampyridæ in 1851 (Proc. Acad. Nat. Sc., Phila, 1851, 331), but few species of the family have been described in this country, and no very important improvement has been made in their classification: about the same time I published in the Journal of the same Society (New Ser., i, 73) a synopsis of Lycidæ, one of the sub-families of Lampyridæ. This last mentioned synopsis is one of my early and crude contributions to science, which, if the study of Natural History had been farther advanced in this country, would have been kindly suppressed, or returned to me for revision. In the Classification of the Coleoptera of N. A., I have established the family with different limits than those adopted by European writers at that time (1851), and constituted it of the three following sub-families, which are here more fully defined:

Middle coxæ distant, epipleuræ wanting . . . . . Lycidæ.

Middle coxæ contiguous, epipleuræ usually wide at base, epistoma of metathorax with inner margin sinuate . . . Lampyridæ.

Middle coxæ contiguous, epipleuræ narrow at base, epistoma of metathorax not sinuate on inner margin . . . . . Telephoridæ.

A detail of the minor groups and tribes composing these families would be here out of place, and may be found in my Classification; they will be fully exposed in a synopsis of the genera and species now ready for press. Otherwise, the habits and life history of a few species have been more or less thoroughly observed and recorded.

For the past few years I have been trying to procure material to enable me to make a more complete synopsis of the genera and species, and a better exposition of their affinities to each other than I have been able to give.

For [illegible] specimens of larvæ, pupæ and imagines of species from [illegible] am under obligations to Mrs. V. O. King, of Austin, Texas. An account of the transformations of [illegible] has been published in Psyche, [illegible] descriptions of larvæ of other species [illegible].

* [illegible] genera [illegible] the [illegible] of the genus [illegible] to the [illegible]

I congratulate myself, that by the slow progress of my studies and the tardy manner in which some of my correspondents have replied to my request for larger series of specimens, I am now able to profit by the recent publications of Mr. C. O. Waterhouse[2] on Lycidæ, and Rev. H. S. Gorham[3] on Lampyridæ.

The object of the present essay is simply to give some popular information in regard to the characters of these insects, and to correlate, so far as our species may permit, the light-giving faculty with other structures; I will, therefore, not enter here into a close analysis of the relations of the genera.

It is then sufficient to say, that none of the Lycidæ or Telephoridæ possess any light-giving organs, and that they are diurnal in their habits. In some of the Lycidæ the front part of the head is prolonged into a beak, and in many of them the elytra are very large, expanded and coarsely reticulated. The peculiar structures of some Telephoridæ will be noticed farther on.

The Lampyridæ proper comprise all the luminous species, though this faculty is possessed by them in a very unequal degree, and in some genera and species of diurnal habits is quite wanting. For our present purposes their division may be indicated into tribes and groups as follows:

A. Side pieces of Metathorax narrow;

a. ♂ and ♀ similar or nearly so; antennæ long, last joint simple.

b. ♂ and ♀ conspicuously different; antennæ short, last joint with acicular appendage.

B. Side pieces of metathorax wide (♀ unknown); palpi very unequal, mouth organs more developed.

The series A *a* contains the largest number of genera and species, and exhibits a gradation from *Matheteus*, with widely separated pectinate antennæ, and general Lyciform appearance, through *Photinus*, with approximate filiform antennæ, and head retracted under the prothorax, to *Photuris*, with the antennæ filiform, and the head partly exposed. There is thus a continuous line of affinities in this series from the diurnal Lycidæ to the diurnal Telephoridæ.

Now besides the gradations in structure just mentioned there are great differences in the sizes of the eyes, and in the develop-

[2] Br. Mus. Cat. Illustrations; Coleoptera, Part 1, Lycidæ, 1879.
[3] Trans. Ent. Soc. London, 1880, p. 1, 63, 83, and Proc. loc. cit. infra.

ment of the light organs. In the species usually seen flying by day (*Lucidota*, *Ellychnia*, etc.) the light organs are indicated by feeble yellowish spots on the last ventral segments, but do not seem to possess any light-giving power; in these the eyes are lateral, rather small in ♀, but larger and more convex in ♂; they are widely separated above and beneath as in Lycidæ.

The series A *b* contains a much smaller number of genera, and in them the antennæ are approximate, usually filiform, rarely (*Pleotomus*) bipectinate. The number of joints varies from 9 to 14, and the last joint has at the end a small acicular appendage simulating an additional joint. The eyes of the ♂ are excessively large, almost contiguous above and beneath, leaving very little room for the mouth and antennæ; in the ♀ the eyes are moderate, or even small, lateral and widely separated. The light organs are varied, sometimes brilliant in both sexes, sometimes weak in both sexes, and sometimes brilliant only in the ♀. Their food consists of small terrestrial mollusca.

The third category, B, consists of *Phengodes* and allied genera in which the side pieces of the metathorax are wide. I know nothing by observation of the luminous qualities of these insects, of which only males are known. They are all rare, and I have seen none alive. But we here owe our special thanks to Mrs. King for the patient quest which has been rewarded by the discovery of the pupa of *Phengodes*, and which will probably result in another season in the detection of the larva.[4]

I am also indebted to Mrs. King for a larva of *Mastinocerus*, of slender, cylindrical form and pale color. It was feebly luminous, and lived upon small snails. The perfect insect is thus mentioned in a letter, the observations being made upon a specimen attracted by the lamp: "June 4th saw running rapidly over the table near a lighted lamp, a small Coleopter; it was twisting its abdomen up over its wings, and evidently trying to straighten them out, as they seemed moist and twisted at their ends. The general appearance suggested *Mastinocerus*, and acting on this thought, I captured it and sat up till a late hour to be assured of the truth. The

[4] It is still uncertain whether the large luminous larvæ described by Baron Osten Sacken (Proc. Ent. Soc. Phila., i, 125, pl. 1, f. 8) belong to the Elateride *Melanactes* or to *Phengodes*. Species of both genera are found in nearly all parts of the United States, but though the larvæ seem to resemble that of *Mastinocerus* referred to in the text more than any described Elateride larvæ, it is more probable that they should be referred to the latter family.

insect was in a small vial, and moved quickly. It gave out light conspicuously from the head, feebly from the anal end, and still more so from about the base of the abdomen. The light seen in the head, though visible in the dark as a round spot, yet when taken into a room obscurely lighted was invisible from above; but when the insect was suddenly thrown upon its back a light no larger than a pin point was seen just about the junction of the head and prothorax." . . .

It is quite possible that the genera of the other continent constituting the sub-family Drilidæ should be placed as a group of this series; but upon this point I cannot speak with confidence, as I have had no opportunity of studying them carefully. They live on snails, and some of them pass through their transformations in the shells of the animals upon which they have fed.

As regards the third sub-family, Telephoridæ, it is merely necessary to mention that the eyes, without being excessively large in either sex, are invariably larger in the ♂ than in the ♀, and that in the lower forms (*Malthodes*, etc.) there is an extreme complication in the development of the last abdominal segments.

We have, then, in Lycidæ a tendency, with simple sexual characters, to elongation of the anterior part of the head. In the Lampyridæ the sexual characters are diffused over the whole body, but with no tendency to elongation of the head or complication of the posterior abdominal segments; and in addition there is a peculiar apparatus for the emission of light, which although absent in some genera, does not exist at all in the other two sub-families.

Finally, in Telephoridæ there is a slight reminiscence of the anterior extension of the head in certain species of *Podabrus*, which have a broad muzzle. In general the mouth organs are more powerful than in the other sub-families. The sexual characters are of an ordinary kind, but in *Chauliognathus* and *Malthodes* the last abdominal segments of the ♂ are largely developed. In some species of the last named genus the complication of these rings resembles nothing that I know in nature, except the curious structures of Tipulidæ figured by Osten Sacken.[5] In *Ichthyurus*, an Asiatic genus, the middle legs of the ♂ are singularly inflated; and in *Silis* there are curious processes near the hind angles of the prothorax; a very deep fissure limited on each side by a prolongation, and complicated by a movable articulated process

[5] Proc. Acad. Nat. Sc. Phila., 1859, 197, pl. 3 and 4.

attached to the inflexed flank of the prothorax. In several species this articulated process terminates in a long bent filament, and the apparatus probably serves like a somewhat similar one on the first antennal joint of the ♂ of the Malachide *Collops*, to clasp the antennæ of the ♀.

After this statement, which is as brief as I can make it, of the arrangement into sub-families and tribes of the Lampyridæ, with the principal modifications of structure in each, we are prepared to consider the variations in the light organs, and their sexual correlation with the eyes and wings in the Lampyridæ proper.

We have seen that the greatest development of the eyes takes place in the male of the series A *b*, or Lampyrini, in which the antennæ are very short in both sexes. The female is without wings, and has the eyes moderate (*Phausis*), or very small (*Microphotus*). The light organs are either brilliant in both sexes (*Phausis reticulata*), wanting in the male (*P. inaccensa*, female unknown), feeble in male and brilliant in female (*Pleotomus*, and probably *Microphotus*). In the Photini the light organs are completely wanting (*Tenaspis*, n. g.), obsolete and ineffective (*Lucidota*, *Ellychnia* most species); well developed in both sexes, but more brilliant in male than female (*Pyractomena*, *Photinus*); equally brilliant in both sexes (*Photuris*): in all these the antennæ are long, either slender or broad, and closely approximate; the eyes are widely separated on the upper side, and usually also beneath. In *Matheteus* and *Polyclasis*, the antennæ are pectinate, or bipectinate, and rather widely separated; the eyes are more distant, and the light organs wanting.

The Phengodini are known only by the male. The eyes are lateral, convex, moderate in size, and widely separated; the antennæ are distant at their insertion, plumose in *Phengodes* and *Zarhipis* (n. g.); bipectinate in *Mastinocerus* and *Cenophengus* (n. g.); pectinate in *Pterotus*, and serrate in *Tytthonyx*, if I am correct in associating that genus with this tribe. *Phengodes* is said by Lacordaire[6] to be luminous, while the observations of Mrs. King above cited prove that *Mastinocerus* is also phosphorescent.

From this detailed statement it may be inferred that there is no distinct correlation between the eyes, the antennæ, and the light organs of the two sexes which obtains for the whole sub-family.

That the eyes of the male should, in comparison with the other

[6] Gen. Col., iv, 345.

organs of special sense, the antennæ, be more largely developed than in the female, is explicable from the more generally active disposition of that sex, but that these characters should prevail in the contradictory categories, where the female is more brilliant, and where she is less brilliant, than the male, does not seem to me explicable either on grounds of teleology or natural selection, and especially do these explanations seem imperfect when we consider that the largest eyes are possessed by those males which seek the most brilliant, but also the most helpless females.

The luminous powers of these insects suggest three distinct investigations, which seem to me very important, and to which I would earnestly invite the attention of my colleagues in other branches of science:

1st. Spectroscopic examination of the nature of the light, and an analysis of the elements concerned in producing such brilliance at low temperatures.[7]

2nd. Biological observation of the particular arrangement of cells, which enables such an amount of light of a high grade to be produced by the metamorphosis of ordinary tissues, without the heat of incandescence.[8]

3rd. Chemical analysis to determine the nature of the proximate principles which are thus oxidized in the production of light without heat.[9]

None of these researches can be conveniently made in the northern states, but in the middle and southern states, and especially in tropical America, they can be readily entered into, and none of them fall properly within the sphere of investigation of entomologists.

[7] Some slight efforts have been made in this line of investigation, but nothing satisfactory has been published. Mr. Meldola (Proc. Ent. Soc. London, 1880, p. iii), observes: "that the exact nature of the phosphorescence was still an unsolved problem both to the physicist and biologist. Some years ago he had examined the spectrum of the glow-worm, and found that it was continuous, being rich in blue and green rays, and comparatively poor in red and yellow."

[8] In regard to the structure of the light-giving organs, I have found only some superficial notices by authors whom it is scarcely necessary to cite on the present occasion. It is narrated that they are rich in fat cells, and abundantly supplied with nerves and air-tubes; the histological observations of Schultze do not show the manner in which the light is produced.

[9] As to the composition of the fats contained in these special organs, I believe that no investigation has been made except the few unsatisfactory ones mentioned in my Synopsis of Lampyridæ of the U. S. (Trans. Am. Ent. Soc. 1881, p. 15), now passing through the press, though in this instance, as in the cases of the acid secretion of *Harpalus* the liquid explosively emitted by *Brachinus*, and the singular excretion *Chlaenius*, which combines the odor of camphor and creosote, the materials are easily procured, and the results would be physiologically important.

It is unnecessary for me to expand on the scientific value of the investigations here proposed. Luminosity of a much lower grade than that exhibited by these insects has been generally ascribed by physicists to matter at very high temperatures, even in the cases of auroræ and nebulæ; and in fact the experiments of Mr. Crookes upon radiant matter under the influence of electrical currents would tend to confirm such a supposition.

Should, however, the investigations I have suggested justify the belief, as seems probable, that large evolution of highly refrangible light may take place without high temperature, our cosmical theories may need to be partially modified. And should the chemist ascertain with accuracy what precise combination of molecules of the ordinary constituents of organic bodies will thus by slow oxidation give such a disproportionate amount of light, we may enter upon a path which leads to the accomplishment of one of the great desires of civilization, the production of light, without undue expenditure of energy in the development of heat, as a waste product.

In regard to the manner in which the light is evolved, I have but little information to give, though what I say may serve to correct some erroneous views which have been elsewhere expressed.

The popular name of these insects, *lightening* (or lightning) *bugs*, as distinguished from *fire flies*,[10] is derived from the fact that the light is intermittent. It is never entirely extinguished, but is paroxysmally weak and brilliant. When seized, under the influence of fear, the intervals become irregular, and the flashes are frequent. When put into alcohol there is at first a fitful and rapid exhibition of light, but afterwards the light becomes moderate and permanent for some minutes. When the light organs are separated and crushed, the light also continues for some time, becoming gradually weaker.

I therefore infer that the excitement of the light-giving organs to the highest degree of activity is manifested by the supreme effort of the will of the animal, and that the exhibition of light is dependent upon a particular structure, more or less under the control

[10] Fire flies are Elateridæ of the genus *Pyrophorus* (Spanish *Cucuyo*), of which one of the smaller species, *P. physoderus*, occurs in Florida and Texas, and shine with a constant light, chiefly visible in two vesicles near the hind angles of the prothorax, which are convex, and covered with a transparent chitinous integument. All species of *Pyrophorus* are not phosphorescent.

of the will, but containing a special material capable of evolving the light independent of the will. In a similar way to that in which the voluntary muscles of all animals evolve motion, and the special electrical organs of certain fishes evolve electrical currents, so do these organs of the Lampyridæ evolve light from some peculiarly constituted structure fitted for the purpose.

It may be conceded, after what has been said above, that the idea of Mr. Gorham,[11] that "the external white vitreous parts are diaphanous, and permit the light to shine through," is perfectly correct, but that his belief that "the source of light within the body of the insect can be pressed against these windows, or retired from them at its pleasure," is quite without foundation.

Several authors have remarked upon the tendency of the winged Lampyridæ, in countries where large numbers of the same species are in view at one time, to exhibit their light at rhythmic intervals. I have never observed this in the United States, nor in those parts of tropical America which I have visited. I think that, in this respect, there must be great differences in the habits of the species. In travelling by night on railroads in the middle states, I have frequently seen in low and moist lands near the road many thousands of these insects (chiefly *Photuris pensylvanica*), which sparkled and twinkled to such an extent that one might imagine himself on the Glittering Heath[12] itself. But I have never seen any approach to a rhythmic effect in these sparklings, as described in the books.

[11] Trans. Ent. Soc. London, 1880, p. 66.

[12] But lo! at the last a glimmer, and a light from the West there came,
And another and another, like points of far off flame;
And they grew and brightened and gathered, and whiles together they ran
Like the moon-wake over the waters, and whiles they were scant and wan.
—Sigurd the Volsung, Book II, p. 137.

[13] Proc. Ent. Soc. London, 1880, p. ii, Mr. McLachlan . . "had at that time advanced the opinion that the phenomenon in question might be caused by currents of air inducing the insects to simultaneously change their direction of flight." Sir Sidney Saunders: "The simultaneous character of these corruscations among vast swarms would seem to depend upon an intuitive impulse to emit their light at certain intervals as a protective influence, which intervals became assimilated to each other by imitative emulation."

Ibid. p. vii: "Mr. McLachlan, in connection with his idea of the supposed contemporaneous flashing of all the individuals in a swarm of Lampyridæ, called attention to flies of the genus *Argyra*, which when flying exhibited at times an appearance similar to that of small snowflakes, owing to the silvery pubescence with which part of the body was clothed, but which was observed in certain positions, and especially when the

The causes to which this singular phenomenon is ascribed are either physical or physiological.[13] In order that my readers may give these views due weight, I have cited them at some length in the foot-note. My own impression is that Mr. Belt and Sir Sidney Saunders have given, between them, the true explanation of the rhythmic exhibition of light, and apart from the æsthetic realization in nature of this plan of making night glorious by the wonderful brilliancy of such insignificant objects (upon which idea this is neither the time nor the place to discourse), it is primarily a defence of the insects against danger, and is secondarily caused by that tendency to act in concert or imitation which operates upon all sentient beings. This tendency may be equally observed in a flock of sheep following its leader, a school room of hysterical girls, a political meeting, a spiritistic séance, or a hyper-sentimental religious assemblage. And I regard all these occurrences, however differing in the importance of their final results, as individual instances in a large class of similar phenomena, caused by aggregated sympathy.

I would therefore agree with Sir Sidney Saunders and Mr. Meldola in quite rejecting Mr. McLachlan's view that it is produced by a change in position of the insects caused by currents of air, or even voluntary movements in direction of flight.

To recur to the process by which the light is produced, I would add to what I have said in the beginning of this essay, that the chemical processes possible in the bodies of Lampyridæ can be scarcely if at all different from those which take place in neighbor-

insect rested, owing to the pubescence being then concealed; he thought this to some extent was an analogous case to that of the light of swarms of Lampyridæ." Sir Sidney Saunders observed: . . . "as to the contemporaneous flashes of myriads, such as are more frequently congregated on the calmest nights, surrounding objects, previously involved in obscurity, become suddenly illuminated as if by electricity, and as rapidly plunged into their antecedent gloom at alternate intervals. He could not concur in the hypothesis that currents of air had any connection with such displays or exhibitions, when not a breath was stirring around; nor that these manifestations might be evoked by sexual influences, amid vast hosts instigated to combine therein, and act in unison. He would rather attribute this phenomenon to an inherent tendency to emit their light from time to time, requiring a certain amount of repose to recruit their powers; and when any thus surcharged felt intuitively inspired to take the initiative, the others— prompted to obey a corresponding influence — followed such suggestion in responsive sequence." Ibid, p. viii. — "Mr. Meldola stated that Mr. Thomas Belt (Naturalist in Nicaragua, p. 320) had expressed his belief that the luminosity of the Lampyridæ played the same part as the bright colors of many caterpillars, *i. e.*, that it served as a danger signal, warning nocturnal foes of the inedibility of the species of this family, which he had found to be generally distasteful to birds, etc."

ing and closely allied tribes. We may therefore infer from the observations of Mr. Meldola that the ordinary metamorphoses of tissues, by the aid of some slight modification of composition and cellular structure, are capable of evolving light, which belongs to the upper end of the spectrum, such as is generally significant of the highest temperatures.

It is therefore the more extraordinary to find in these insects light of a high order not dependent on elevation of temperature, and consequent waste of energy in heat. For it must be observed, that while in one sense heat is the cause of all the phenomena we perceive, since they all have existence only within certain ranges of temperature, in another sense heat is frequently a waste product, and the only one by which the dissemination of energy occurs so as to become imperceptible.

I am aware that the sketch I have here given of the present condition of our knowledge of Lampyridæ has been written to the demonstration of my own ignorance (and that of all other students) in regard to some of the most important questions involved. But if I succeed in causing you to look upon these remarkable insects with more interest than you have previously felt, neither my time in preparing, nor your time in listening to this essay, will have been wasted.

I would especially invite the attention of the younger observers in entomology, who have to pass through the labor of patient field work and close observation of habits, before they can ascend with profit to the higher retirement of the museum and the library, to contribute more fully to our knowledge of the development and habits of the different species.

I would equally ask the attention of my colleagues, who by long training in refined experimental research are qualified for such investigations, to the solution of the physical and chemical problems suggested by the singular production of high light without heat by these animals, which are within such easy reach. And by the solution of these problems I am convinced that our knowledge of molecular physics will be increased, and our powers of theorizing less fettered by conventional ideas.

The Endocranium and Maxillary Suspensorium of the Bee.

By George Macloskie, of Princeton, N. J.

The apodemes formed by an insect's crust may be either foldings outwards (exodemes), or involutions extending inwards (endodemes), or modifications of the chitinous lining of the alimentary tract (splachnodemes). Such processes are always by their nature double (including the outgoing and returning plates), and in exodemes (as the wing of a bee or the pleura of a lobster), this double nature is maintained by interposed nutritive matter (like the meat in a sandwich). In the internal foldings, however, as in the endoskeleton of insects, there is usually a complete fusion of the compound parts.

The endocranium, or endoskeleton of the head, consists of ridges, pillars, and plates, variously disposed in different orders of insects, yet adhering to a plan. Its central part when appearing as a plate is usually termed *tentorium*, from a fancied resemblance to the structure so named in mammals. The necessity of describing and comparing together these parts in the different groups of insects will be apparent, if we reflect that all attempts to evolve the embryology of an insect's head presuppose a knowledge of the adult structure, inside as well as external.

Fig. 1.

The upper part of a bee's cranium consists of three pieces, epicranium (EC), clypeus (C), and labrum (LR). (See fig. 1). The epicranium, or crown, extends from the occipital foramen, up the back of the head and over the vertex, to a transverse suture in front of the antennæ. It covers the whole roof and back of the head, and is medially separated in many insects (especially larvæ) into right and left divisions. It is flanked on the sides by the large compound eyes (OC); it bears at its vertex the three small eyelets; near its front margin it gives insertion to the antennæ (AT); and it is continued forward on the sides of the head by the genæ (G). The epicranium has no apodemes. It may have a few ridges on parts subject to strains, and it has a rim around the large eyes, and thickenings for the insertion of the antennæ; but there are no such ridges as to suggest segmentation in this region. This agrees with Balfour's conclusion (*Comparative Embryology*, chap. xvii) that the procephalic region does not correspond with the pos-

terior parts, and that "the antennæ can hardly be considered to have the same morphological value as the succeeding appendages."

The clypeus or "face" affords insertion at its lateral borders to the mandibular condyles, whose cup belongs rather to it than (as usually stated) to the genæ. It is strengthened by ridges so as to bear the strain of these appendages.[1] The hind border of the clypeus bends down into a strong transverse ridge whose extremities at both sides give insertion to the tips of two stout endocranial pillars (MC in fig. 2). These pillars (which we shall term the *mesocephalic* pillars) descend obliquely downward through the cranial cavity, and have their lower insertion in the floor of the

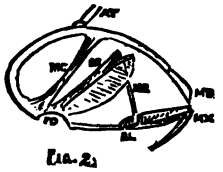


Fig. 2.

Fig. 3.

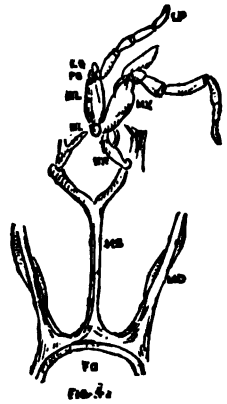


Fig. 4.

skull, just in front of the occipital foramen. They are shown (MC) in figures 2 and 3, and also in the ant in figure 4.

These pillars arise by strong roots from the most rigid part of the cranial floor, and give support above to the clypeus which has to bear the mandibles. They also afford attachment to muscles and serve to limit the delicate organs of the head. Hence a bee's head is strong though its shell is rather thin. Near the top each pillar divides into two branches, so as to widen the surface of insertion. In the ant's neck I observed stout tendons so placed as to antagonize their downward thrust.[2]

[1] Beetles usually have the mandibular cup in a process descending on each side from the clypeus: but in Catalpa the clypeus is turned under and has the cups on its angles.

[2] Burmeister speaks of "a broad ridge from the lower margin of the occipital aperture, which is prolonged towards the frons in two points." He seems to have broken off the pillars and to have observed only their inferior fragments. By the kindness of Dr. Hagen my attention has been called to Dr. O. J. B. Wolff's elaborate essay and

These large pillars ascend in front of the cerebral brain-lobes, in the fossæ between the cerebral mass and the ophthalmic lobes. They thus pass between the ophthalmic and the antennal nerves; and Dr. Wolff has shown that near their base they give insertion to muscles which supply the antennæ. The eyes, being very large in this group of insects, pass forward so as to outflank the insertion of the antennæ and these pillars.

Burmeister assigns no endocranium to Diptera, Hemiptera, and Lepidoptera. He states that in the first two of these orders "the head is a mere horny bladder without any internal processes" (*Manual of Entomology*, Shuckard's Transl., p. 232). This may be true as to Muscidæ; but I have elsewhere tried to show that the basal segment of the proboscis of Muscidæ represents the endocranium, then become an evertible pedestal for the proper mouthparts.[3] There is also in them a small arch thrown across the foramen magnum, corresponding to the *jugum* of the bee (as named by Wolff). In other Diptera, as the Gadfly and Mosquito, I find a pair of mesocephalic pillars, much as in the bee (in addition to a strong pharynx-case or splachnodeme, supporting the complex oral armature). As to Hemiptera, Burmeister's statement is only partially correct. The Squash bug (Coreus) has a pair of processes depending from the clypeus in the style of the upper part of the mesocephalic pillars, and Cicada has the pillars flattened out and attached to the sides of the head (the small eyes here not encroaching on their province), thus explaining the nature of the mesocephalic pillars as endodemes or involutions of the side wall of the cranium.

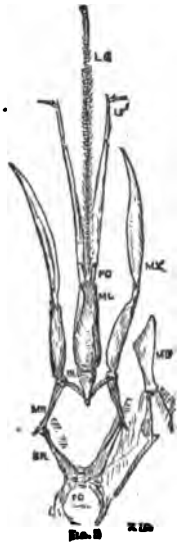
Lepidoptera likewise have a representative of these structures, in a strong sub-quadrate frame arising in front of the occipital foramen, and reaching forwards to near the roots of the proboscis. The Dragon fly has a ridge before the occipital foramen, sending up processes to the clypeal region, but these processes are broadened out and very thin, as in the weak and smaller clypeus.

The maxillæ and labium of insects are so closely connected in

drawings of the bee's head in *Nova acta Leop. carol.*, xxxviii, 1. While in many points he has anticipated and advanced beyond what I have attempted, he has I think failed in some part to catch the meaning of what he describes. This seems due to a double defect, of failing to follow up the variations of parts in other groups of insects, and of supposing that he ought to reproduce the bones of the vertebrate skull in that of the insect. This last fault is fatal to the reception of his terminology, as where he terms the mesocephalic pillars "alisphenoids."

[3] American Naturalist, March, 1879.

their mode of attachment that we must consider them together. In the bee they are slung upon a long retractile framework with elbows and hinges. I shall call this framework the "maxillary suspensorium;" all the published drawings of it that I have seen are grossly inaccurate, save that of Dr. Wolff, and even he seems to have missed some of the most interesting points. The basal part, or basicranial rods (BR in fig. 5), are inserted close to the roots of the mesocephalic pillars, in front of the occipital foramen. The rods run forwards towards the oral opening, the basicranial wall being excavated so as to allow them full play. They slightly ascend when the mouthparts are retracted (BR in fig. 2), and are nearly horizontal when the proboscis is extended. They are united to the rim of the excavated part of the basis cranii by a web (just as the mesocephalic pillars of Cicada are joined to the sidewall). They are rigidly inserted at their roots, and are slightly elastic above, while their motion is limited by the web.[4] (The two basicranial rods are bound to each other by a still more delicate web which forms the limiting wall of the head at the excavated part, and yet permits the play of the suspensorial mechanism.)



The basicranial rods are forked at their distal extremities, where they bear a pair of *maxillary rami* (MR in fig. 5). These are hinged to them by a very efficient elbow-joint, enabling the rami to fold downwards, and so to protrude or retract the maxillæ which the rami support. It seems to me that these rami correspond to the cardo pieces of the maxillæ of other insects (as CA in fig. 6). The name "cardo" has been given (by Kirby) to what I term the *labial rami* (BL, fig. 5), which do not support the maxillæ and are scarcely represented in the ant (fig. 4). It is safer to designate all these structures by terms which do not involve a theory.

The *modus operandi* of the maxillæ on their rami is noteworthy. Each maxilla has a flat stipe surmounted by a lacinia resembling a knifeblade, and bearing (in the honey bee a rudimentary) palp at the waist. The blade can bend downwards and

[4] An engineering friend on seeing this remarked, that it involved the principle of a machine lately patented for producing a limited and steady movement.

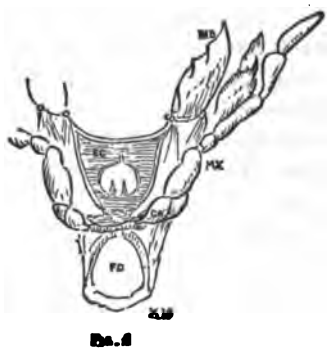
backwards on the stipe (see fig. 2) so as to be out of the way and to let the stipe project as a flat plate. When the suspensorium is retracted the two maxillæ are thus bent down, and their platelike stipes are approximated, furnishing a hard underlip for the mouth, upon which the mandibles play in their operations (as in cutting a piece of cork, or when the carpenter-bee operates on wood). The delicate limiting membrane which extends from the maxillary rami to the maxillæ is stretched when the suspensorium is protruded and thus serves to divaricate and fix the maxillæ.

The labial rami join the distal end of the maxillary rami with the labium, thus giving an additional joint, and a hinge which can move freely backward and forward, the result of which is great play to the parts of the labium. The labium consists of a basal piece, usually termed submentum, and of a middle piece, usually called mentum. It is safer to refer to these parts respectively as the basilabium and medilabium. The distilabium may indicate collectively the parts known as paraglossæ, labial palps, and ligule (PG, LP, and LG of figs. 4 and 5).

Many of the allies of the bee have these parts so simplified as to afford instructive hints as to their nature and relations. In *Stizus grandis* (with proboscis scarcely retractile) the basicranial rods are merely high ledges upon the margins of the excavated basicranium, and serving for insertion of the maxillæ. The ant (fig. 4) has only one basi-cranial rod, as if the two sides of the excavation had approximated and coalesced into a regular endodeme. Here the maxillary rami are nearly as in the bee, but the labial rami are almost obsolete, and the distalabial parts are much condensed.

The series of gradations thus presented gives promise of further discovery, and in attempting to correlate the parts of insects of different types I have fallen on some interesting revelations. It is at this part that Huxley's otherwise excellent description of the cockroach (*Anatomy of the Invertebrated Animals*, chap. vii,) is specially defective. He states that its endocranium "extends as a cruciform partition from the inner face of the lateral walls of the cranium to the sides of the occipital foramen," and adds that the centre of the cross is pierced by a rounded aperture through which the circumœsophageal nerve-collars pass. Fig. 6 will show how widely it differs from a cruciform pattern: and a comparison of this with figs. 3 and 4 will enable us to interpret its structure. Its lateral margins answer to the endocranial pillars of

the bee. Its anterior margin is the transverse ligament which usually binds together the roots of the mandibles, and prevents their divarication. These parts are webbed so as to form a flat (or broadly grooved plate) excepting at the centre where a passage is required for the nerves. In the Locust the lateral pillars approximate more closely so as to resemble the letter x, and so as to confirm the interpretation here offered. In describing the maxillary adjustments, Mr. Huxley seems to be still more unsuccessful. He states that the maxillary cardo is connected with "a thin band which runs round the posterior margin of the epicranium and is firmly united with it only on its dorsal side." Thus he regards the maxillæ as inserted on the back of the skull through a band which he is consequently compelled to regard as a part of the cranial wall.



This view, if sustained, would clash with the mode of suspension observed in the bee, where the maxillæ have endocranial connections with the basi-occipital or ventral region of the skull. A careful examination of the cockroach has convinced us that here also the cardines of the maxillæ rest on a ridge which crosses the basis-cranii in front of the occipital foramen, and that it is only a slender ridge, not intimately connected with the maxillæ, which passes round like a rim behind this foramen. The transverse ridge which supports the maxillæ of the cockroach thus turns out to be only a simplified variation of the bee's suspensorium.

The basi-cranial region of coleoptera seems unwilling to be brought into line with the same part in other insect tribes: and Gegenbaur is certainly wrong in citing them as an instance of insects with large endocranium. The occipital foramen of beetles is arched over by a small but beautiful framework, which may correspond to Wolff's jugum at the root of the bee's mesocephalic pillars. In the beetles there are no pillars however, at least in the normal situation; but if we examine the clypeus we shall find the upper ends of such pillars, with ridges descending near the outer walls to the anterior basicranial region (to the submentum); and near their lower end the maxillary cardines are

inserted. This may indicate that the beetle has the usual parts, but resolved so as to give the long mentum, submentum, and gula, while other insects have all these parts condensed into the complex system of cross ridges in advance of the occipital foramen.

Only a few words can be said as to the cranial splachnodemes, or that part of the endocranium which consists of hardenings of the pharynx. The mouth is floored by a stiff, tongue-like plate (*hypopharynx* or *lingua*, to be distinguished from the long *ligule* or tip of the labial proboscis). This hypopharynx has a large number of small perforations which Wolff has shown to be olfactory: it receives two large ducts from the cephalic salivary glands.[5] From the posterior end of the hypopharynx run back two long rods, barbed on one side at their extremities. Over the mouth is a similar but simpler epipharynx. To these chitinous pieces the border of the pharynx is attached, and they are supplied with muscles. If we open the bee's mouth, we shall find the funnel-shaped opening kept expanded by the epipharynx above, the hypopharynx below, and its long processes (faucial processes we may say) at the sides. All these hard structures keep open the soft membrane of the pharynx, just as a naturalist's dredge is kept gaping by its iron armature. The piercing insects have the cranial splachnodemes developed into a strong pharynx-case which emits and supports the piercing setæ.

EXPLANATION OF CUTS.

FIG. 1.—Internal view of frontal region of bee. C, clypeus: EC, epicranium: G, gena: LR, labrum: MD, mandible: OC, compound eye: AT, insertion of antennæ.

FIGS. 2. and 3.—Lateral and posterior (diagrammatic) views of head of bee. MC, mesocephalic pillars: BR, basicranial rods: MR, maxillary ramus: BL, basilabium: MD, mandible: MX, maxilla (its blade folded): AT, antenna: FO, occipital foramen.

FIG. 4.—MS, suspensorium of ant: MR, its maxillary rami: MX, maxillæ: BL, basilabium: ML, medilabium: PG, paraglossæ: LG, ligule: LP, labial palp: FO, occipital foramen: MC, mesocephalic pillars.

FIG. 5.—Bee's suspensorium and its connections. BR, its basicranial rods, arising from front of (FO) occipital foramen: MR, its maxillary rami, supporting (MX) maxillæ: (BL) basi labium: ML, etc., as in fig 4, magnified 30 diameters. Below BL its labial rods are seen.

FIG. 6.—Endocranium, etc., of cockroach (Blatta). EC, endocranium: CA, cardo: MX, etc., as in fig. 4.

[5] The thoracic salivary glands send their long duct to the medilabium and thence to the ligule. Carl v. Siebold has discovered three distinct salivary systems in the bee, two cephalic and one thoracic. (*Zoologist*, 1873, p. 3498.)

Sub-elytral Air-passages in Coleoptera. By Carl F. Gissler, of Brooklyn, N. Y.

The rapidly progressing coloration of the entire integument at the time of transition from the chrysalis into the imago-state, the closing chapter in ontogenesis, is partly due to the photographic influence of light, partly to an oxidation by the atmosphere, produced by increased activity of breathing often observed in the first few minutes of existence as imago. By the fuller breathing and repeated extending of the wings and elytra (by lifting the latter, if connate), a heightened gas-exchange is made possible in the still semi-chitinized integument.

Another thing for us to consider in the elytron, now receiving the finishing touch, is its pattern and sculpture inherited from ancestors and often varied through climatological influences as well as through the law of adaptation and the phenomenon of mimicry. In many elytra we find fine pores on the upper side and in others longitudinal canals. I do not take the longitudinal canals of the uppermost layer, the cuticula, for tubes with walls proper, but regard them merely as lacunary clefts, the spaces through which air found access in the first few moments of existence as chrysalis solidifying and gluing together the elementary cells of the elytron.

In Opatrinus and Boletotherus we find a small elongate "plate" produced by a fold of the matrix; it is more (Opatrinus) or less (Boletotherus) concave and occurs on the inner side of the epipleuræ, about in the middle where its thinnest margin is inflexed. This inflection is an arrangement for pleural breathing. The fold occurs especially in those species of Coleoptera where the epipleuræ are wanting, Buprestidæ, Curculionidæ, etc., and is called by Dr. Leconte "inflexed epipleural fold." He says its object is to enable the insect to fix the elytra and body firmly together.

In many, if not in all winged Coleoptera, we also find a small, more or less circular smooth niche, usually near the inner humeral bend of the elytron, undoubtedly intended for fitting into the wing and holding it in position. The inner fold may be called "plica trachealis," the niche (excavation) "fossa."

The plica, often running parallel with the external epipleura,

plays undoubtedly a rôle in the breathing of Coleoptera. In many cases the supply of air is brought about through a fissure, "fissura trachealis," usually found at the middle of the epipleura or through a groove beginning at the elytral tip and opening into the sub-elytral space near the middle.

In Coleoptera with connate elytra,—Eleodes, Embaphion, Curculionidæ,—air finds access by an occasional flapping of the last few flexible abdominal segments at the time of depositing excrements, as often observed by me in Tenebrionidæ, Coccinellidæ, etc.

The following genera have been examined by me: Opatrinus, Boletotherus, Dyscinetus, Euryomia, Allorhina, Cucujus, Megalodacne, Melanotus, Alaus, Coccinella, Hylobius, Eros, Brachinus, Calosoma, Galerita, Carabus, Lebia, Byrrhus, Eleodes, and Amphidora.

Aberrations have been noticed to occur in *Cucujus clavipes*. Here the fissure is wanting. Air finds access through a deep, longitudinal median groove, along the dorsal segments of the abdomen, extending to the pygidium.

Megalodacne heros: Pleural fissure faintly indicated, but with a thickened light brown ridge at the margin of the broad, black median band. Elytra exteriorly smooth and round, lower lamellæ with longitudinal grooves and flat interspaces. This arrangement represents a system of canals extending over the folded wings, allowing an access of air from tip of the elytra.

Eros fraternus: Entire integument soft, abdominal segments lobe-like, prominent at the pleuræ; thereby, as well as through the anastomosing elytral facetations, a receptacle for air is formed in this flat bark-beetle.

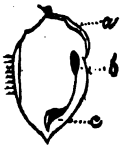
In Brachinus and Galerita the elytra but loosely cover the abdomen, therefore requiring less developed characters in the elytra. In Carabus the body is wingless, showing therefore a capacious air-receptacle. Elytral characters wanting.

In *Lebia grandis*, a bark-beetle, the epipleura is suddenly narrowed at the place where usually the fissure is found, the latter being entirely wanting. About fifteen large pores are found, perforating the elytron and running "seriatim" from near the humerus to near the external emargination of elytral tip.

In Byrrhus a duplicature or bead runs off from the smooth

plica to the posterior end forming there a conspicuous fissure. The connate elytra fit nearly hermetically to the body, overlapping the abdominal segments. Here air will find access by the occasional flapping of the last three or four segments of the abdomen, which is also the case in Coccinella.

FIG. 1.



ELYTRON OF BYRRHUS: *a*, epipleura; *b*, anterior fossa; *c*, posterior, concave, inflexed portion, or plica.

TWO NEW USES OF IMPORTANT INSECTICIDES. By A. J. COOK, of Lansing, Mich.

As is well known, there is scarcely a worse pest to the pomologist than the codling moth, *Carpocapsa Pomonella*, Linn. Many thousand dollars' worth of our fruit is consumed annually to feed this destroyer. The mischief done is augmented from the fact that the best preventive hitherto known has not been effective until the insect destroyed had wrought its evil work. We have known no way to destroy the moths, but could only capture and destroy the larvæ after the apples were eaten. Last winter I learned, from Mr. J. S. Woodward, of Lockport, New York, that trees thoroughly treated with Paris green, about the 20th of May, bore fruit which was wholly exempt from the ravages of the "apple worm." Acting upon this suggestion, I sprayed some apple trees the 25th of May and again the 20th of June; but I used London purple, arsenite of lime, which is nearly as insoluble in water as Paris green. I used one pound of the poison to one hundred gallons of water. The trees were loaded with fruit; and their fruit has been seriously injured by codling moth larvæ whenever they have borne fruit during previous years. Careful examination made August 20th of this year disclosed no injured apple. A few apples showed signs of the previous work of the larvæ; but as no burrow extended for more than one-fourth of an inch, no harm was done. Other apple trees a few rods distant, to which the

poisoned liquid was not applied, are bearing fruit one-fourth to one-half of which is wormy. From the slight amount of the poison applied to each tree, not more than one-third of an ounce, the cost of the remedy is light. Neither do I apprehend danger from its use at so early a date. To one portion of one tree I applied the poison in a greater quantity than was necessary, so that some of the foliage was killed. On the 19th of August, I cut from the trees, which had received the concentrated mixture, one hundred of the blossom ends, the part where the poison would be most readily held, and submitted them to Professor Kedzie for chemical analysis. No trace of the poison was found. The fact that this poison is now extensively used in the West to destroy the canker worm, and with no sign of evil, sustains the argument in favor of its harmless nature so far as the fruit is concerned. The danger of poisoning the soil is a matter for the chemist to decide. If future experience sustain the conclusions as to the efficiency of this remedy, in case of the "apple worm," it will be an important discovery.

Up to the present time there has been no satisfactory method of fighting such pests as the cabbage maggot, and other insects of the same genus, and the squash borer. The late Dr. Walsh recommended hot water; this has proved but partially successful. From the value of bisulphide of carbon as a destroyer of museum pests, and from its efficacy in destroying the phylloxera of Europe, I was led to try its power in overcoming our subterranean insect pests. Both my pupils and myself tried experiments with the happiest results. A small hole was made in the earth near the main root of the plant, by use of a walking stick, and about fifteen cubic centimetres of the liquid were poured in, and the hole quickly filled with earth, which was pressed down with the foot. In every case the insects were killed without injury to the plants. I believe this liquid promises satisfactory results in fighting the above mentioned insects, the peach borer, and other insects which attack the roots of our cultivated plants. We must remember, of course, that this liquid is very volatile and the vapor exceedingly explosive.

TITLES OF OTHER PAPERS READ IN SUBSECTION OF ENTOMOLOGY.

BRIEF REMARKS ON THE MECHANICAL INGENUITY OF THE HOUSE SPIDER, AND THE HABITS OF THE HOUSE FLY. By W. W. Wheildon, of Concord, Mass.

REMARKS ON TREE-CRICKETS. By C. V. Riley, of Washington, D. C.

REMARKS ON THE EARLY STAGES OF BLEPHAROCERA. By C. V. Riley, of Washington, D. C.

ON BIOLOGICAL COLLECTIONS OF INSECTS. By H. A. Hagen, of Cambridge, Mass.

ON SOME VERY RARE INSECT DEFORMITIES. By H. A. Hagen, of Cambridge, Mass.

ON THE HESSIAN FLY. By H. A. Hagen, of Cambridge, Mass.

ON THE ANATOMY OF PRODOXUS DECIPIENS. By H. A. Hagen, of Cambridge, Mass.

INSECTS FROM COPAL. By D. S. Martin, of New York, N. Y.

MIGRATIONS OF ROCKY MOUNTAIN LOCUSTS. By A. S. Packard, Jr., of Providence, R. I.

OCCURRENCE OF ALETIA ARGILLACEA IN WISCONSIN. By P. R. Hoy, of Racine, Wis.

SOME POINTS IN THE ANATOMY OF THE COCCIDÆ. By E. L. Mark, of Cambridge, Mass.

STRUCTURE AND DEVELOPMENT OF CERTAIN HYMENOPTEROUS GALLS. By H. F. Bassett, of Waterbury, Conn.

LIST OF COLEOPTERA HATCHED FROM A FEW HICKORY TWIGS. By J. L. Le Conte, of Philadelphia, Pa.

THE HONEY-ANTS OF THE GARDEN OF THE GODS, COLORADO. By H. C. McCook, of Philadelphia, Pa.

NOTES ON NORTH AMERICAN GALEODES. By J. D. Putnam, of Davenport, Iowa.

CONTRIBUTIONS OF APICULTURE TO SCIENCE. By A. J. Cook, of Lansing, Mich.

GENERIC CHARACTERS OF THE NOCTUIDÆ. By A. R. Grote, of Staten Island, N. Y.

ON THE CLASSIFICATION OF THE TORTRICIDÆ. By C. H. Fernald, of Orono, Me.

ON PHOCOPTERIS ANGULIFASCIANA. By C. H. Fernald, of Orono, Me.

ON THE STRUCTURE OF THE MOUTH ORGANS IN THE LEPIDOPTERA. By E. Burgess, of Boston, Mass.

THE CONTRIBUTIONS OF THE CAMBRIDGE ENTOMOLOGICAL CLUB TO THE PROGRESS OF ENTOMOLOGY. By B. Pickman Mann, of Cambridge, Mass.

PERMANENT

SUBSECTION OF ANTHROPOLOGY.

ADDRESS

BY

PROFESSOR J. W. POWELL,

CHAIRMAN OF SUBSECTION OF ANTHROPOLOGY.

WYANDOTTE GOVERNMENT. A SHORT STUDY OF TRIBAL SOCIETY.

In the social organization of the Wyandottes four groups are recognized—the family, the gens, the phratry, and the tribe.

THE FAMILY.

The family as the term is here used is nearly synonymous with household. It is composed of the persons who occupy one lodge or, in their permanent wigwams, one section of a communal dwelling. These permanent dwellings are constructed, in an oblong form, of poles interwoven with bark. The fire is placed in line along the center and is usually built for two families, one occupying the place on each side of the fire.

The head of the family is a woman.

GENS.

The gens is an organized body of consanguineal kindred in the female line. "The woman carries the gens," is the formulated statement by which a Wyandotte expresses the idea that descent is in the female line. Each gens has the name of some animal—the ancient of such animal being its tutelar god. Up to the time when the tribe left Ohio, eleven gentes were recognized, as follows:—

Deer, Bear, Highland turtle (striped), Highland turtle (black), Mud turtle, Smooth large turtle, Hawk, Beaver, Wolf, Sea Snake, and Porcupine.

In speaking of an individual he is said to be a Wolf, a Bear, or a Deer as the case may be, meaning thereby that he belongs to that gens; but in speaking of the body of people comprising a gens they are said to be relatives of the Wolf, the Bear, or the Deer, as the case may be.

There is a body of names belonging to each gens so that each person's name indicates the gens to which he belongs. These names are derived from the characteristics, habits, attitudes, or mythologic stories connected with the tutelar god.

The following schedule presents the name of a man and woman in each gens as illustrating this statement.

	INDIAN.	ENGLISH.
Man of deer gens	— De-wa-ti-re	= Lean deer.
Woman of deer gens	— A-ya-jĭn-ta	= Spotted fawn.
Man of bear "	— A-tu-e-tĕs	= Long claws.
Woman of bear "	— Tsa-man'-da-ka-ĕ'	= Grunting for her young.
Man of striped turtle gens	— Ta-ha'-son-ta-ra-ta-se	= Going around the lake.
Woman of " " "	— Tso-we-yŭñ-kyn	= Gone from the water.
Man of mud turtle gens	— Sha-yan-tsu-wat'	= Hard skull.
Woman of mud turtle gens	— Yan-däsh-shu-räs	= Finding sand beach.
Man of smooth large turtle gens	— Hun'-du-cu-ta	= Throwing sand.
Woman of " " " "	— Tsu-ca-ĕn'	= Slow walker.
Man of Wolf gens	— Ha-ro'-un-yû	= One who goes about in the dark a prowler.
Woman of Wolf gens	— Yan-di-no	= Always hungry.
Man of Snake gens	— Hu-ta-hu'-sa	= Sitting in curled position.
Woman of Snake gens	— Di-je-rons	= One who ripples the water.
Man of Porcupine "	— Han-du'-tun	= The one who puts up quills.
Woman of " "	— Ke'-ya-runs-kwa	= Good sighted.

THE PHRATRY.

There are four phratries in the tribe—the three gentes, bear, deer, and striped turtle constituting the first; the highland turtle, black turtle, and smooth large turtle the second; the hawk, beaver and wolf, the third; and the sea snake and porcupine the fourth.

This unit in their organizations has a mythologic basis and is chiefly used for religious purposes, in the preparation of medicines, and in festivals and games.

The eleven gentes as four phratries constitute the tribe.

Such is the social organization of the Wyandottes.

Each gens is a body of consanguineal kindred in the female line, and each gens is allied to other gentes by consanguineal kinship through the male line, and by affinity through marriage.

To be a member of the tribe it is necessary to be a member of a gens; to be a member of a gens it is necessary to belong to some family; and to belong to a family a person must have been born in the family so that his kinship is recognized; or he must be adopted into a family and become a son, brother, or some definite relative; and this artificial relationship gives him the same standing as actual relationship in the family, in the gens, in the phratry, and in the tribe.

Thus the tribe is a body of kindred.

Of the four groups thus described, the gens, the phratry, and the tribe constitute the series of organic units; the family or household as here described is not a unit of the gens or phratry as two gentes are represented in each — the father must belong to one gens and the mother and her children to another.

GOVERNMENT.

Society is maintained by the establishment of government, for rights must be recognized and duties performed.

In this tribe there is found a complete differentiation of the military from the civil government.

CIVIL GOVERNMENT.

The civil government inheres in a system of councils and chiefs.

In each gens there is a council composed of four women called Yu-wai'-yu-wa'-na.

These four women councillors select a chief of the gens from its male members — that is from their brothers and sons. This gentile chief is the head of the gentile council.

The council of the tribe is composed of the aggregated gentile councils. The tribal council therefore is composed one-fifth of men and four-fifths of women.

The sachem of the tribe or tribal chief is chosen by the chiefs of the gentes.

There is sometimes a grand council of the gens composed of the councillors of the gens proper and all the heads of households and leading men — brothers and sons.

There is also sometimes a grand council of the tribe composed of the council of the tribe proper and the heads of households of the tribe and all the leading men of the tribe.

These grand councils are convened for special purposes.

METHODS OF CHOOSING AND INSTALLING COUNCILLORS AND CHIEFS.

The four women councillors of the gens are chosen by the heads of households — themselves being women. There is no formal election, but frequent discussion is had over the matter from time to time in which a sentiment grows up within the gens and throughout the tribe that in the event of the death of any councillor a certain person will take her place.

In this manner there is usually one, two, or more potential councillors in each gens who are expected to attend all the meetings of the council though they take no part in the deliberations and have no vote.

When a woman is installed as councillor a feast is prepared by the gens to which she belongs and to this feast all the members of the tribe are invited. The woman is painted and dressed in her best attire and the sachem of the tribe places upon her head the gentile chaplet of feathers and announces in a formal manner to the assembled guests that the woman has been chosen a councillor. The ceremony is followed by feasting and dancing often continued late into the night.

The gentile chief is chosen by the council women after consultation with the other women and men of the gens. Often the gentile chief is a potential chief through a period of probation. During this time he attends the meetings of the council but takes no part in their deliberations and has no vote.

At his installation the council women invest him with an elaborately ornamented tunic, place upon his head a chaplet of feathers, and paint the gentile totem on his face. The sachem of the tribe then announces to the people that the man has been made chief of the gens and admitted to the council. This is also followed by a festival.

The sachem of the tribe is selected by the men belonging to the council of the tribe. Formerly the sachemship inhered in the bear gens but at present he is chosen from the deer gens from the fact, as the Wyandottes say, that death has carried away all the wise men of the bear gens.

The chief of the wolf gens is the herald and sheriff of the tribe. He superintends the erection of the council house and has the care of it. He calls the council together in a formal manner when directed by the sachem. He announces to the tribe all the decisions of the council and executes the directions of the council and of the sachem.

Gentile councils are held frequently from day to day and from week to week and are called by the chief whenever deemed necessary. When matters before the council are considered of great importance a grand council of the gens may be called.

The tribal council is held regularly on the night of the full moon of each lunation and at such other times as the sachem may determine; but extra councils are usually called by the sachem at the request of a number of councillors.

Meetings of the gentile councils are very informal; but the meetings of the tribal councils are conducted with due ceremony.

When all the persons are assembled the chief of the wolf gens calls them to order, fills and lights a pipe, sends one puff of smoke to the heavens and another to the earth. The pipe is then handed to the sachem who fills his mouth with smoke and, turning from left to right with the sun, slowly puffs it out over the heads of the councillors who are sitting in a circle. He then hands the pipe to the man on his left and it is smoked in turn by each person until it has been passed around the circle.

The sachem then explains the object for which the council is called. Each person in the way and manner he chooses tells what he thinks should be done in the case. If a majority of the council is agreed as to action the sachem does not speak but may simply announce the decision. But in some cases there may be protracted debate which is carried on with great deliberation. In case of a tie the sachem is expected to speak.

It is considered dishonorable for any man to reverse his decision after having spoken.

Such are the organic elements of the Wyandotte government.

Functions of Civil Government.

It is the function of government to preserve rights and enforce the performance of duties. Rights and duties are correlative. Rights imply duties, and duties imply rights. The right inhering in the party of the first part imposes a duty on the party of the second part. The right and its correlative duty are inseparable parts of a relation that must be maintained by government; and the relations which governments are established to maintain may be treated under the general head of rights.

In Wyandotte government these rights may be classed as follows:—

First:—Rights of Marriage.
Second:—Rights to names.
Third:—Rights to personal adornments.
Fourth:—Rights of order in encampments and migrations.
Fifth:—Rights of property.
Sixth:—Rights of person.
Seventh:—Rights of community.
Eighth:—Rights of religion.

To maintain rights, rules of conduct are established, not by formal enactment but by regulated usage. Such custom-made laws may be called regulations.

Marriage Regulations.

Marriage between members of the same gens is forbidden, but consanguineal marriages between persons of different gentes are permitted.

For example, a man may not marry his mother's sister's daughter as she belongs to the same gens with himself; but he can marry his father's sister's daughter because she belongs to a different gens.

Husbands retain all their rights and privileges in their own gentes though they live with the gentes of their wives. Children, irrespective of sex, belong to the gens of the mother. Men and women must marry within the tribe. A woman taken to wife from without the tribe must first be adopted into some family of a gens other than that to which the man belongs. That a woman may

take for a husband a man without the tribe, he must also be adopted into the family of some gens other than that of the woman. What has been called by some ethnologists endogamy and exogamy are correlative parts of one regulation, and the Wyandottes, like all other tribes of which we have any knowledge in North America, are both endogamous and exogamous.

Polygamia is permitted, but the wives must belong to different gentes. The first wife remains the head of the household. Polyandria is prohibited. A man seeking a wife consults her mother, sometimes directly and sometimes through his own mother. The mother of the girl advises with the women councillors to obtain their consent, and the young people usually submit quietly to their decision. Sometimes the women councillors consult with the men.

When a girl is betrothed, the man makes such presents to the mother as he can. It is customary to consummate the marriage before the end of the moon in which the betrothal is made. Bridegroom and bride make promises of faithfulness to the parents and women councillors of both parties. It is customary to give a marriage feast in which the gentes of both parties take part. For a short time at least, bride and groom live with the bride's mother or rather in the original household of the bride.

The time when they will set up housekeeping for themselves is usually arranged before marriage.

In the event of the death of the mother the children belong to her sister or to her nearest female kin, the matter being settled by the council women of the gens. As the children belong to the mother, on the death of the father the mother and children are cared for by her nearest male relatives until subsequent marriage.

NAME REGULATIONS.

It has been previously explained that there is a body of names the exclusive property of each gens. Once a year, at the green-corn festival, the council women of the gens select the names for the children born during the previous year, and the chief of the gens proclaims these names at the festival. No person may change his name, but every person, man or woman, by honorable or dishonorable conduct, or by remarkable circumstance may win a second name commemorative of deed or circumstance, which is a kind of title.

REGULATIONS OF PERSONAL ADORNMENT.

Each class has a distinctive method of painting the face, a distinctive chaplet to be worn by the gentile chief and council women when they are inaugurated, and, subsequently, at festival occasions, and distinctive ornaments for all its members to be used at festivals and religious ceremonies.

REGULATIONS OF ORDER IN ENCAMPMENT AND MIGRATIONS.

The camp of the tribe is in an open circle, or horseshoe, and the gentes camp in the following order, beginning on the left, and going around to the right:—

Deer, Bear, Highland Turtle (striped), Highland Turtle (black), Mud Turtle, Smooth large Turtle, Hawk, Beaver, Wolf, Sea Snake, Porcupine.

The order in which the households camp in the gentile group is regulated by the gentile councillors, and adjusted from time to time in such a manner that the oldest family is placed on the left and the youngest on the right. In migrations and expeditions the order of travel follows the analogy of encampment.

PROPERTY RIGHTS.

Within the area claimed by the tribe each gens occupies a smaller tract for the purpose of cultivation. The right of the gens to cultivate a particular tract is a matter settled in the council of the tribe, and the gens may abandon one tract for another only with the consent of the tribe. The women councillors partition the gentile land among the householders and the household tracts are distinctly marked by them. The ground is repartitioned once in two years.

The heads of households are responsible for the cultivation of the tract, and should this duty be neglected the council of the gens calls the responsible parties to account.

Cultivation is communal; that is, all of the able-bodied women of the gens take part in the cultivation of each household tract in the following manner:—

The head of the household sends her brother or son into the forest or to the stream to bring in game or fish for a feast; then the able-bodied women of the gens are invited to assist in the

cultivation of the land, and when this work is done a feast is given.

The wigwam or lodge and all articles of the household belong to the woman — the head of the household — and at her death are inherited by her eldest daughter, or nearest of female kin. The matter is settled by the council women. If the husband die his property is inherited by his brother or his sister's son, except such portion as may be buried with him. His property consists of his clothing, hunting and fishing implements, and such articles as are used personally by himself.

Usually a small canoe is the individual property of the man. Large canoes are made by the male members of the gentes and are the property of the gentes.

RIGHTS OF PERSONS.

Each individual has a right to freedom of person and security from personal and bodily injury, unless adjudged guilty of crime by proper authority.

COMMUNITY RIGHTS.

Each gens has the right to the services of all its women in the cultivation of the soil. Each gens has the right to the service of all of its male members in avenging wrongs, and the tribe has the right to the service of all its male members in time of war.

RIGHTS OF RELIGION.

Each phratry has the right to certain religious ceremonies and the preparation of certain medicines.

Each gens has the exclusive right to worship its tutelar god, and each individual has the exclusive right to the possession and use of a particular amulet.

CRIMES.

The violations of rights are crimes. Some of the crimes recognized by the Wyandottes are as follows:—

1. Adultery.
2. Theft.
3. Maiming.
4. Murder.
5. Treason.
6. Witchcraft.

A maiden guilty of fornication may be punished by her mother or female guardian, but if the crime is flagrant and repeated so as

to become a matter of general gossip, and the mother fails to correct it, the matter may be taken up by the council women of the gens.

A woman guilty of adultery, for the first offence is punished by having her hair cropped; for repeated offences her left ear is cut off.

THEFT.

The punishment for theft is twofold restitution. When the prosecutor and prosecuted belong to the same gens, the trial is before the council of the gens, and from it there is no appeal. If the parties involved are of different gentes, the prosecutor, through the head of his household, lays the matter before the council of his own gens; by it the matter is laid before the gentile council of the accused in a formal manner. Thereupon it becomes the duty of the council of the accused to investigate the facts for themselves, and to settle the matter with the council of the plaintiff. Failure thus to do is followed by retaliation in the seizing of any property of the gens which may be found.

MAIMING.

Maiming is compounded, and the method of procedure in prosecution is essentially the same as for theft.

MURDER.

In the case of murder, if both parties are members of the same gens, the matter is tried by the gentile council on complaint of the head of the household, but there may be an appeal to the council of the tribe. Where the parties belong to different gentes, complaint is formally made by the injured party, through the chief of his gens in the following manner:

A wooden tablet is prepared upon which is inscribed the totem or heraldic emblem of the injured man's gens, and a picture-writing setting forth the offence follows.

The gentile chief appears before the chief of the council of the gens of the offender, and formally states the offence, explaining the picture-writing, which is then delivered.

A council of the offender's gens is thereupon called and a trial is held. It is the duty of this council to examine the evidence for themselves and to come to a conclusion without further pre-

sentation of the matter on the part of the person aggrieved. Having decided the matter among themselves they appear before the chief of the council of the aggrieved party to offer compensation.

If the gens of the offender fails to settle the matter with the gens of the aggrieved party, it is the duty of his nearest relative to avenge the wrong. Either party may appeal to the council of the tribe. The appeal must be made in due form, by the presentation of a tablet of accusation.

Inquiry into the effect of a failure to observe prescribed formalities developed an interesting fact. In procedure against crime, failure in formality is not considered a violation of the rights of the accused, but proof of his innocence. It is considered supernatural evidence that the charges are false. In trials for all offences, forms of procedure are, therefore, likely to be earnestly questioned.

TREASON.

Treason consists in revealing the secrets of the medicine preparations, or giving other information or assistance to enemies of the tribe, and is punished by death. The trial is before the council of the tribe.

WITCHCRAFT.

Witchcraft is punished by death (stabbing, tomahawking, or burning). Charges of witchcraft are investigated by the grand council of the tribe. When the accused is adjudged guilty he may appeal to supernatural judgment. The test is by fire. A circular fire is built on the ground through which the accused must run from east to west, and then from north to south. If no injury is received he is adjudged innocent; if he falls into the fire he is adjudged guilty. Should a person accused, or having the general reputation of practising witchcraft, become deaf, blind, or have sore eyes, earache, headache, or other diseases considered loathsome, he is supposed to have failed in practising his arts upon others, and to have fallen a victim to them himself. Such cases are most likely to be punished.

OUTLAWRY.

The institution of outlawry exists among the Wyandottes in a peculiar form. An outlaw is one who by his crimes has placed himself without the protection of his clan. A man can be declared an outlaw by his own clan who thus publish to the tribe that they will not defend him in case he is injured by another. But usually outlawry is declared only after trial before the tribal council.

The method of procedure is analogous to that in case of murder. When the person has been adjudged guilty and sentence of outlawry declared, it is the duty of the chief of the Wolf clan to make known the decision of the council. This he does by appearing before each clan in the order of its encampment, and declaring in terms the crimes of the outlaw and the sentence of outlawry which may be either of two grades.

In the lowest grade it is declared that if the man shall thereafter continue in the commission of similar crimes it will be lawful for any person to kill him, and if killed, rightfully or wrongfully, his clan will not avenge his death.

Outlawry of the highest degree makes it the duty of any member of the tribe who may meet with the offender to kill him.

MILITARY GOVERNMENT.

The management of military affairs inheres in the military council and chief; the military council is composed of all the able-bodied men of the tribe; the military chief is chosen by the council from the Porcupine gens. Each gentile chief is responsible for the military training of the youth under his authority. There is usually one or more potential military chiefs who are the close companions and assistants of the chief in time of war, and in case of the death of the chief take his place in the order of seniority.

Prisoners of war are adopted into the tribe or killed. To be adopted into the tribe it is necessary that the prisoner should be adopted into some family.

The warrior taking the prisoner has the first right to adopt him, and his male or female relations have the right in the order of

their kinship. If no one claim the prisoner for this purpose he is caused to run the gauntlet as a test of his courage.

If at his trial he behaves manfully, claimants are not wanting, but if he behaves disgracefully he is put to death.

FELLOWHOOD.

There is an interesting institution found among the Wyandottes as among some other of our North American tribes, namely, that of Fellowhood. Two young men agree to be perpetual friends to each other, or more than brothers. Each reveals to the other the secrets of his life and counsels with him on matters of importance, and defends him from wrong and violence, and at his death is chief mourner.

The government of the Wyandottes, with the social organization upon which it is based, affords a typical example of tribal government throughout North America. Within that area there are several hundred distinct governments. In so great a number there is great variety, and in this variety we find different degrees of organization; the degree of organization being determined by the differentiation of the functions of government, and the correlative specialization of organic elements.

Much has yet to be done in the study of these governments before safe generalizations may be made. But enough is known to warrant the following statement.

Tribal government in North America is based on kinship, in that the fundamental units of social organization are bodies of consanguineal kindred, either in the male or female line; these units being what has been well denominated "gentes."

These "gentes" are organized into tribes by ties of relationship and affinity, and this organization is of such a character that the man's position in the tribe is fixed by his kinship. There is no place in a tribe for any person whose kinship is not fixed, and only those persons can be adopted into the tribe who are adopted into some family with artificial kinship specified. The fabric of Indian society is a complex tissue of kinship — the warp is made of streams of kinship blood and the woof of marriage ties.

With most tribes military and civil affairs are differentiated. The functions of civil government are, in general, differentiated only to this extent: that executive functions are performed by

chiefs and sachems; but these chiefs and sachems are also members of the council. The council is legislature and court. Perhaps it were better to say that the council is the court whose decisions are law, and that the legislative body properly has not been developed.

In general, crimes are well defined. Procedure is formal, and forms are held as of such importance that error therein is *prima facie* evidence that the subject-matter formulated was false.

When one gens charges crime against a member of another, it can of its own motion proceed only to retaliation. To prevent retaliation the gens of the offender must take the necessary steps to disprove the crime, or to compound or punish it. The charge once made is held as just and true until it has been disproved, and in trial the cause of the defendant is first stated. The anger of the prosecuting gens must be placated.

In the tribal governments there are many institutions, customs and traditions which give evidence of a former condition in which society was based,—not upon kinship, but upon marriage.

From a survey of the facts, it seems highly probable that kinship-society, as it exists among the tribes of North America, has developed from connubial society which is discovered elsewhere on the globe. In fact, there are few tribes that seem scarcely to have passed that indefinite boundary between the two social states. Philologic research leads to the same conclusion.

Nowhere in North America have a people been discovered who have passed beyond tribal society, to national society based on property, *i. e.*, that form of society which is characteristic of civilization. Some peoples may not have reached kinship-society; none have passed it.

Nations with civilized institutions, art with palaces, monotheism as the worship of the Great Spirit,—all vanish from the priscan condition of North America in the light of anthropologic research.

Tribes, with the social institutions of kinship, art with its highest architectural development exhibited in the structure of communal dwellings, and polytheism in the worship of mythic animals and nature-gods, remain.

PAPERS READ.

The Dacotah Tribes: their beliefs, and our duty to them outlined. By Henry B. Carrington, Colonel U. S. A.

The pulse of our quick life will brook no check. In the rage for gold, the white man has held for nought the red man's rights. How to get all he has and then get rid of him, at the first chance, and be glad when he is gone, is the sum which we count up, as fast as we can, to kill his race.

I wish to tell some things which will give him grace at your hands. I do not tell where he came from, but as he is a man, he has value, and if the brain force which is here can plan good for the live red man and let him live, it will be worth as much as to read what stones and coals say of the dead past. I will not try to work out the fact as to how he came to be. You who strive to get at the first start of each old race can do that. He is here, and I speak for him.

I felt a strange thrill of zeal for him when Irwakura, the chief of the Japan Legation which visited this country, stated, that "he asked that his train might stop at Echo Cañon, so that he might look upon the first red man he ever met." I give what he said, for your thought. It was this: "We have a tradition in our country, that our people came from the skies in a boat, and we have pictures which represent our ancestors. I know enough, now, to see that people cannot come out of the sky; but when I see those, who for the first time remind me of the pictures of our forefathers, I wonder, with awe, whether America was not their home and the ocean waves took them to Japan." It was a new thought to me, and I pass it to you who are adepts in this branch of study, for further solution.

I speak for the red men of Dacotah, but include all who were associated with my service on the Plains, principally the Pawnees, Winnebagoes, Northern Cheyennes, Arapahoes, Ogallalla and Brulè Sioux and the Crows. I do not say that like thought stirs the brain of all these bands; but in none have I found words which

curse God. White Horse, an old Cheyenne chief, was aroused when he heard white men curse, and said, "it was not so bad for a boy to curse his father and mother, as for a man to curse the Great Spirit, who gave him air, earth, water and all good things." One, thus stated his idea, pointing to a child: "Pappoose wiskeat auteas? Pappoose wiskeat autrara?" (Pappoose curse father and mother)." "The child would have had no father and mother, and the father and mother would have had no child, but for the Great Spirit. Why do the white men curse?" The red man takes up our strong words to express anger; but his reverence for the Great Spirit is above that of some white men who claim to be his master.

Their views as to life after death are no less striking. When the dead were rescued from the battle-field after Fetterman's massacre in 1866, when three officers and seventy-eight men were killed and cut up in thirty minutes, I found that nearly every body was stripped of the muscles of the arm, breast, back, thigh and calves of the legs. The bodies were filled with arrows, one hundred and sixty-eight having been found in three bodies. I quote from the Official Report as to these mutilations. "Eyes torn out and laid on the rocks; teeth chopped out; joints of fingers cut off; brains taken out and placed on rocks, with members of the body; entrails taken out and exposed; hands and feet cut off; arms taken out from socket; eyes, ears, mouth and arms penetrated with spearheads, sticks and arrows; punctures upon every sensitive part of the body, even to the soles of the feet and the palms of the hands." I asked a member of the Red Cloud band, why this was done; and the key to these mutilations was startling and impressive. Their idea of the spirit land is, that it is a physical paradise, but we enter upon its mysteries just in the condition we hold when we die. In the Indian paradise every physical taste or longing is promptly met. If he wants food it is at hand. Water springs up for ready use. Ponies and game abound. Blossoms, leaves and fruit never fail. All is perennial and perpetual. But what is the Indian hell? It is the same in place and in profusion of mercies; but the bad cannot partake. Like Dives who saw, craved and panted for relief, he cannot enjoy. In the light of this idea, these tortured bodies had a new significance. With the muscles of the arms cut out, the victim could not pull a bow-string or trigger; with other muscles gone he could not ride in the stirrup or stoop to drink; so that while every sense was in agony for relief from

hunger or thirst, there could be no relief at all. The red man does not have the moral sense which would argue that every vice is crippling the moral and mental muscles, so that every fault leaves man less perfect for beginning a grand career beyond the grave; but the germ thought is in his mind, and the white man can give it better force and activity if he will care half as well for the red man as he does for Hottentots, Asiatics and strangers of the South Pacific Isles. Before Chief Joseph of the Nez Perces fled from his rightful home, in 1877, to fight for his very life, he was asked if he wanted schools on the Wallowa reservation, he said "No!" When asked his reason, he replied that "it would bring churches." "Don't you want churches?" was the next inquiry. "No, no," he answered, "it will teach us to quarrel about God, as the Catholics and Protestants do. We fight each other, but we don't want to learn to fight about God." When asked to sell his reservation, he sharply replied: "Do you believe that we came out of the bosom of the Earth? I know you do. Then the earth is our mother. Would you sell your mother? I never will sell my mother."

Their mode of burial by raising the dead upon platforms, above the reach of wild beasts, until the dust returns to mother earth, is full of strange fancies. Their dread of being hanged is due to their fear that as the spirit leaves the body while the feet are above the earth, it will be doomed to the loss of all capacity for walking and running hereafter, the most horrible of fates for the red man whose hope and joy are in physical bliss alone.

One more phase of the red man's life is pertinent to this inquiry. "What shall we do with and for him?" It seems to me that here is just the place to speak a word for him, because the work of science is to bless man; and we do not push inquiry so far back into the silent past and into the cold forms of the once igneous trap and granite, and reconstruct old life from impress and fragment, that we ignore the living present. It is better to preserve a race with which our fathers did compete for this fair continent, than to explain how the red man came here, and what was his pedigree direct from Adam, or some anterior protoplasm. We call him a savage, but he can be a friend. The Narragansetts and Delawares were true to the white man. The Pawnees, Winnebagoes and Crows have been true to the white man. Spotted Tail, with his ten thousand companions, is true, and he has not failed to meet pledges made in 1866. Red Cloud and his eleven thousand

followers are true, in spite of repeated change of his reservation, and untold deprivations which the tide of western growth has brought. I say, plainly, that the red man when he enters into a fair contract, understandingly, is as faithful to obligation as the average white man, and that from 1865 until the present time, there has not been a border campaign which did not have its impulse in the aggressions of the white man. When Dull Knife, the Cheyenne chief, broke out of Camp Robinson, and his braves and squaws fought until nearly all were killed, it was because they would rather die than be sent to the Indian Territory, where they had neither friends nor country. And yet this man, in 1866, refused to take the war path with Red Cloud, and suffered much to prove that he was the white man's friend.

The occupation of the Powder river country, and the valleys of the Big Horn and Yellowstone, during that year, in the face of Harney's treaty of 1865, opened up that series of disasters which has sacrificed so many white men, and exacted so much of vengeance upon the red man himself. It was time that he was dealt with as other men are dealt with. Legislation and adjudication have changed the old theory of our Supreme Court, which held that the Indians were internal nations, dependent indeed, but capable of contracting by treaty. We must treat them as men, under law, and punish the red and white men alike and protect them alike. Then there will be found in America an asylum even for those whose fathers were here when our fathers sought an asylum, and we shall crown our work of the emancipation of the African, by the preservation of the Indian.

I was asked yesterday to explain why the Indians used arrows so freely upon the bodies of their victims. It is a part of their superstition. While they will not often use, again, an arrow shot at an enemy, if it miss him, because the Great Spirit did not wish it to hit, they count to their credit every act of courage. Thus, when in single combat, the red man would fail to get a scalp, unless aided, the friend who has helped him, shoots arrows into the body and keeps the record, to show that while he did not get the scalp he was the cause of its being secured. This very chief, Dull Knife, when in 1866, he refused to join Red Cloud, at the first outbreak of war, was slashed across the shoulders with bows, in contempt, with the cry of "Coo!" (coward) and these "Coos" are recorded by knots tied in their ponies' manes, as the next thing to

victory. So the arrows used upon a body under a state of facts such as given are so many "Coos" to the credit of a warrior. The hundreds of arrows found in the bodies after the Fetterman massacre, showed that the whole force of the red man was employed to silence the brave men who fought with desperation against an overwhelming body. It is enough for us to know that white men first stole their lands, and then sold them arms, and did by every low passion work out the scheme by which we should treat the Indian as a brute to be exterminated, rather than as a man to be saved. In the horrors of that calamity when loved companions fell so suddenly after safely passing the ordeal of four years of war, and it seemed as if there was no salvation for the rest of the small force in Dacotah, and when our wives and our children were in peril, so that no one knew what the next hour would bring of toil or trial, I could not but feel that, if I had been a red man, I would have fought as bitterly, if not as cruelly, for my rights and my home, as the red man fought.

Be it our part to strengthen the hands of those who would save the red man, so that the eternal disgrace of his extinction shall not attach to America while Christianity is its strength and its glory.

THE THEORY OF PRIMITIVE DEMOCRACY IN THE ALPS. By DENMAN W. ROSS, of Cambridge, Mass.

As long ago as 1871, Sir Henry Maine said, in his book entitled "Village Communities in the East and West," that he knew nothing more striking among Mr. Freeman's many contributions to our historical knowledge than his identification of the fragments of Teutonic society, organized upon its primitive model, which are to be found in the Forest Cantons of Switzerland. Sir Henry Maine refers to the Swiss *Landesgemeinden*, or commonwealths, and to Mr. Freeman's theory about them; that they are an archaic political institution that has survived to our day.

You all remember, I am sure, the stirring description that Mr. Freeman gives of this institution in his Essay upon the English Constitution. "Year by year," he says, "on certain spots among the dales and mountain-sides of Switzerland, the traveller who is

daring enough to wander out of beaten tracks, and to make his journey at unusual seasons, may look on a sight such as no other corner of the earth can any longer set before him. He may there gaze and feel, what none can feel but those who have seen with their own eyes, what none can feel in its fulness more than once in a life-time, the thrill of looking for the first time, face to face, on freedom in its purest and most ancient form. He is there in a land where the oldest institutions of our race, institutions which may be traced up to the earliest times of which history or legend gives us any glimmering, still live on in their primeval freshness. He is in a land where an immemorial freedom, only less eternal than the rocks that guard it, puts to shame the boasted antiquity of kingly dynasties, which, by its side, seem but as innovations of yesterday. There, year by year, on some bright morning of the spring-tide, the Sovereign People, not intrusting its rights to a few of its own number, but discharging them itself in the majesty of its corporate person, meets in the open market-place, or in the green meadow at the mountain's foot, to frame the laws to which it yields obedience as its own work, to choose the rulers whom it can afford to greet with reverence as drawing their commission from itself." After saying so much in general with regard to "freedom in its purest and most ancient form," which he has discovered in the Alps, Mr. Freeman goes on to describe what he calls "the immemorial democracy of Uri;" and this description serves to illustrate what I have just read to you.

Without doubt we have in the Swiss commonwealths, in Uri and Appenzell, for example, the purest form of democracy,—democracy without representation. Every freeman has an equal voice in the assembly, and he is there in person to help in the making of laws and the appointment of magistrates; but the assertion that this democracy is immemorial and archaic is the statement of a theory, which, though it has been generally received, has never been established by any sufficient or satisfactory evidence.

It is the object of the following argument to show:—1. That the theory of primitive democracy in the Alps has not been established by satisfactory evidence; that it cannot, therefore, be regarded as a contribution to historical knowledge; and, 2. That according to the accounts and records there was nothing of democracy in the political constitution of early Teutonic society. You will see that there were two classes of freemen,—the chieftains

and their followers: that the office of chieftain was hereditary and also elective; not elective, however, upon democratic principles, but by individual choice, prompted by individual preference. The adherence of the followers to their chieftains was in all cases personal; and it was voluntary unless the followers were dependents. You will see that the evolution of the political and economic institutions of the early Germans was directly towards feudalism, a social system based upon personal adherence and personal dependence.

I have just said that Mr. Freeman has given us no satisfactory evidence to show that the democracy of the Swiss Cantons is either immemorial or archaic, to show that it is an archaic political institution that has survived to our day. He tells us, in the first volume of his "Norman Conquest" (Cap. III, § 2, note), that he intends to treat the history of the democratic Cantons of Switzerland more at large in the second volume of his "Federal Government," and he says: "I will now only say that, though the amount of independence enjoyed by the ancient Cantons has been greatly exaggerated, there is evidence to show that, in some districts at least, the old Teutonic system can be traced back uninterruptedly as far as we have any records at all, so that we may fairly presume an unbroken succession from the Germans of Tacitus." The same argument is reiterated less concisely in the appendix of notes to the "Essay on the English Constitution." This is all the argument we have as yet received from Mr. Freeman. He intends to enlarge upon it in the second volume of the "Federal Government;" but the volume has not appeared, and we have no more promise of it. In the meantime we have a brief which we will proceed to examine.

Mr. Freeman tells us that we can trace the old system back a long way into the Middle Ages, that we may therefore presume an unbroken succession from the Germans of Tacitus. You will observe, in the first place, that Mr. Freeman assumes that it is the old system which we trace back in this way. He assumes that the present existing system is substantially the same as that which existed in the time of Tacitus, which is certainly something to be proved, and not to be assumed. Mr. Freeman should have said the present system may be traced back a long way into the Middle Ages. He should then have shown by a careful analysis and comparison that it is identical with that which existed in the time

of Tacitus. Then he might have pointed out the probable connection between the two systems. But even then the argument would have been a very weak one; for, as you will observe, in the second place, Mr. Freeman admits that there is a gap in the records across which we are helped by inference and not by evidence. We trace, he says, the old system back into the Middle Ages, and presume that it goes back to the Germans of Tacitus. Of course the force or weakness of the argument depends upon the extent of this gap between the mediæval records and the account of Tacitus. The greater the gap, the weaker the argument.

It happens that the first record containing any reference to the Canton Uri is of the year 732. Eto, the Abbot of Reichenau, was banished by the Duke of the Alamanni to Uri,—*in Uroniam*. (Monumenta Germaniæ. Scriptores, V, p. 98.) A document of 853 contains the words *pagellum Uroniæ*, and a reference to *homines tam liberos quam et servos*. The little canton, with its lands and people, churches and houses, is given by Ludwig the German to the cloister of nuns at Zurich. It appears as private estate, serving private ends,—*jure proprietatis et ad nostrum opus*. (See Neugart. Codex Diplomaticus, CCCXLIX, and also Rilliet's "Origines de la Confédération Suisse," p. 401.) The first record, however, which contains anything from which we can infer any organized and independent movement among the people of Uri is of a later date. It is of the year 955. It is a record in the archives of Zurich, given, however, in full in the appendix to Rilliet's "Origines." The inhabitants of Uri (*inhabitantibus Uroniam*), the dependents and tenants of the Fraumünster at Zurich, resist a proposed increase in their rents and dues, on the ground that it was for them, the people of Uri, to decide what additional rents and dues might be levied. It was a matter to be decided according to custom and law (*patrum nostrorum jure et lege*), not by the authority of the Abbess or her advocate. The custom or law referred to is probably that which we find in the Lex Alamannorum (Tit. XIII). The freemen upon the estates of the Church, *quos colonos vocant*, paid to the Church the same rents and dues that the freemen upon the estates of the king paid to him.

You see that there is really no evidence in the document of 955 to establish the existence of communal or democratic institutions in Uri. All that we can infer from it is, that there was in Uri a

group of men who had, by law, a certain right, which in the year 955 they were able to maintain against usurpation. (Compare Rilliet, p. 37.) It was not until the thirteenth century that Uri obtained what could be called political independence, and it was not until afterwards that the people had the right to choose their own magistrate, or *Landammann*. (Compare Bluntschli, Geschichte des Schweizerischen Bundesrechts, I, p. 28.) But I will grant, for the sake of argument, that the democracy of the Swiss Cantons may be traced back to the year 955, not in its present developed form, but somehow or other as Mr. Freeman would have it. (See appendix of notes to the Essay on the English Constitution.)

If the earliest record in the history of communal or democratic institutions in the Alps dates in the year 955—and regarding the other Cantons beside Uri there are no records of so early a date from which we can infer any independent organization among the lower classes—if, as I say, the earliest record in the history of communal or democratic institutions in the Alps dates in the year 955, the middle of the tenth century, and the account of the Germans of Tacitus was written at the beginning of the second century, and there is no evidence to establish the existence of democratic institutions during the intermediate time, there is a gap of eight and a half centuries in the history of those institutions; and you will agree with me, I am sure, when I say that their continuity from the time of Tacitus to the present day cannot be established.

Mr. Freeman is inclined to admit, in his "Norman Conquest" (Cap. III, § 2, note), that in all but "a few of the smallest Swiss Cantons, and in several of these, the ancient freedom had to be reconquered, and was not uninterruptedly retained." He says that this was "altogether so" in Glarus and Appenzell and "in Uri to some extent." He does not tell us where it was not so. Still he insists that the democracy in the Alps, if not a survival, is at least a revival, of the democracy which existed among the Germans in the time of Tacitus. And this seems to be Mr. Freeman's theory, divested of rhetorical exaggeration and reduced to simplest statement.

Granting that the political constitution of early Teutonic society was democratic, Mr. Freeman's argument is very weak, owing to the lapse of at least eight and a half centuries, probably more

than twelve centuries, during which we have no records to show, either that the democracy of early times was surviving, or that the traditions of it were being preserved. If, however, the political constitution of early Teutonic society was not democratic, not at all like the constitution of the democratic Cantons of Switzerland, Mr. Freeman's theory will deserve no support, nor any further consideration.

I shall now proceed to lay before you, in as good order as I can and as briefly, the facts which we have which relate to the political organization of early Teutonic society. We must find out whether it was in any respect democratic, or not; we must find out, if we can, precisely what it was.

The first account we have of the Germans, at least the first account from which we learn anything with regard to the organization of society among them, is found in two or three chapters of Cæsar's Commentary "de Bello Gallico." The facts contained in these chapters are few and bare enough, but we have them from an original witness, and one of the highest character. Cæsar describes as the prime constituents of Teutonic society two classes of persons,—the chieftains, or *principes*, and the people; *gentibus cognationibusque hominum qui una coierunt*. In any nation of Germans there were numbers of chieftains, and the mass of the people were grouped under their leadership. We learn that in time of peace there was no common ruler over the chieftains, but each governed his followers independently. Cæsar says of them (B. G., VI, 23): *inter suos jus dicunt controversiasque minuunt*. All this agrees with what Baed says of the Saxons (Hist. Eccl., V, 10). They had no king, but a great many rulers instead; *satrabas plurimos suae genti praepositos*. And what was true of the Germans in general in the time of Cæsar, and of the Saxons according to Baed, was true also of the Lombards after the death of King Kleph, at the end of the sixth century. As Paul the Deacon tells us in his History (II, 31): For ten years the Lombards had no king, but were governed by their dukes alone, and each of these ruled over his own city. In all there were thirty-six of them. Much more evidence could be adduced to show that in early times there were many chieftains in each nation of the Germans, and, when they had no common ruler, no king or chief of chieftains, they ruled over their respective followers independently.

We know, however, that there were general or national assemblies in which the chieftains with their respective followers met together; and Cæsar tells us that in assemblies of this kind one of the chieftains would rise and propose an expedition, and invite others to join him in it, and those who approved of the man, and of the expedition he proposed, rose and promised to follow and obey. This invitation to join in expeditions was not extended to the mass of the common people; for, as Cæsar tells us (B. G., IV, 1), they were divided each year into two bands, and while one was led to war, the other was obliged to remain at home to superintend the cultivation of land, to provide the means of subsistence for themselves and those who were absent. *Sic neque agricultura nec ratio atque usus belli intermittitur.* There were many chieftains in each nation of the Germans, and they were constantly engaged in military expeditions, now under one leader, now under another. When they were starting upon their expeditions they divided their respective followers into two bands, one of which they took with them, while they left the other at home. This is what we gather from the testimony of Cæsar.

Now we want to find out, if we can, whether the office of chieftain was elective or hereditary; and, as Cæsar has nothing to say about that, we will call up Tacitus. He tells us in his "Germania" (Cap. 13), that the right to be a chieftain was inherent in those persons who were of noble birth or distinguished parentage; *insignis nobilitas aut magna patrum merita principis dignationem etiam adulescentulis adsignant.* From which it appears that the right to be a chieftain was hereditary. This conclusion is supported by the concurrent testimony of the later records. Then Tacitus goes on to say, that the young chieftains were associated with those who were older, who had more experience than they; *robustioribus ac jam pridem probatis;* and, he adds, they were not ashamed, although they were chieftains, to be seen among the followers, *inter comites.* Then we have a description of the following of a chieftain, his *comitatus:* from which it appears that the freemen who did not attain to the dignity of chieftainship were ranked in the following of one or another chieftain. And Tacitus tells us that there was a great emulation among the chieftains to get and retain the greatest number of devoted adherents; *cui plurimi et acerrimi comites.* The question arises, How did the chieftains get these adherents? Evidently, from all we can

gather from Tacitus as well as from later sources of information, by individual choice prompted by individual preference on the part of the freemen. The freeman selected among all the chieftains the one he preferred to follow and obey; came to him with his arms, *cum arma sua*, and took the oath of allegiance; *Trustem et fidelitatem conjuravit* (see old formula, Rosière, VIII). This is the sort of election that obtained among the Germans, and it is not inconsistent with the hereditary character of their chieftainship. The chieftainship of the early Germans was hereditary and also elective; not elective, however, in the modern fashion, by majorities of the electors, but by their individual choice prompted by their individual preference. The chieftains were elected by divisions among the electors. Drop a number of magnets of different size and power among a lot of iron tacks, and you will see an election of the magnets by the tacks upon the old Teutonic principle. There were many chieftains in each nation, and among them the freemen were at liberty to choose those whom they individually preferred to follow and obey. The result of this was that the freemen were constantly separating into groups, and each group had its chieftain; and the size of the group varied according to the attractive force of its leader. If he was particularly noble by birth, rich in land or stock, or distinguished for courage or success, he was sure to have many followers. If he was not particularly noble, nor rich, nor distinguished, he had very few.

This interpretation of the testimony of Tacitus is quite different from that which has been generally adopted. The prevailing opinion is, that, according to the account of Tacitus, there were chieftains who were elected upon democratic principles, who acted as representatives of the people; and each of them had about him a band of personal followers, as a sort of body guard. As Canon Stubbs says (Doc. Ill. of Engl. Hist., Introd., p. 3): The villages and cantons were governed by the *principes* appointed by the nation in its popular assembly. . . . . and the *principes* had the privilege of being attended by a train of *comites*, who fought for them in battle, waited upon them in peace, and regarded the honor of association with them as more than a compensation for such diminution of freedom as the relation of patron and dependent involved. The text of the Germania does not, however, admit of any such interpretation; nor is it consistent with the testimony of the later records.

The conclusion we reach then is, that there were two classes of freemen,—those who had the right to be chieftains and those who were the personal followers and companions of the chieftains. The right to be a chieftain was hereditary; but those who had not this right were at liberty to choose among the chieftains those whom they personally preferred. Although the office of chieftain was hereditary, it did not *per se* involve the adherence of followers. The chieftainship was hereditary, but followers were not inherited with it. The adherence of the followers was in all cases personal. It was also voluntary.

This last statement must not be made, however, without a qualification. The adherence of the followers was in all cases personal, and it was voluntary unless the followers were dependents. We must divide the followers into two classes,—free followers and dependents.

We might infer, from what Tacitus tells us in his fourteenth chapter, that the followers were regularly dependent upon their chieftains for food and arms; but I cannot regard the statements in the chapter as strictly correct; for, in Chapter 13, Tacitus tells us that some of the followers got their arms from their fathers or kinsmen, as if by inheritance; and, in Chapter 20, he speaks of a law of inheritance, according to which all property was equally divided among sons or nearest kinsmen. Therefore some of the followers, probably many of them, had wealth of their own, and were not necessarily dependent upon their chieftains. There were two classes of followers,—those who were dependent upon their chieftains for food and arms, and those who had their own means of subsistence and self-defence; and it made a great difference in that early society whether a man belonged to one class or the other.

Tacitus tells us (Germ., Cap. 44) in a passage seldom quoted, but a passage that seems to me very significant, that among the Suiones, the Germans of the North, wealth was greatly esteemed, and that it followed from that that the chieftains could govern with no doubtful claim to obedience: *est apud illos et opibus honos, eoque unus imperitat, nullis iam exceptionibus, non precario iure parendi.* Compare the capitulary of the year 813; where it is written (XVI), that no one was free to leave his chieftain (*senior*) after receiving from him the equivalent of one *solidus;* unless his chieftain attempted to slay him, or beat him with a stick, or

attempted to dishonor his wife or daughter, or sought to deprive him of his inheritance.

The Visigothic Law (V, III, 1) reads as follows: If any one give his follower arms or anything else, and he, the follower, desires to desert his patron to choose another, he must first restore to him everything received by way of benefice. We see from this that if the Tacitean *comes* got his arms from his chieftain, he was not free to desert him until he gave them back. According to the ancient custom of the Lombards, as we learn from a capitulary of the year 793, a freeman could commend himself to whomsoever he pleased; *De liberis hominibus Langobardis ut licentiam habeant se commendandi ubi voluerint.* So in the Edict of Rothar, the Lombard king (Sec. CLXXVII), "A freeman can go where he pleases with his *fara* [*familia pecuniaque*], with the king's permission; but if he has received anything from his chieftain, or any one else, he must restore that to him or to his heirs." The phrase of Domesday Book (I, p. 175), *si ita liber homo est ut habeat sacam suam et cum terra sua possit ire quo voluerit*, is significant in this connection.

These and other similar records testify, that among the followers there was a class of dependent persons, who got their arms and the means of subsistence from their respective chieftains; that they were bound in consequence of this indebtedness, not only to remain with their patrons, but to perform for them such services as were consistent with their rank or dignity. Compare the capitulary of the year 815, Cap. VI. If a man was full-free, he was not to do, or, rather, should not be required to do, any servile work. Compare the capitulary of the year 793: *De liberis hominibus ad servilia non cogendis;* and I may refer you also to the Sirmondi Formula, No. 44, to the words, *ingenuile ordine tibi servitium vel obsequium impendere debeam.*

You see that the freemen were divided into two classes,—the chieftains and their personal followers; and you see that the personal followers of the chieftains were divided into two classes,—those whose adherence was voluntary, who could desert their chieftains *ad libitum*, and those who were bound in dependence, who were not at liberty to desert their chieftains. These were the prime factors in the organization of early Teutonic society.

The clans and nations of men were confederations of more or less feudalized states, each under its chieftain; and a general or

national assembly was a meeting of the chieftains, accompanied by their personal followers, voluntary and dependent.

Looking at the account of a general or national assembly among the early Germans, the account that Tacitus gives us in the eleventh chapter of his "Germania," and explaining it by the aid of modern political facts and modern political theories, we are inclined to say that the chieftains were the representative magistrates of the people. We are inclined to describe the assembly as democratic in character. But the modern point of view is not the only point of view, it is not the right point of view.

Do we not read in the Law of Athelstan, the Anglo-Saxon king, (Ord. 2,) that every man had a lord in the *folkmoot?* Canon Stubbs, Mr. Freeman, and most of the German writers, would say that this came to pass in course of time. But do we not learn from Tacitus what we learn from this law of Athelstan? He tells us that those who were not chieftains took their choice among those who were, and became their personal followers. It appears, therefore, that the *folkmoot*, or popular assembly, in the time of Tacitus, was—what it was in the time of Athelstan—an assembly of chieftains accompanied by their personal adherents.

The philologists tells us that the German word *König*, our *king*, corresponds with the Sanskrit *ganaka;* and this signifies the father of a family. (See Max Müller, Science of Language, II, 284.) This is a text for us, pointing to the fact that the kingship or chieftainship of early times is rooted in the idea of paternity and paternal authority. The authority of the king or chieftain is primarily the authority of the father or the patriarch: and we may say that, among the branches of the Aryan race at least, the tap-root in the history of institutions is the *patria potestas.* It was an absolute authority, not over wife and child simply, but over followers in dependence, and slaves in servitude. The father of the family was its sovereign ruler; and the clans and the nations of men were associations of families bound together by the blood relationship of their heads. This blood relationship was the sole bond of their confederation; and we have what Mr. Grote has very happily described as the omnipotence of private force, tempered and guided by family sympathies. In the course of time, however, certain families became eminent above others by reason of their success in the struggle for existence. The movements of these families at first determined, and afterwards governed, those

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of all the other families with which they were in any way associated; and the members of such prominent families became the leaders and the rulers of their fellow-men, as their chieftains or their kings; and the authority of these chieftains or kings was the authority of heads of families, which was the only sort of authority known to that early society, passing beyond the limits of families over bands of personal and voluntary adherents. It was an absolute authority, tempered only by the independence and liberty of those over whom it was exerted. It was a sovereign authority struggling under adverse condition.

It was a sovereignty, limited by competition. There were numbers of chieftains in every nation, and the people were free to transfer their allegiance from one to another; the result of which was, that the competition among the chieftains to get and retain the greatest number of devoted adherents caused their authority over these adherents to sink to a minimum. The real character of that authority is revealed only in the course of time, when this competition ceased, in consequence of the spread of dependence, of settled life and local attachment; or as the result of agreement among the chieftains in opposition to the independence and liberty of those whom they governed.

In the year 587, the Frankish kings Guntram and Childebert agreed that they would not attempt to allure one another's followers from their allegiance. They would neither persuade them to desert, nor receive them if they did so: *similiter convenit ut nullus alterius leudes nec sollicitet nec venientes excipiat.* As if our capitalists should agree not to solicit one another's workmen, nor to employ them should they come seeking labor. As if our great land-owners should agree not to receive one another's tenants, should they present themselves and ask for leases of land. If such events should come to pass, the power of the capitalist and the landlord would be revealed to our amazement. So it was with the kings and chieftains of early times. So long as the competition among them to obtain the greatest number of devoted adherents was unchecked, their authority was kept at a minimum. But when this competition ceased, great powers, which had been latent before, became apparent to the world in the feudal monarchy of the Middle Ages.

The aim of the student who pretends to work in a scientific way is first to attain a point of view embracing all ascertainable

facts in their natural and true relationship, and then to lead others to that point of view. So now, if you have followed me through my argument, and if I have led you to comprehend the facts we have which relate to the political organization of early Teutonic society, we are prepared to reconsider together Mr. Freeman's picture of free institutions in the Swiss Cantons, and we are prepared to declare at once that it will not serve to describe the political organization of Teutonic society as revealed in the earliest accounts and records.

In early times the office of king or chieftain was hereditary. The office of magistrate in the Swiss commonwealth is elective. The elections take place periodically, and are accomplished by the concurrence and agreement of a majority of the electors. The early kings and chieftains were also elected; not, however, by majorities of the electors, but by divisions among them. Every freeman, who was not a dependent, chose among the kings or chieftains the one whom he personally preferred, and remained with him, under his leadership and government, as long as he pleased, and no longer. The government of the early kings and chieftains was paternal or monarchical, in early times rather paternal than monarchical, in later times the reverse of this. In either case the authority emanated from the persons who exercised it, and not from the persons over whom it was exercised. In the Swiss commonwealth, on the contrary, the chief magistrate is the representative of the people. It is their sovereignty, and not his own, that he administers. And as to the law; in early times it was the voice of the fathers, of the chieftains, of the kings, while in the modern democracy it is the voice of the people as a legislative body.

In no respect do we discover any resemblance between the constitution of the democratic Cantons of Switzerland and the political organization of early Teutonic society: and it follows that the constituting of the Swiss commonwealth is not an archaic political institution that has survived to our day; but an institution which has come into being within comparatively recent times. Its origin is not prehistoric, not archaic, but mediæval. Particularly what it was will be a subject for future investigation.

THE ANTIQUITY OF MAN IN EASTERN AMERICA, GEOLOGICALLY CONSIDERED. By HENRY CARVILL LEWIS, of Germantown, Pa.

[ABSTRACT.]

IN the course of an investigation of the surface geology of southeast Pennsylvania, the writer has determined some facts regarding one of the gravels, which, bearing directly upon the antiquity of man in America, become of interest. In former papers the writer has shown that the gravels of the Delaware valley belong to several distinct ages; and if, therefore, at any place the remains of man are shown to occur, it will be important to know to which of these gravels they should be referred.

The surface formations of southeast Pennsylvania may be provisionally classified as five clays and four gravels. These are, beginning with the oldest: (1) *Jurasso-cretaceous* plastic clay, seen at Turkey Hill, Bucks Co.; (2) Tertiary clays of the "*Brandon Period*," associated with the iron ore, kaolin and lignite of the Montgomery county valley; (3) "*Bryn Mawr gravel*," a high level gravel often found at elevations of four hundred feet, characterized by the presence of an iron conglomerate and of pebbles of Potsdam, but never of Triassic rocks, and conjectured to be late Tertiary; (4) "*Branchtown clay*" of probably similar age; (5) "*Glassboro' gravel*," of latest Pleiocene age, found also on the watershed in New Jersey, between the Atlantic and the Delaware, and known by its pebbles of Niagara limestone and of other fossiliferous rocks; (6) "*Philadelphia red gravel*," of Champlain age, which contains numerous boulders of all materials, fragments of Triassic rocks, etc., which shows flow-and-plunge structure and wave action on a large scale; which rests on a decomposed gneiss, and which is confined to the river valley; (7) "*Philadelphia brick clay*," which, with its boulders, rests upon the last, and, like it, appears to have been deposited by the waters of the melting northern glacier; (8) "*Trenton gravel*," a sandy river gravel forming the bed of the Delaware; (9) the modern *alluvial mud* now forming in the tidewater swamps.

Of these formations, one of the least conspicuous at Philadelphia is that now called the Trenton gravel. It is a true river gravel, rising here but a few feet above the water, and forming a quicksand when below water level. It is of a gray color, and contains pebbles composed entirely of the rocks which form the upper valley of the river. Unlike older gravels, it has very few quartz pebbles, and its pebbles are generally flat. In the middle of the river at

Philadelphia it is one hundred feet deep. On tracing this gravel up the Delaware it is found to rise higher above the river and to extend farther back from it as we proceed up stream. Thus, at Bristol it extends two miles back from the river, and is bounded by a well-marked hill, upon which rest the older gravels. At Trenton, the limit of tidewater, the narrow upland portion of the valley begins; and from there up this gravel is shallow, and confined to the river bed. The oceanic gravels trend across New Jersey, and are no more seen. Two surface formations alone remain—the river gravel of post-glacial age, and the brickclay, with its boulders, of Champlain age. The first lies within the last, and both can be traced up to the great terminal moraine which crosses the river at Belvidere. It is to be especially noted that the Trenton gravel is newer than a drift of Champlain age. It is in this Trenton gravel, and in this gravel only, that traces of man are found.

The Trenton gravel, at the locality which gives it its name, is remarkably well exposed. Trenton is at the point where a long narrow valley with continuous downward slope opens out into a wide alluvial plain, and where the rocky floor of the river suddenly descends below ocean level. It is here that the bulk of a gravel, swept down the upper valley, would, on meeting tidewater, stop in its course, and with its boulders be heaped up in a mass, immediately afterward to be cut through by the river. It was thus that a cliff of gravel fifty feet high was here formed, the river having cut through the gravel instead of flowing upon it, as at Philadelphia. This explanation dispenses with the necessity of assuming, as some geologists have done, the submergence of the land by the ocean at the time of the deposition of the gravel. That southern New Jersey was at that time dry land is shown by the fact that this gravel at Trenton extends inland a few miles only, and, having filled up a bay in the ancient flooded river, is bounded by hills of the older gravel which covers the southern portion of the state.

There are many facts indicating that the Trenton gravel is a true river gravel and not a glacial moraine. The absence of glacial marks on the rocks, the stratified character of the gravel, the topography of its banks, the comparative amount of its erosion and the character of its materials, all point to the conclusion that it was deposited by a great flood of the river; and this, when taken in connection with the fact that it lies in many places within a channel cut through gravel deposited by the waters of the melting glacier, indicates a post-glacial or at least very late glacial age of the Trenton gravel.

The important bearing of this conclusion upon the antiquity of man on the Delaware, which, as will appear, depends directly upon the age of this gravel, is here apparent. Calculations based upon the erosive power of running water show that the time necessary for the river to cut through this gravel down to the rock need not have been long. On the other hand, no such flood as deposited this gravel has ever occurred within the historical epoch. No such large boulders are ever now carried down the river, and no modern rainstorms could cause such a flood. It is difficult to assign any other cause than that of a melting glacier. Since, however, it can be shown that the Philadelphia brickclay — a clay containing more and more boulders as it approaches the terminal, moraine — was formed by the melting of the glacier, we here find evidence of a second and more recent glacier in the Delaware valley, the melting of which may have given rise to the Trenton gravel.

The hypothesis of a *second glacial epoch* seems to explain all the facts observed. A similar period in Europe — the reindeer period — is supported by many facts. Should such a period not be traced in America, we must assume two periods of glacial melting, the one much more recent than the other. Evidence is accumulating to show that the final melting of the last glacier was not very remote.

The relics of man which occur in the Trenton gravel, and which were first found by Dr. C. C. Abbott, are of great interest. In shape, in size, in workmanship, and in material the implements here found are quite different from those used by the Red Indian. These "palæoliths" are embedded at various depths in undisturbed Trenton gravel. There are two points which offer strong evidence that they are as old as the gravel. The first is the fact that modern Indian implements ("neoliths"), although abundant on the surface, never occur more than a few inches below it, and are never associated with the palæoliths, which are found at depths of from five to forty feet below the surface. This fact alone argues a different age for the two classes of implements. The second fact is that, when found below the surface, the palæoliths always occur in the Trenton gravel and never in older gravels. The writer has gone over, with Dr. Abbott, much of the ground where the implements occur, and it was very interesting to find that it was only within the limits of the Trenton gravel, previously traced out by the writer, that Dr. Abbott had found implements below the surface. Here, then, is the strongest probability, even if the implements were found on the surface only, that they belonged to and were of coëval deposition with the river gravel.

The implements found in the river gravels of Europe are of the same type, though as a rule perhaps less rude. Similar implements have been used by the Eskimos, and it is probable that that race, now living in a climate and under conditions perhaps similar to those once existing in the Delaware, may have some kinship with the pre-Indian people of this river. The occurrence of bones of arctic animals and the absence of all mollusca in the Trenton gravel indicate a period of cold.

All the evidence now gathered points to the conclusion that at the time of the Trenton gravel flood, man, in a rude state, lived upon the ancient banks of the Delaware. If future archæological work can show a connection between this people and the Eskimos, it may be appropriate to call the period of the Trenton gravel and of this palæolithic people, a period perhaps following a second glacial age, the *Eskimo period*, a name more suggestive, and derived from a higher order of beings than that which gave the name "Reindeer Period."

While others have held that the occurrence of implements in the Trenton gravel indicates the existence of man in inter-glacial or even pre-glacial times, the writer believes that the investigations here described indicate the origin of man, at a time which, geologically considered, is recent. Neither in the Champlain deposits, in the morainic material of the north, nor in any older gravels have undoubted traces of man been discovered.

The actual age of the Trenton gravel, and the consequent antiquity of man in the Delaware, cannot be determined by geological data alone. It is the aim of this paper to define man's antiquity in relation to geological rather than to historical events. If, in showing that the period of the Trenton gravel is the last of the geological ages, it does not necessarily follow that it is by any means recent; it must be remembered, on the other hand, that its high antiquity is not proven by the facts thus far observed.

The conclusions to which the facts seem to point are summarized as follows:

1. That the Trenton gravel, the only gravel in which implements occur, is a true river deposit of post-glacial age, and the most recent of all the gravels of the Delaware valley.

2. That the palæoliths found in it really belong to and are a part of the gravel, and that they indicate the existence of man in a rude state at a time when the flooded river flowed on top of this gravel.

3. That the data obtained do not necessarily prove, geologically considered, a vast antiquity of man in Eastern America.

Ancient Mounds of Illinois. By William McAdams of Otterville, Illinois.

The State of Illinois, especially in the region about the mouth of the Illinois and Missouri Rivers, is remarkably rich in the remains of a pre-historic people. In a radius of fifty miles from the mouths of the above-named rivers there are thousands of mounds, representing nearly every variety found in the Union. The central geographical position and great natural resources of this region doubtless made it the great centre of our ancient population. As I have during the past twenty-five years explored a considerable number of mounds in the vicinity designated above, the result may not be uninteresting.

It is generally believed by the public, or used to be, that all these mounds of earth are old burial-places, and if one will dig down to the bottom of the structure he will find the bones of a human body, and such weapons and other things as were buried with it. This I have not found to be so. Many of you will no doubt be surprised to hear me state that I have dug in hundreds of mounds in this State without finding a single tomahawk or stone axe, and I am inclined to think that some of the mounds were constructed for other purposes than that of burial.

There is in this region a peculiar class of mounds that was for a long time a puzzle to me. They are usually found in groups of from two or three to twenty or thirty, and even more, and are generally on some pleasant knoll, or rising ground, in the vicinity of a spring or watercourse, especially in the vicinity of our prairies or level areas of land. These mounds are from one to three, and, in a few instances, even four feet in height, and from twenty to fifty feet in diameter. One mound of the group is always larger than the rest, and always occupies the commanding position. Sometimes the group is arranged in a circle; other groups have no apparent design in arrangement. Numbers of these mounds can be seen in the cultivated fields.

Although I have made excavations in them, and dug trenches entirely through them, I have found nothing but ashes, charcoal, decayed portions of the bones of fishes and animals partially burned, shells from the adjacent streams, flint chippings, and, in one or two instances, a flint implement of a rude character.

After examining many of these structures I am induced to believe that they are possibly the remains of ancient dwellings, made by placing in an upright position the trunks of young trees in a circle, or

in parallel rows, the tops of the poles inclining inward and fastened together, the whole being covered with earth and sod to form a roof, or in the same manner as many Indian tribes make their mud lodges, as, for instance, the Mandans and the Omahas. Such a structure, after being repaired from time to time by the addition of more earth on the top, would finally, by the decay of the poles, fall inward, and the ruins would form a slight mound.

Conant and Putnam describe such mounds in Missouri and Tennessee, some of the largest of these ancient towns being provided with streets and highways. They are also found in southern Illinois, Indiana, and Ohio. Putnam has described an enclosed town in Tennessee, in which were many low mounds, or rather, as he calls them, earth circles, that he has pretty conclusively shown to be the sites of the lodges or houses of the people.

In this vicinity there are also hundreds of burial mounds, and among them there is a marked difference, and they have been made, apparently, by different tribes or peoples, who have succeeded one another as occupants of the locality. The oldest of these are, perhaps, the simple mounds of earth, one to four feet in height, and twenty to thirty feet in diameter, measuring across the top.

These old mounds are as firm and compact as the undisturbed subsoil, and might not be considered artificial if it were not for the occasional finding of a tooth or a portion of decayed human bone. Sometimes nothing is found at all, except the discolored earth where ages ago a human body once lay.

Another kind of burial mound more numerous, and generally much larger, is oval or egg-shaped. These are mostly found near the river bluffs. A large mound of this class is situated on the farm of S. P. Dinsmore, two miles from the mouth of the Illinois River. It is one of a group of a dozen on the top of a sharp ridge, and is a conspicuous landmark. The top of the mound measures one hundred feet in length, and fifteen in width, and has a steep slope from the sides and ends to the base. It is about sixteen feet high. This mound was in a most perfect state of preservation until some two years ago, when Mr. Dinsmore invited me to be present at its examination. Using ploughs and road-scrapers, some two feet or more of the entire surface of the mound was taken off, when a number of human skeletons were revealed in a tolerable state of preservation, due no doubt to the peculiar character of the surrounding earth, together with the favorable position of the mound.

Near the head of one of the skeletons, at the western end of the mound, was found a large sea-shell, fashioned into a neat vessel. In this vessel were a number of bone instruments made from the leg-bones of birds, — probably the wild turkey. Each one of these bone implements had near its middle a spur larger than that of a chicken. About the neck of the skeleton were a number of small sea-shells, each of which was perforated, and they evidently had once formed a necklace. The skull of the skeleton we carefully preserved, and I have since given it to the Cambridge Museum.

At the western end of the mound, a few feet below the surface, another skeleton was found, with several implements of bone, a flint knife, a rude pipe, a large plate of mica, nearly ten inches square, and half an inch in thickness; also several pieces of lead ore, from one to six pounds in weight. One of the pieces of ore had been worked with smooth faces, apparently for an ornament. We have no evidence as yet that the mound-builders, or ancient inhabitants, had any knowledge of smelting ores.

An excavation near the middle of the mound, at the depth of fifteen feet, disclosed a basin of hard-burned clay, in which were dry ashes and the remains of another human skeleton, with implements of bone, and a large number of small perforated sea-shells, similar to those found near the top of the mound.

Another mound, a few hundred yards distant, was also examined. It was nearly conical in shape, ten feet high, and some thirty feet in diameter at the base. An excavation was carried down from the centre, and at the depth of eight feet a hard basin of clay was reached, and in the ashes was another curious vessel, made from an immense sea-shell. In the shell were portions of partially burned human bones mingled with ashes. This shell around its largest part measures not far from thirty inches.

From a number of other mounds of this kind in the county I have taken sea-shells, pipes, copper, and stone ornaments, but seldom any kind of a flint implement or weapon. Occasionally, an arrow point of rude manufacture is found with the remains, but in such cases I am inclined to believe they were in the body, and perhaps the cause of death. There are a great many mounds of this character in Calhoun County. Nearly all of them contain sea-shells, rude pipes, and bone implements.

There are considerable numbers of a peculiar kind of burial mound, differing but little from those just described, that also seem to have

been used for purposes of cremation. An altar or basin of clay was made on the surface of the ground, raised, in most instances, a couple of feet or more. This basin was sometimes round, and five or six feet in diameter; in other instances, it was elongated, and from ten to thirty or forty feet long. In this basin the body or bodies were placed, and, after being covered with combustible substances, the whole was burned, and when it had cooled, some of the ornaments of the deceased, with his pipe, were placed on the ashes. A large mound was then erected over the remains.

Sometimes the remains, consisting of the calcined bones, were placed in a large sea-shell; at other times, they were left on the ashes, and in two instances I have found such remains in large turtle-shells.

These cremation mounds are sometimes wholly of earth and sometimes partly of stone. One large mound of this kind, near the mouth of the Illinois River, is largely composed of stones mingled with earth. The mound seems to have been built up gradually by the cremation and burial of many bodies until the structure was finished. The altars, made of flat stones, lie one above another. Possibly this mound was used by a tribe or large family, on which they burned and buried their dead for generations.

Evidences in connection with some of these mounds have led me to believe that funeral rites of the most horrid description might have been sometimes practised. On opening a large earth mound in Calhoun County, I came upon the raised basin or altar in the centre, which showed unusual care in its erection, it being ornamented, apparently, with brilliant sheets of mica. The altar was burned nearly as hard as brick, and was filled with ashes, in which was a curious shell vessel containing ashes and partially burned pieces of human bones. On the ashes beside the vessel were a primitive pipe and some curious ornaments of stone. While I was examining these strange relics with an antiquarian's interest and delight, my companion, who had continued the excavation, came upon a second circle of burned clay, lower down, and surrounding the first altar. When this second structure was laid bare, the fact was revealed that a number of bodies had been burned upon it. No ornament of any kind was found on the lower circle. After a season of thought we were constrained to believe that, possibly, we had opened the tomb of some ancient savage ruler, whose funeral obsequies had been celebrated with great ceremony; and when his body was cremated on the altar of clay, human sacrifices were also made, that his servants might accompany him to the happy hunting-grounds in the realms of the sun.

It is thought by some that the later Indians or Red Men, with whom the whites came in contact, sometimes erected small mounds, but the custom certainly was not general among them, and there are few well-authenticated accounts of white men ever actually seeing the modern Indians erecting a mound, either over their dead or for other purposes. Yet there are very many burial-places in this vicinity that might be ascribed to the later Indians. Their favorite and common places of burial were, no doubt, the high points along the river bluffs, — natural mounds in the soft, marly earth, in which it was easy to dig their shallow graves. The pioneer white settlers state that the Indians sometimes brought their dead from distant points to bury them on some favorite prominence. Sometimes in ploughing these knolls, at the present day, the remains are exposed, so shallow have they been buried, and there is no doubt that mounds already made were often used as burial places by the modern Indian. There is, in fact, scarcely a mound near the mouth of the Illinois River but what, if you attempt its exploration, the very first time, perhaps, the spade is sunk in the soft loess, you will have evidence of an intrusive burial, and one is almost startled to turn out suddenly a smooth, well-preserved Indian cranium; and you have to dig some distance below the intrusive burial to find the ancient and primary deposit over which the mound was built; and one is sometimes greatly surprised, if he should be so fortunate as to find a skull at the base of the mound, to see how different it is from the one first discovered.

Sepulchral mounds, such as were made by the people who erected the great mounds and earthworks of Ohio and elsewhere, are also found in this vicinity. There are a few in the low lands of Calhoun, Green, Scott, and Pike Counties, and across the river in St. Charles County, Mo., but in the American Bottom, opposite the mouth of the Missouri River, below Alton, they are numerous, and of the grandest proportions.

One which I measured, three miles from East St. Louis, is forty feet in height and seventy yards long at the base. It is quite perfect, and at a distance resembles a huge rick of hay. In some of the large fields in this vicinity dozens of them can be seen like ricks of hay in a meadow.

One large mound of this description, but different in shape at the base, has been nearly removed by the Chicago, Alton, & St. Louis and other railways at a station called Mitchel, on Long Lake, ten miles this side of St. Louis. The mound was a hundred paces long,

very nearly as wide, and between thirty and forty feet in height. Several railroad tracks go through either side, and only a portion of the centre, from north to south, remains, giving the investigator a fine opportunity to determine from its homogeneous structure its artificial origin. Many curious and interesting relics, once the property of the ancient builders, were taken from this mound by the workmen, and are now in my possession. Near the base of this mound the workmen came upon a skeleton with a wrapping of matting made from woven grass. With the body had been interred a large number of copper implements and ornaments. The copper had preserved portions of the matting, together with portions of the head of a buffalo, including the teeth. This is the first instance, I believe, on record, in which the remains of a buffalo have been found in an ancient mound.

Mounds, such as here described, in the American Bottom and in the low lands of Illinois, are seldom, if ever, found on the bluffs. On the rich bottom lands of the Illinois River, within fifty miles of its mouth, I have seen great numbers of them, and examined several. The people who built them were probably connected with the Ohio mound-builders, although in this vicinity they seem not to have made many earthen embankments, or walls, enclosing areas of land as is common in Ohio. Their manner of burial was similar to the Ohio mound-builders, however, and in this particular they had customs similar to the mound-builders of Europe.

Many of these mounds have yielded the richest relics, some of which are very peculiar, and are calculated to throw great light on the manners and customs of that ancient people. Implements of stone and copper are very common in the mound-builder mounds of the West. Those of Illinois differ but little from those of Ohio, and I shall not enter into any description of them in detail excepting the pipes, of which, in Illinois, we have three distinct types. Those peculiar pipes, with the crescent base, and characteristic of the mound-builders, are found in this vicinity in great perfection. In fact, some of the finest mound-builder pipes we have ever seen have been taken from mounds in Illinois.

Among all the ancient relics the pipe seems to indicate best the former condition of its owner. That peculiar people who built the earth works and the great mounds disclosed a higher taste and a much greater skill in the manufacture of their pipes than any other prehistoric people of this country. They had great genius for delin-

eation, and, with wonderful patience and skill, carved from rock imitations of animals and birds and other objects of nature, which, when finished, served them for a pipe.

The mound-builders' pipes were mostly made from catlinite, which is not a very hard rock. I have never seen a pipe made from porphyry or any of the varieties of granite. It is probable that a rock harder than limestone was seldom used for the manufacture of pipes.

There is another class of mounds, of which we have numbers. They are generally situated on the elevated bank of some lake or stream, and are usually not more than a foot or two above the surrounding level. In some of these burial places no elevation is perceptible, and they are simply cemeteries, where hundreds of persons were buried so close together that graves made previously have been intruded upon.

I call them pottery-mounds from the fact that in nearly all the graves vessels of pottery have been deposited. It seems to have been the custom of this people to deposit, in the grave with their dead, one and oftentimes two earthen vessels, one containing food and the other liquid, probably water.

Nearly all the food vessels contain one or two valves of a shell, *Unio*, together with a dark mould. In the mould in the bottom of these vessels I have found carbonized corn. Many of these earthen vessels are nicely made, and indicate a high degree of knowledge of the ceramic art, as well as the imitative genius of that people.

As the mound-builder imitated nature in his pipes, so the pottery-maker copied after nature in the manufacture of his earthen vessels, which were shaped to represent birds, beasts, fish, human beings, and many other objects that happened to strike his fancy. Some of these vessels are made to represent objects with great fidelity; while others, especially of the human form, are ridiculous caricatures. Some time ago, while exploring in one of these mounds, I suddenly came upon a huge tortoise, which I sent with some force from the shovel, and unfortunately it struck a tree, and broke into fragments, and revealed our great loss. The tortoise was a perfect gem of ceramic art. In the same mound I came near casting to one side the valve of a shell, which proved to be a perfect and beautiful piece of earthenware.

Some of the long-necked water-bottles from the pottery mounds strongly resemble in shape those of ancient Egypt, while others resemble the ancient water-vessels of Peru. These earthen vessels are made of a compound of which clay is the principal part, and appear to have been baked or hardened by fire. We have seen some of these

vessels that still show traces of bright-colored pigments, with which they had been painted. Other vessels are ornamented with scroll work and lines arranged with taste.

The pottery-makers, like the mound-builders, were evidently great smokers; but they made their pipes in a different shape. I have yet to see a single pipe from a pottery mound with the curved base forming the mouthpiece so characteristic of the mound-builder. The pipes found with the pottery are almost invariably made for the insertion of a stem; the hole for which is funnel-shaped, like that of the bowl for the tobacco.

Some of these pipes are of large size, and look more like idols or heathen images than pipes for smoking. I have two curious pipes of this kind that represent human beings of hideous countenance, and holding up before them a perfect representation of one of their characteristic vessels. The vessels form the bowl of the pipe, while the place for the insertion of the stem is in the back of the image. Putnam figures a pipe of this same design, cut out of steatite, from a mound in Tennessee.

The pipes from the pottery-mounds are generally made of stone, the majority of them being catlinite. I have them also made of limestone, sandstone, and clay.

The third type of pipes which I recognize are those commonly used by our modern red men. In this pipe the hole for the insertion of the stem, instead of being funnel-shaped like the pottery-maker's pipes, is generally a cylindrical hole, of uniform size its whole length.

Another class of mounds of an exceedingly interesting character have been called "Temple Mounds," and are distinguished by their great regularity and generally large dimensions. They are chiefly pyramidal structures, truncated, and generally have graded avenues to their tops. In some instances they are terraced. But, whatever their form, — whether round, oval, square, or octangular, — they have invariably flat or level tops. Antiquarians generally agree in believing that the summits of these structures were crowned with temples, which were constructed of wood and perishable materials.

The best representative of this class of structures on this continent is to be seen in the stupendous mound on the bank of the Cahokia Creek, four miles from the bridge approach in East St. Louis. This great work has been often described, and I need only allude to it in this connection.*

* For a recent account and figure, see Putnam, 12th Report Peabody Museum, p. 470.

There is still another class of mounds, found in the American bottom, and elsewhere in the rich alluvial bottoms of the Mississippi, as well as occasionally in Ohio. They are called platform mounds, and are usually square in shape, with a level surface, and from two to thirty or forty feet in height. They are more numerous and of larger proportions in the American bottom, opposite the mouth of the Missouri River. From the elevated position on the top of the great temple mound on the Cahokia Creek, many of the square platforms can be seen; some of them quite near the base of the great mound, others far out on the plain. Numbers of these platforms are utilized by the people in the American bottom for building sites, for which purpose they answer excellently.

In excavating in these platform mounds for cellars, wells, foundation walls, etc., great quantities of curious relics of human handiwork are met with, showing they certainly were used for sepulchral purposes; yet some archæologists are of the opinion that the broad, level tops of these mounds were used for other purposes.

In addition to the different kinds of mounds described, we probably have some others in this section, such as signal mounds and treaty mounds; but they have as yet yielded little or nothing of interest. The subject is not by any means exhausted; and, as I propose to continue my explorations, something more of interest may be found.

There are, in various parts of the United States, extensive earthworks, consisting generally of walls of clay enclosing considerable areas of land. In Ohio, Kentucky, Indiana, Illinois, and Missouri they abound; and they are met with in Wisconsin, and still farther west.

It is a little singular, that, although the Great American Bottom contains such a grand array, both in size and number, of the ancient mounds, I know of but a single embankment or enclosure; and that was discovered a short time ago in the county of Calhoun, Ill., near the mouth of the Illinois River.

Ancient Agricultural Implements of Stone. By William McAdams, of Otterville, Ill.

Among the most interesting relics of ancient people, who in the past inhabited this continent, are their implements used in agriculture.

They tilled the soil, and no doubt raised considerable quantities of corn and vegetables.

In the great alluvial bottoms, and on the rich soil of the prairie adjacent to the streams, in a region near the mouths of the Missouri and Illinois Rivers, many of these implements are found, showing that in prehistoric times the cultivation of the soil was carried on to a considerable extent.

These implements are generally made of stone of a silicious character, which is found in the vicinity. They are mostly of a red or brown color, but we have seen some very fine specimens of perfectly white chert. They are usually ovoid in shape, and sharp at the broader end, which is generally polished by long use and attrition with the soil. Their usual length is from eight to ten inches. We have specimens twelve to fourteen inches long and six inches broad. They range from one half to one inch in thickness. Some of these implements taper gradually from the broad cutting edge, while others have deep lateral notches opposite the working edge, and form perfect hoes. These were apparently fastened to handles, and formed effective implements, similar to our modern hoe. Others were plainly intended to be fastened to handles in a similar manner to our spades and shovels. Others still were probably fastened to some sort of frame or stock, as the peculiar way in which these stones are worn shows that they were held in the manner of a plough, and pulled through the ground. This, without doubt, was our prehistoric plough; and, as we have no evidence that any domestic animals were used in this connection, it is probable that the implement was pulled by two persons, and guided from behind by a third.

This mode of cultivating crops on the rich, sandy, alluvial soils of this region would have been much more speedy and effectual than the hoe, and doubtless was practised by the ancient mound-builders and pottery-makers.

While we seem to have made no discovery as yet that these ancient people domesticated the buffalo, yet we have nothing to actually prove the contrary. That they were acquainted with the buffalo we have no doubt. In one of the large mounds in the Great American Bottom, at the depth of nearly forty feet, we found the skull of a buffalo. The teeth were perfect, having been preserved by being in contact with a number of copper implements, belonging, apparently, to the age of the old mound-builders. In two other instances we have found in large mounds the hair and skin of the buffalo preserved by being in contact

with copper. The majority of these ancient implements of husbandry are made after definite patterns, each variety or kind to be used for special purposes being similar. Of six of the deeply notched hoes, from different localities, one could discern little difference excepting in size. The broad-edged spades, the oval shovels, and the ploughs have apparently been made after certain patterns, the form of which was followed with great faithfulness.

That such neat and efficient implements should have been made of such obdurate material as flint and jasper shows no inconsiderable skill. These stone implements are generally found in our cultivated fields, where they have been turned up by the plough. On several occasions, however, we have found them in the graves of the ancient pottery-makers. From one of these burial-places in St. Charles County, Mo., I recovered two quite perfect earthen vessels, one of which had evidently, from its shape, been used to hold a liquid. In the other was a mussel shell, *Unio*, and some black mould, in which were carbonized grains of corn. Near the vessels was a pipe characteristic of the pottery-makers, and a nicely finished spade, such as we have described. That the mound-builders made and used agricultural implements of stone is highly probable, but we have not found them in their mounds. The fact of the finding of several of these implements in the later pottery-makers' graves seems to point to them as the manufacturers.

A Stone Implement from a Well in Illinois. By William McAdams of Otterville, Illinois.

Last year an old gentleman from Green County, Ill., came to my house and told a curious story of the finding of a stone axe in the base of the drift while digging a well. It seems that, during the dry weather in the fall, Mr. Abraham Young undertook to dig a well on his farm, and near his residence.

At the depth of seventy-two feet the rock in place was reached, and in cleaning off this, preparatory to blasting, the axe in question was found. Thinking from the character of the parties, who are well-to-do farmers, and known to me, that possibly there might be some truth in the matter, I visited the locality, obtained the relic, and the following information in relation to its discovery: Four wells have been dug at

different times in fruitless search for water. The depth of these wells is given as from thirty to seventy-two feet. The one in which the axe is claimed to have been found is seventy-two feet in depth. I examined the earth thrown out from the well, and could easily see that it was almost entirely loess. Just before the bottom of the well, or rock, was reached, the loess assumed more of a clayey character, and contained angular fragments of chert, not apparently water-worn. Lying on the surface of the rock, covered with clay and fragments of chert, the axe is claimed to have been found.

The finders of the relic seemed not to have much interest in the matter, and would receive no compensation for it, and when I wrote out a statement of the time and manner of the discovery they signed it without hesitation, and apparently in good faith. Afterwards they went of their own accord before a justice, and made the following affidavit: —

GREEN COUNTY, ILL., Feb. 1, 1880.

Be it known by these presents that J. R. Cade, A. B. Young, David Mateson, and Rowel Hunnicutt, all of Walkerville Township, County of Green, State of Illinois, appeared before me, John Painter, a Justice of the Peace in Green County, Ill., and, after being duly sworn, said parties state that on or about the 20th of May, 1879, while engaged in digging a well on the farm of A. B. Young, in Green County, Ill., at the depth of seventy-two feet, J. R. Cade found, while digging, an ancient stone axe, which was raised from said well by A. B. Young and David Mateson, and preserved for curiosity.

Rowel Hunnicutt states that he was present a short time after said discovery, and measured the depth of the well and found it to be about seventy-two feet to where the axe was found, and that he took said axe in his possession and placed the same in possession of one William McAdams, of Jersey County, Ill.

Signed, J. R. CADE,
A. B. YOUNG,
DAVID MATESON,
ROWEL HUNNICUTT.

Subscribed and sworn to before me this 1st day of February, 1880.

JOHN PAINTER, J. P.

I examined the well closely, to see if by any possible means the axe could have fallen in from the top while they were at work, but, from the very top down, the excavation was as smooth and almost as hard as a cemented cistern; and the discoverer of the relic says he found it covered with the hard, undisturbed clay on the rock.

I am inclined to believe they found the axe as stated.

I asked the men how they supposed the axe came to be in that position, and one of them replied that he could think of only one possible way by which its presence could be accounted for where they found it: "Some half-mile north of the well was a sink hole; possibly the axe might have been washed into this, and conveyed by some channel or watercourse to where it was found."

Prehistoric Altars of Whitesides County, Illinois. By W. C. Holbrook, of Coleta, Ill.

Four structures known as altars have been found in Whitesides County; two on the north bank of Rock River, near Sterling, and two near Rock Creek, in Clyde. The former were each situated near groups of burial mounds, while the latter were at least three miles distant from any prehistoric works.

These altars were built of flat limestones carefully fitted together. The edges of some of the pieces had been dressed or chipped off; but no cement was used. Their form was either that of a circle or an ellipse. One in Carroll County was, I am told, a crescent like the moon when about eight days old; but no rectangular structure has ever been found.

Altar Number One was found on the top of a low, flat mound; one of the group of burial mounds on the bank of the river, near the Catholic cemetery of Sterling. It was covered with vegetable mould and sod. Form, an ellipse; diameters, four and one half and six feet. Height, including mound, about three feet. Height of stone work, eighteen inches. Four layers of stone so fitted together that each layer "broke joints." The upper layer was reddened and crumbled by the heat of many fires. In the charcoal on and around this altar were found charred fragments of bones. Every one that could be identified was found to be some portion of a human skeleton. Some of the long bones — the *femur* and the *humerus* — had been broken and split into small pieces. In the burial mounds near this altar I found small fragments of burned rock, as if fire from this sacred place had been thrown on the dead during the rites of burial. There was no evidence of cremation found in the burial mounds: none of the bones were charred. The altar mound was the most easterly mound of the group.

Altar Number Two was found by workmen while grading the western part of the race track of the Sterling fair grounds. The stone structure was composed of one layer of large flat stones, and was in the form of a circle, about twelve feet in diameter. It was buried about two feet below the present surface of the soil, and its position was not marked by a mound or embankment. The soil above it was a sedimentary deposit of river sand and mud. Like Number One, it is east of a group of burial mounds. This group and its

altar appear to have been on an island in Rock River in prehistoric times; but the north channel is filled up, and the ancient island, like its inhabitants, has long since ceased to exist. Under this altar was found the decomposed skeletons of five adult persons. They were buried with heads together at the centre, and feet at the circumference, and equidistant, like the spokes of a carriage wheel. All were buried supine. One of the skulls was pierced with a small sharp arrow-point, — a red chert point, such as are quite common in this locality. These bones were very brittle; and some were of a purplish color, like those described in Orflia's "Exhumations Juridiques" (vol. i. p. 350). Adjoining this altar on the west was found a large quantity of human bones, — the greatest number of persons I have ever found buried in one place. The bodies had been thrown into a deep pit in a promiscuous manner. In one burial mound of this group I found fragments of burned rock, and two stone knives that had been reddened by fire, also implements made of the prongs of antlers of deer, and a few fragments of rude pottery.

Altars Numbers Three and Four were found about four rods apart, northwest and southeast, in the centre of a broad valley on Rock Creek, one of the tributaries of Rock River. They were alike in all respects. Both were circular in form, about six feet in diameter, and composed of three layers of flat limestones. Both were also covered by a sedimentary deposit of clay. One was accidentally found by the point of a subsoil plough turning up a piece of burned rock. The other was found by probing the soil with a sharp iron rod. They had been built on the ancient surface of the valley, and had been covered by clay washed down from the hills. The stone work of both was crumbled almost to powder by the action of heat. The subsoil about these altars was filled with charcoal; but no bones were found. Only one implement was found, — a large stone axe, a few feet north of the northwest altar.

In this paper I have excluded ancient "fireplaces," where it appeared that they were used for domestic purposes; and also mounds without stone structures, that appear to have been used for beacon-lights. There is a marked difference in the structures. Fireplaces are found at the foot of some sheltering bluff or hillside. They were usually built into the hillside, and were concave or cup-shaped in form; as deep as they were broad. Fire might be buried in one of the fireplaces, and the embers would remain many days. They appear to be as old as the altars; for they are buried in the soil, and above

one of them was the decayed stump of a white-oak about three feet in diameter. Beacon mounds are somewhat irregular-shaped, never more than three in a group, and situated on the highest summits. The clay of which they are composed is mingled with ashes; but human remains are not found in them. I have never found a fireplace or a beacon mound near a group of burial mounds.

Antiquities in the Town of New Haven, Vermont. By John McNab Currier of Castleton, Vermont.

In September, 1875, a Mr. Prime of Bristol, Vt., showed me a fragment of an arrow-head, which he said he found a few days previously on some earth entangled among the roots of a tree that had been blown down by the wind, in the eastern part of the town of New Haven, Vt. The fragment was rudely chipped; and the material was a bluish quartzite, such as a large proportion of the arrow-heads and spear-points are made from in this section of Vermont. I had some curiosity to examine this locality for myself. On reaching the spot I found a hemlock tree blown down, without breaking or tearing many of the roots. The soil among the roots was fully eight inches thick, and remained undisturbed. Below the soil a layer of clay was also taken up at the same time undisturbed, from three to six inches thick. In this layer of clay Mr. Prime said he found the fragment of arrow-head. On examination I found many flint flakes of similar material to the fragment, and unmistakably of human workmanship. They seemed to be mingled with the clay immediately underlying the soil, to the depth of four or more inches. Many flakes also were found on the ground where the tree had stood.

I examined the soil on the roots of the tree with great care for flakes and other stone implements, but nothing of the kind could be found until the clay was reached. On cutting into the tree near its roots, I counted between ninety and one hundred annual rings. Near by were standing other trees of greater diameters; also there were several old decayed stumps in the immediate vicinity, fully three feet in diameter. The ground on this spot had never been ploughed, and I should judge that the forests had never been entirely cleared off. It was on a slightly elevated ridge of clay, about six rods wide, sloping

to the east and west into marshy ground, unfavorable for cultivation. This locality is about two miles west of the Green Mountain range, and one half mile west of the New Haven River. The face of the country is very uneven, being broken up into small plains, valleys, and ridges. This spot slopes gently northward and eastward, and is not well adapted for retaining upon its surface the decaying vegetation that would make up its soil. This clay ridge is undoubtedly of glacial formation.

I regard this as positive evidence of a much earlier occupation of the Champlain Valley than is commonly supposed. Certainly the formation of a deep layer of soil was subsequent to the dropping of those flint flakes.

On the Alabaster Quarries, Flint Mines, and other Antiquities of Wyandot Cave. By Horace C. Hovey, of Fair Haven, Conn.

Relics from American caverns have hitherto been found chiefly in dry chambers, where a pure atmosphere and a nitrous earth have kept them from decay. The most interesting examples of this antiseptic preservation are the textile fabrics and desiccated human bodies discovered in various caves in Kentucky. Stalagmitic deposits, which in France and England have yielded valuable archæological treasures, have here been but imperfectly explored. The only thorough method is that of cutting vertical slices through the cave-floor down to the solid rock, subjecting each barrow load to inspection. An approach to this was in the extensive excavations made by miners for saltpetre, who, during the war of 1812, worked over many acres in Mammoth and Wyandot Caves. Their object being to obtain materials for gunpowder, rather than scientific information, they have only left us legends of skeletons and other remains long ago scattered and lost. They were rude frontiersmen; and it has been customary to lay to their charge spoliations, some of which, as I am convinced, are of an older date. But, before giving the grounds of this opinion, it will be well to state the more general evidences of Indian preoccupancy, especially of Wyandot Cave, the antiquities of which it is the main design of this paper to describe.

Wyandot Cave is in Crawford County, Indiana, near the Ohio River; and from the first has been in the possession of the Rothrock

family, whose aid I have had in making the researches now to be recorded. For many years after its discovery its entire length was thought to be only about two miles; and, aside from its valuable beds of nitre and Epsom salts, it attracted little public attention; but now its ramifications are estimated at a total length of twenty-three miles. The portion called by way of distinction the "New Cave" was discovered in 1850, by enlarging a small opening noticed among loose fragments of rock. Beneath these a scuttle appeared, that had at some former time been used, and afterwards closed up. The first white persons who passed through this found a well-beaten path leading inward amid smoke-stained walls. Bits of hickory bark with charred ends were abundant. Numerous poles were found in various places, that may have served a double purpose, — as alpenstocks, and as weapons against wild beasts. Many tooth-prints on them are still to be seen. These poles were all saplings, from five to eight feet long, having no marks of any edge-tool, but seeming to have been pulled up by the roots, and the branches then twisted off. None of them were hickory or oak; but some kind of soft wood, such as poplar, pawpaw, or sassafras. I saw all these things on the occasion of my first visit, in 1854. An orifice known as the "Auger-hole" had at that time just been artificially enlarged from a diameter of six inches to that of twenty-one inches; barely admitting us, through a group of bulky stalactites, to a large area of unexplored territory beyond. Centuries must have elapsed since that narrow aperture had been so closed as to be impassable; yet there, in the nitrous earth, soft as a newly raked garden bed, were the moccasin tracks of an aboriginal party that once searched these dim avenues, going up on one side and returning down the other. The footprints, as I noticed, were parallel to each other, with heel and toe in line, instead of with the toes turned outward, — a fact confirming our opinion that they were left by Indians. I regret to say that these vestiges are now entirely obliterated by the heedless tracks of more recent visitors.

There are abundant deposits of clay in Wyandot Cave, especially of red and yellow ochre, very free from grit and earthy impurities. In its moist condition it is plastic; and when dry may be scraped, and then burnished, simply by friction with the palm. Reduced to dust, it makes an excellent pigment. As the Indian paths ran by these beds, it may be inferred that they obtained supplies of this useful material. While examining one of the heaviest clay beds in the south arm, two years or more ago, a strong current of air was observed

through a crevice above it. Heeding this indication, the guide dug a trench through the indurated clay. Then, crawling on our faces for nearly twenty yards, we entered a room, circular in form, forty feet in diameter and eight feet high, where it is certain that no white man had ever been before. Wolf tracks were numerous, which we traced to a spot where the animals had been wont to lie down. Hence we called the chamber "the Wolf's Lair." Quantities of charred bark lay strewed about the floor. Two entire torches, the ends only having been burnt, projected from a crevice overhead. Who could conjecture when those extinct flambeaux had last been touched by human hand? Reverently examining them, we replaced them where they had been discovered. The original entrance to the Wolf's Lair was not through the clay-bank pierced by us, but through a passage at the further end of the apartment, as was indicated by the curved strata bending to the floor; as if the rocks, breaking by their own weight, had fallen in. The place can hardly be more than twelve hundred feet from the mouth of the cave, and anciently was of easy access and frequent resort, though now reached only by a *détour* of perhaps a mile and a half.

ALABASTER QUARRIES. — The chief sin laid to the charge of the saltpetre miners of 1812 was an alleged attempt by them to fell a a huge stalacto-stalagmitic column, known as the "Pillar of the Constitution," standing at the end of the Old Cave, and two miles from its entrance. I found, on subsequently exploring the New Cave, in localities which the saltpetre miners never saw, other columns cut into precisely as this had been. This exonerates these pioneers from vandalism, and stimulates further inquiry as to the cause of such ancient excavations.

On measuring the Pillar of the Constitution, it was found to be about forty feet high, seventy-five feet in girth, and its base to be three hundred feet in circumference. Its material is a hard, white, striated, translucent mineral, slowly deposited by the evaporation of water freighted with carbonate of lime. This has from classic days been known as alabaster, and now is called "Oriental alabaster," to distinguish it from the more common and softer sort, composed of the sulphate of lime. The pillar is not merely crusted with this beautiful mineral, but is solid and homogeneous. Its immense weight has at some remote period caused the subjacent rocks to settle, thus opening cracks in the spreading, stalagmitic base, many yards long, and varying in width from an inch to a foot. Some are healed over; others remain open.

Starting from some of these cracks, a segment with an arc of thirty feet was cut from the base, and a cavity was made in the shaft itself about ten feet wide, seven high, and five deep. No one doubts that this was done artificially. I have attempted to ascertain how long a time has elapsed since it was done. The right edge of the incision runs underneath a stalagmitic wrapping eight feet wide and ten inches thick at its thickest part. Inspection shows that drippings like those now healing this wound were at work before it was inflicted, and that the cut was primarily made through a mass like that by which it is now overlapped. The region above ground is still covered by forests. Hence, as there has been no change of conditions, there has been constancy of stalagmitic growth. A series of experiments has established the fact that in the immediate vicinity of the pillar stalactites grow at the rate of one inch in twenty-five years, but that stalagmites grow only one quarter of an inch in that period, or an inch in one hundred years. At this rate of increment, it must then have required one thousand years for the wrapping to have attained its present thickness of ten inches; and that length of time has therefore elapsed since this alabaster quarry was worked. Confirmatory of this calculation is the fact that some of the fragments thrown out from the pillar are cemented over cavities where clusters of exquisite stalactites have had time to form. Further search by me, in 1878, revealed the implements with which the ancient quarrymen wrought; namely, round or oblong bowlders of granitic rock, extremely hard, and of a size (from four to ten inches) suitable to be wielded by hand, or twisted in a withe and swung as a maul. Rothrock, who has been familiar with the cave from boyhood, denies having at any time carried any such stones into it, or of any one else having done so to his knowledge. The customary tool for gathering specimens is a hammer or hatchet; and these clumsy stone-pounders would be used only by those who had no better tools.

The theory that these pebbles were swept in by an underground stream is untenable: first, because barometrical observations show the place to be twenty feet higher than the entrance; then, there are here no beds of sand or gravel, or other indications of recent fluviatile action; and, moreover, the entire region lies below the line of glacial drift. Professor Collett, the State geologist, informs me that the entire cave is at least one hundred feet below any drift yet discovered in Crawford County. It is certain, therefore, that these small bowlders were brought by hand from a long distance. Although nothing

has been done by art to modify their natural proportions, they seem to have been carefully selected with reference to their fitness for use. Hard as the material is, the ends are whitened and battered by much pounding. My conclusion, therefore, is well grounded that these were the implements used by the Indians, more than one thousand years ago, in breaking from this alabaster quarry blocks of a portable size.

Quite probably the spot was resorted to by successive generations for material to be made into amulets, ornaments, disks, and images; such as we are told by Dr. Joseph Jones have been repeatedly exhumed in the Southern States, and also, according to Professor S. F. Baird, from tumuli in Illinois. If not found abundantly in Indiana, the reason may be that, although this kind of alabaster is hard and durable when not exposed to the elements, it is fibrous in its structure and liable to decay amid atmospheric vicissitudes, as was proved by fragments found in a crumbling condition just outside the cave.

As remarked, there are other alabaster quarries in the newer portions of Wyandot Cave. One of these is in the stalagmitic mass crowning Monument Mountain, — an eminence 135 feet high, rising under a dome 205 feet high and 1,000 feet in circumference. The stalagmite is 25 feet in diameter at its base, pyramidal in shape, and sustains three statuesque figures, the tallest of which is 8 feet high. I observed, what it is a little singular no one has remarked upon before, an incision in the base and side of this alabaster pyramid, precisely like that in the Pillar of the Constitution. Searching amid the *débris* below, I found similar fragments of alabaster and granitic pounders.

In the south arm of the cave a different quarry was noted in what is called the "Pillared Palace." Here the alabaster was obtained by felling the smaller columns, from 3 to 7 feet in height. Some were broken up, while others are still nearly entire. Granitic pounders, again, were found lying under adjacent limestone ledges, as if secreted there.

It should be added that, in all cases, the fractures, instead of showing a sharp edge and bright surface, like those made by recent visitors, have a discolored, corroded appearance befitting their antiquity.

THE FLINT MINE AND WORKS. — The proprietor of Wyandot Cave is impressed by the notion that it was once a favorite resort for bears. Here and there are "bear slides," which I suspect were really worn smooth by Indians clambering over the rocks. In like manner,

for twenty-five years he has pointed out some "bear wallows" not far from the Pillared Palace. These are circular depressions, twenty or more in number, each a yard wide and a foot deep. A thin crust of clay covers them, and their appearance agrees well with their name. About two years ago, however, I had the satisfaction of proving them to be the remains of ancient flint-works. Happening to remove the clay crust from a bear-wallow, I found a pile of ashes and cinders on one side and a quantity of flint chips on the other. On examination this proved true of each wallow. Further removal of the crust brought to light hundreds of flinty prisms with parallel faces, and averaging four inches in length by one and a half in width and half an inch in thickness.

The mine is near by, abounding in flint nodules, lying in rows in the cave walls, and occasionally in bands or belts. Each nodule has a coating of some grayish mineral, perhaps discolored flint, and between them is usually a soft, chalky substance, easily cut by a knife. Freshly fractured, a bright black surface appears, in contrast with the dingy, faded blocks by the wallows. This change of hue is due to the gradual removal of the traces of iron found with the silex. Many of the blocks were rejected on account of flaws or imperfections. The nodules are easily split into this form, which is convenient for transportation. Arrow-head making, however, was carried on here to a considerable extent, as appears from the chips. Pounders like those in the alabaster quarries were found along with the flints, showing the means of breaking the nodules.

The only manufactured article dug up in this spot was a little stone saucer, containing a soft black substance. This may have been a rude lamp.

Search at the mouth of the cave unearthed quantities of flint chips, and also finished arrow-heads. The question has been raised why the Indians should delve for flint balls amid subterranean darkness, when quantities of such spheres are found along the beds of streams and elsewhere in the open air. The reason is that the latter, having been exposed to the elements, have deteriorated in quality; they also break with irregular cleavage. Hence the Indians sought to get flints fresh from the strata where they were originally deposited; and which, because of their moisture, readily part into quadrangular prisms under the hammer.

Since making known the existence of this flint mine in Wyandot Cave, I have learned of flint-pits dug along Indian Creek and else-

where in Harrison County, Indiana; and Professor Collett's observations on their peculiarities confirm my own conclusions, independently arrived at, as to the probable date of these works, the nature of the implements used, and the final disposition made of the materials obtained.

Some Observations on Geometric Beauty, considered as consisting in Angular rather than in Linear Ratios.

By S. Edward Warren, of Newton, Mass.

[abstract.]

The object of this paper was to present some results of a fresh study of the general proposition that, if one geometrical form is beautiful, and another not so, there is a discoverable geometrical reason why the former is so, which reason, being known, would facilitate ornamental geometrical design, the definition having been ventured that beauty in general consists in spontaneous expression of excellence of some sort; that is, of some form of good, as, for instance, of some worthy purpose.

Three abstract principles of beauty were then mentioned and illustrated: *unity*, whereby a whole, consisting of parts, is made one by the possession of one aim or use; *uniformity*, by which parts, while evident and appropriate components of a whole, are alike; *harmonious variety*, by which parts may be unlike, and yet each, in its own appropriate way, may contribute to the perfection of the whole; *freedom*, by which was meant wholes of different design for the same purpose, yet each so self-consistent throughout, and so consistent with surrounding circumstances, as to be agreeable.

Next, passing from purely abstract principles to quantity in general, elementary numerical representatives of the foregoing principles were found in 1, as the representative of unity; $2 = 1 + 1$, as that of uniformity; $3 = 2 + 1$, as that of variety; $5 = 2 + 3$, as that of the combination of the last two; and 7, as that of freedom, owing to the number of ways in which it embraced the preceding.

Next, lest the foregoing, being abstract, should be deemed only fanciful, it was rehearsed that musical chords are based on the numbers 1, 2, 3, 5, and rarely on 7, and on multiples of these.

Next it was argued, from the admitted unity of nature, that if beauty

of sound is demonstrably founded on the numbers mentioned, beauty of *form* might be so also. But here exists the alternative between supposing beauty of form to consist in ratios between the *lengths of its determining lines*, or between its *angles*. It was argued that form and size were distinct elementary geometrical ideas, *size* depending on the *number* of points in a magnitude (considering a point not now as an absolute zero of magnitude) and *form*, on their *arrangement*. *Direction* was considered as being to *form* what *distance* is to *size*; that is, as more fundamental to the idea of form than distance is; and, as an angle is generally defined as difference of direction, it was argued that *ratios between the angles* of a magnitude, and founded, as in the ratios in musical chords, upon the numbers 1, 2, 3, 5, and perhaps 7, are the secret of purely geometric beauty. Some illustrations from rectilinear objects were then given, preferring rectangular ones as most frequent and practically important.

For example: A parlor being 24 feet long, shall its width (being not arbitrarily chosen) have a certain simple ratio, as $\frac{2}{3}$ or $\frac{4}{5}$, to its length, or shall it depend on the ratio between the angles into which its floor diagonal divides a corner right angle? Upon the latter, on the principles of the paper; and, accordingly, making these angles 36° and 54°, with a ratio $\frac{3}{4}$, the width would be $17' : 6''$; or, if these angles were 30° and 60°, giving the ratio $\frac{1}{2}$, the width would be $13' : 10''$. In either case a longer wall diagonal, dividing the rectangular corner into angles of $22\frac{1}{2}°$ and $67\frac{1}{2}°$, having the ratio $\frac{1}{3}$, would give the harmonious, as well as practically suitable, height of 9'.9 feet.

Passing to the more intricate subject of curvilinear beauty, the superiority of ovals, or egg-curves, to bi- or multi-symmetrical curves (ellipse and circle), was mentioned, and the distinction between natural curves, formed according to a uniform law, and artificial curves, composed of arcs of more elementary curves, was noticed; and then a method of constructing ovals indefinitely varied in form, from the most compressed (or turnip-shaped) to the most elongated (or spindle-shaped) was shown, consisting in making various sections of a right *conoid* (warped surface) of circular base, by planes parallel to its line of striction (rectilinear directrix), with an attempt to connect these ovals systematically with certain governing angular ratios.

The paper concluded with a few applications of such ovals to the design of house and garden vases, fruit dishes, gas shades, etc., and a hope that attention might be more generally directed to a subject possessing so much, both of philosophical and industrial interest.

Note on an American Example of a "St. Giles Staircase." By S. Edward Warren, of Newton, Mass.

The St. Giles staircase is one of the most complicated conceivable constructional features in architecture and engineering. Therefore, as a visit to Marblehead promises to be in order during the present session of the Association, a note upon the construction may interest those visitors who are interested in such matters.

In popular language, the structure is a winding stairway, arched overhead as it winds; the whole, steps, walls, and spiral arched covering being in cut stone. It will be found in the old abandoned fort at Marblehead, garrisoned, when last seen, by one man in working dress, and armed only with a wooden rake.

To those who are unfamiliar with the construction, its essentials can be made intelligible, step by step, without model or diagram, as follows: —

1. Hold a rod vertically in the left hand.

2. Hold a second rod horizontally in the right hand, one end of it meeting the first rod.

3. If the second rod be moved upward, keeping it parallel to itself, it will sweep over, that is, generate a vertical plane area.

4. If, instead of this upward motion, this same second rod be revolved about the vertical rod, keeping it horizontal, and meeting the vertical rod at a fixed point, it will generate a horizontal circular area.

5. If both the motions just described be combined, and if both be uniform, the horizontal rod will have an ascending spiral motion, always in a horizontal position, around the vertical rod, and will generate the spiral surface of a square-threaded screw, — the same as is seen on a larger scale in the plastered under-side of circular stairs winding around a cylindrical central post or pit, and called a helicoidal surface.

6. But now substitute for the horizontal moving rod a semicircle, having that rod for its diameter, and in a vertical position; that is, so that the vertical rod shall be tangent to it at one extremity of its horizontal diameter.

7. Now give this semicircle the same compound or spiral motion that was given to the moving rod in the last case, and it will generate what may be popularly termed the cylindrically spiral surface, which will be the arched ceiling of the staircase.

... stones of a semicircular arch
... overhead arch of the St.
... of the generating semi-
... these joint surfaces will be
... that is, like the surfaces of the
... screw. These straight lines
... vertical rod or axis, and hence
... also intersect the helix or spiral
... moving semicircle. These prop-

... perpendicular to the arched sur-
... circular passage, but over an
... thus perpendicular, we fall on
... would require diagrams and models,
... which are expensive and difficult.
... inquiry for the stone-winding stairs
... that a model in plaster of the like
... Institute of Technology.

PAPERS READ IN THE SUBSECTION OF ANTHROPOLOGY.

IMPLEMENTS FROM THE RIVER DRIFT OF
By C. C. Abbott, of Trenton, N. J.

INDIAN OCCUPANCY OF THE ATLANTIC
AMERICA, SUBSEQUENT TO THAT OF PA-
By C. C. Abbott, of Trenton, N. J.

COUNTY, NEW YORK. By W. M.
Jamesville, N. Y.

ILLUSTRATED BY A LARGE MANUSCRIPT
Sycamore, of New York, N. Y.

FOR DELINEATING THE OUTLINES OF
Sycamore, of New York, N. Y.

IN THE EXTERNAL AUDITORY CANAL
By Clarence J. Blake, of Boston, Mass.

REMARKS ON THE MOUND-BUILDERS. By J. F. Everhart, of Zanesville, Ohio.

CONTEMPORANEOUS EXISTENCE OF MASTODON AND MAN IN AMERICA. By R. J. Farquharson, of Davenport, Iowa.

THE PROBABLE EXISTENCE IN AMERICA OF THE PREHISTORIC PRACTICE OF TREPANNING, IN THE CUTTING OF RONDELLES OR AMULETS FROM THE SKULL. By R. J. Farquharson, of Davenport, Iowa.

ON STONE AXES. By S. S. Haldeman, of Chickies, Pa.

REMARKS ON ABORIGINAL POTTERY. By S. S. Haldeman, of Chickies, Pa.

ANCIENT MOUNDS IN VICINITY OF NAPLES, ILL. By J. G. Henderson, of Winchester, Ill.

SIGN LANGUAGE AND PANTOMIMIC DANCES AMONG THE NORTH AMERICAN INDIANS. By J. G. Henderson, of Winchester, Ill.

TEXTILE FABRICS OF THE ANCIENT INHABITANTS OF THE MISSISSIPPI VALLEY. PT. I. MATERIAL. PREPARATION OF MATERIAL AND SPINNING, ILLUSTRATED WITH SPECIMENS OF BARK, SPINDLE-WHORLS, MODELS OF SPINDLES AND DRAWINGS. By J. G. Henderson, of Winchester, Ill.

TEXTILE FABRICS OF THE ANCIENT INHABITANTS OF THE MISSISSIPPI VALLEY. PART II. THE LOOM AND THE FRUIT OF THE LOOM, ILLUSTRATED WITH MODELS OF LOOMS, MODELS OF SHUTTLES, AND DRAWINGS. By J. G. Henderson, of Winchester, Ill.

TEXTILE FABRICS OF THE ANCIENT INHABITANTS OF THE MISSISSIPPI VALLEY. PT. III. MOUND-BUILDER, STONE-GRAVE AND CAVE FABRICS, ILLUSTRATED WITH SPECIMENS OF CLOTH FROM MOUNDS, COPPER AXES WITH CLOTH ADHERING TO THE SAME, AND POTTERY WITH IMPRESSIONS OF CLOTH ON THE SURFACE. By J. G. Henderson, of Winchester, Ill.

ENGRAVED TABLET FROM A MOUND IN OHIO. By W. J. Knowlton, of Boston, Mass.

THE VOGULS. By A. Kocsis, of Tullahoma, Tenn.

THE INDIAN QUESTION. By D. A. Lyle, of Springfield, Mass.

SCHEME OF THE TENTH CENSUS FOR OBTAINING STATISTICS OF UNTAXED INDIANS. By Garrick Mallery, of Washington, D. C.

THE USES OF THE "CHUNGKEE-STONE." By Alfred M. Mayer, of Hoboken, N. J.

A COMPARISON BETWEEN THE SHELLS OF KJÖKKENMÖDDINGS, AND PRESENT FORMS OF THE SAME SPECIES. By E. S. Morse, of Salem, Mass.

JAPANESE CAVES. By E. S. Morse, of Salem, Mass.

PERSISTENCE OF KOREAN ORNAMENTATION IN JAPANESE POTTERY. By E. S. Morse, of Salem, Mass.

PREHISTORIC AND EARLY TYPES OF JAPANESE POTTERY. By E. S. Morse, of Salem, Mass.

THE MILITARY SYSTEM OF THE EMBLEMATIC MOUND-BUILDERS. By S. D. Peet, of Clinton, Wis.

THE TOPOGRAPHICAL SURVEY OF THE WORKS AT AZTALAN, WIS. By S. D. Peet, of Clinton, Wis.

RELATION OF THE ARCHÆOLOGY OF VERMONT TO THAT OF THE ADJACENT STATES. By George H. Perkins, of Burlington, Vt.

ON THE RANK OF INDIAN LANGUAGES. By J. W. Powell, of Washington, D. C.

THE CLASSIFICATION OF KINDRED BY THE N. A. INDIANS. By J. W. Powell, of Washington, D. C.

CONVENTIONALISM IN ORNAMENTATION OF ANCIENT AMERICAN POTTERY. By F. W. Putnam, of Cambridge, Mass.

ON THE OCCURRENCE IN NEW ENGLAND OF CARVINGS BY THE INDIANS OF THE NORTHWEST COAST OF AMERICA. By F. W. Putnam, of Cambridge, Mass.

MYTHS AND FOLK-LORE OF THE IROQUOIS. By Erminnie A. Smith, of Jersey City, N. J.

ON THE IROQUOIS LANGUAGE. By Erminnie A. Smith, of Jersey City, N. J.

EXHIBITION OF SOME GAMBLING GAMES OF THE IROQUOIS. By Erminnie A. Smith, of Jersey City, N. J.

FEELING AND FUNCTION AS FACTORS IN HUMAN DEVELOPMENT. By Lester F. Ward, of Washington, D. C.

PARTURITION IN A KNEELING POSTURE, AS PRACTISED BY THE WOMEN OF THE MOUND-BUILDER AND STONE-GRAVE RACE. By C. Foster Williams, of Ashwood, Tenn.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

Wednesday, August 25, 1880. — At 10.45 A. M. the Association was called to order in Huntington Hall, Massachusetts Institute of Technology, Boston, by the retiring President, Professor GEORGE F. BARKER, of Philadelphia, who presented the President elect, the Hon. LEWIS H. MORGAN, of Rochester, who expressed his thanks for the honor and his desire to discharge his duties with promptness and impartiality.

At the request of the President, the Rev. GEORGE E. ELLIS, D.D., of Boston, offered prayer.

WILLIAM B. ROGERS, LL.D., President of the Massachusetts Institute of Technology, and Chairman of the Local Committee, then addressed the Association as follows: —

Ladies and Gentlemen: — Speaking as the representative of the local committee of the hospitable city of Boston, and speaking as one of the old representatives of the American Association for the Advancement of Science, I have a double duty to perform, — to speak for the guest and for the entertainer. The American Association for the Advancement of Science has never yet held a meeting in this the city of Franklin, of Bowditch, and of the long line of other scientific worthies, prominent among whom have been Wyman and our great instructor, our adopted citizen, Louis Agassiz. It seems a fitting place for such an association to convene. Its spirit, its institutions, its history, its habits and sympathies, all favor such a reunion between its citizens and the lovers and promoters of science. It was my good fortune, if it is a good fortune to be able to date back one's life for a long period of years, to have been familiar with this institution from its cradle, when it first presented itself as the Association of American Naturalists and Geologists. This, however, was not the earliest congress of science assembled in the world. The origin of this thought of an annual parliament of scientific men seems properly to belong to the great German philosopher Oken, who as early as 1822 organized the German Association for the Advancement of Science. For eight or nine years this example was not followed, but in 1831 Brewster, aided by Brougham, established the British Association for the Advancement of Science, which we are to regard as the parent institution from which we have sprung.

This British Association, meeting first in the ancient city of York in 1831, has held its annual meetings, for a series of years, in all the great capitals and some of the secondary cities of the kingdom, faithfully ad-

ministering to the needs and stimulating the energies of scientific inquiry, and publishing each year its solid quarto, which represents so well the progress of physical and natural science. This parent Association is holding to-day its fiftieth annual meeting; and now, in the afternoon of its work, I picture in thought some of the illustrious chiefs of science that are there assembled; I call to mind Sir Joseph Hooker, Sir William Thomson, Huxley, Tyndall, Balfour Stewart, Stokes, Ramsay, and Owen, and other great representatives of the physical, mathematical, and natural science of the present age. I trust that to-day, before we shall have closed our meeting, there will be transmitted by the cable a note of greeting from the American Association assembled here to the British Association now gathered at Swansea.

It was eight years after this memorable event of the establishment of the British Association that our American Association of Geologists and Naturalists was founded, bringing together a zealous band of scientific workers at the time when our State surveys were reaping their first discoveries, when our geology, palæontology, and our natural history in general were comparatively unexplored. In the year 1847, during my presidency of this earlier society, the plan was framed of a more extended and comprehensive social organization for the advancement of science; and in the following year, under the presidency of Redfield, of New York, the first meeting of this enlarged Association, in its present form, was held in the city of Philadelphia. Since that time this Association has continued to assemble yearly, save during the dark interval when, through the anxieties and sad necessities of the war it was compelled to hold its peace. But resuming its labors in 1865, the Association has continued with renewed vigor to prosecute its work, and we are now assembled at its twenty-ninth meeting.

The functions of our Association are adequately described by its title. As an association for the advancement of science, it expects of all those who become its members that they shall do whatever is in their power to quicken scientific thought, to accumulate facts, to discover and investigate laws, and, in a word, to enlarge the boundaries of every department of positive science. But while we are an association for the advancement of science, we are in the very nature of the case instrumental in its diffusion, as is well shown by the comparatively popular character of the meetings of the American Association. We cannot better promote the progress of science than by arousing in all quarters where the Association meets those latent energies, those inborn talents and tendencies, which may hereafter blossom and fructify in scientific results.

Thus it is, gentlemen, that our Association is assembled here, and while I would not compare it as yet in numbers or strength with the parent association, I see here to-day the promise of a great increase both in numbers and activity for the future. For the last twenty years the British Association has had a list of members averaging 3,500; an aver-

age attendance of nearly 2,500; and an income from its members of $12,500, having at the same time 1,000 life members, being able practically to promote scientific research by grants for different departments of inquiry amounting to from $5,000 to $10,000 a year. Here, my friends, is something worthy of our emulation, a direction of progress in which we shall have the sympathy of all scholarly and scientific men throughout the world.

Looking to the character of the communications presented at our meetings and published in our proceedings, let us make it our special care to exclude from our annual reports all detailed publications which are not of a character actually to add to the stock of human knowledge, whether that knowledge be simply the gathering together of facts by careful processes of research, or the development of laws by mathematical investigation. And let the prolonged discussions, which, however valuable they may be, are not of a quality and character appropriate to the transactions of a great body like this, be printed, as the Secretary suggests, in small type and in abstract in the latter part of the volume.

I thank my friends for the patience with which they have listened to one who does not like to call himself an old man, but who feels something of the spirit of the war-worn soldier who likes at times to shoulder his crutch and fight his battles o'er again. As I have spoken in behalf of the committee of the city of Boston, let me conclude with my personal welcome in behalf of this institution over which I have the honor to preside, and to say to you that the corporation and officers of the Institute of Technology are not only glad but proud to welcome the American Association for the Advancement of Science to this hall and to all the accommodations and comforts which the Institute can offer.

His Honor FREDERICK O. PRINCE, Mayor of the city, then spoke substantially as follows: —

Mr. President: — It gives me much pleasure to meet you and the Association over which you preside on this interesting occasion, and bid you welcome to the city and its hospitalities. Be assured I express the sentiments of our people, and of all our people, when I say that Boston is honored to-day in having within her limits this large body of distinguished scientists; for your rolls contain the names of many whose genius, learning, and labors are recognized and appreciated wherever men know their true benefactors.

This is the first time — if I am rightly informed — that the society has assembled in Boston, although it has met in other cities of the Commonwealth. The event will be a memorable one in our history.

Mr. President, you are among friends. You are among those who are in sympathy with your Association and appreciate the work in which it is engaged. Boston has always recognized the value of science and learning. She has always seen the power of knowledge as a factor in social

and political progress, and as an element of national prosperity. She has therefore always encouraged educational institutions.

In a few days we shall commemorate the two hundred and fiftieth anniversary of our municipal existence. At such a time we naturally survey our past history, and with pride — permit me to say with just pride — we draw attention to the fact that in 1632, two years only after the colonists landed upon this peninsula, then occupied by a single inhabitant, and before there was a white man in any place except Salem, where this organization has held an annual meeting, they organized a school for the instruction of their children, and soon after devised the present system of free popular education and made attendance upon school compulsory, for the reason that society cannot be safe where the people are ignorant, and because of the benefits, social and political, which come from intelligence and culture. From that early day Boston has always contributed most liberally to the support of public schools, and now her annual appropriations for them exceed a million and a half of dollars. In 1638, eight years after the settlement of Boston was commenced, our Puritan ancestors laid the foundation of Harvard College, and this institution has grown and prospered and reached its present exalted position for the most part through the fostering care and munificence of our citizens.

Many of the most distinguished members of this Association, many of those who have achieved triumphs in the fields of science — both the starred and the unstarred — are graduates of Harvard, and amid the classic shades of their alma mater first received the inspiration to engage in the study of nature as the highest of human pursuits.

Thus it will appear that Boston, through her efforts in the cause of education, has developed that love of knowledge which has distinguished her among American cities, and prompts her to welcome the advent of this Association to-day and wish it every success.

We are a hard-working community, busily engaged in those industrial pursuits by which wealth is accumulated. You will find everywhere within our limits evidence of material prosperity. All classes, except the intemperate and vicious, are enjoying as the fruits of their industry not only the comforts, but what would be termed a few years ago the luxuries of life. But this material success has not lessened our interest in intellectual progress. You will find that Boston will always cheerfully contribute from her abundance to the needs of science. She has done so in the past; she will do so in the future.

It is not for me to speak of the natural sciences in this presence. I am wholly ignorant of this department of knowledge; but permit me to say what I suppose will be conceded by all, that as science is the foundation of all the industrial arts, and as through these come civilization and wealth, and most of that which makes the comfort and happiness of society, it is fitting and just that every community should be willing and

ready to do all which may be necessary for the development of scientific truth. What has not science done for man! What pen or tongue can record all its benefactions! In the words of Macaulay, we may say that "it has lengthened life, it has mitigated pain, it has extinguished diseases, it has increased the fertility of the soil, it has given new securities to the mariner, it has furnished new arms to the warrior, it has spanned great rivers and estuaries with bridges unknown to our fathers, it has guided the thunderbolt innocuously from heaven to earth, it has lighted up the night with the splendor of the day, it has extended the range of the human vision, it has multiplied the power of the human muscle, it has accelerated motion; it has annihilated distance; it has facilitated intercourse, correspondence, all kindly offices, all despatch of business; it has enabled man to descend to the depths of the sea, to soar into the air, to penetrate securely into the noxious recesses of the earth, to traverse the land on cars which whirl without horses, and the ocean in ships which sail against the wind; these are but parts of its fruits."

Obviously there is a community of interest between the scientist and those who are engaged in what are called industrial pursuits. Each party needs the other — *alter alterius auxilio eget.* One reveals through his investigations those secret principles of nature which enable the other to accumulate the wealth from which the cost of such investigations should be paid. But the scientist rarely gets for himself much of the pecuniary results of his labors. He sows, but the miner and merchant and manufacturer and mechanic reap. With the former it is usually *sic vos non vobis;* it is usually *tulit alter honores.* But those engaged in scientific pursuits, if influenced by the true spirit, will find their reward and compensation in the delights which follow philosophical discovery and in the proud reflection of employing the intellect, which makes man but a little lower than the angels, upon objects worthy of its divine nature.

I congratulate the Association on the auspicious future before it. The field of science has now been cleared of most of the impediments which come from prejudice, ignorance, and bigotry. It is no longer obstructed by metaphysical conceits or the dogmatic arrogance of sectarian schools. The utilitarian spirit of the Baconian philosophy everywhere obtains. Even the theologian no longer opposes the revelations of God's law from the absurd fear that truth could be made a successful weapon against religion; but is willing to believe with the great philosopher, that if a little knowledge leads to scepticism, complete knowledge conducts to faith. The field before you is free and open, and the grandest discoveries may be expected in every branch and department of science.

We shall be glad, gentlemen, if you find, in the success of this convention, in the evidence you will receive of our sympathy in your cause, and in the hospitable entertainment of our city and its citizens, reasons for carrying away with you pleasing recollections of this visit.

His Excellency Governor LONG, representing the Commonwealth, spoke additional words of welcome : — In behalf, also, of the Commonwealth of Massachusetts, I am happy to extend cordial welcome to the American Association for the Advancement of Science. It was organized thirty-three years ago in this her capital city, and it holds the charter of its incorporate life under the act of her legislature. It has enrolled upon its membership the names of sons of hers who by their contributions to the store of useful knowledge, have paid her the best return for the education she gave them. Among its presidents it reckons names precious in her estimation and memory, — the names of Agassiz, Peirce, Gould, and Gray. Massachusetts regards the true advancement of science with no jealous or distrustful eye, but rather as a synonyme for the greater happiness of the people, the better mastery of nature, the foundation of a surer faith in God the creator, the nearer equality of a democratic state. She rejoices in its achievements when she welcomes from all the States of the Union such an illustrious gathering of scientific men as are here to-day in its interest. And when she hears the ring of its hammer, the click of its chisel, in the hands of her own artisans and mechanics, who, in the varied useful and homely industries of civilization, in her machine shops, at the wheels of her railroad cars, in her manufactories, are at once dignifying and elevating the lot of labor, the craft of handiwork, and who are at the same time contributing to the enlargement of the comforts, the opportunities, the usefulness of human life, and to the common weal of her citizens. For science has no favorites in the beneficence of its results. It discloses no secret that is not echoed around the globe. If it electrify the wire with messages of joy or of appeal, it is for the ear of the humblest laborer as well as for that of a king. If Bigelow invent or perfect his loom, it is that the floor of the farmer's cottage may be carpeted as softly to the farmer's foot and as tastefully to his eye as if he were a merchant prince. Whether it be the inventions that have developed the exhaustless power of steam; that have made the lightning a hand-maiden; that have rendered warmth and light cheap and common comforts for all alike; that have bettered our food; that have provided transportation with marvellous economy and speed, or that have enabled the remotest provincial to be a cosmopolite, and have laid the world under contribution to the American citizen, high or low, rich or poor, science has taken no exclusive as well as no backward step. Her march is like that of the sun. Eternal dawn and brightening go before her. The darkness flies, the shadows disappear, and her blessing falls on all the world alike. It is in this spirit that Massachusetts welcomes you who make science your mistress and who minister to her advancement. If there be within our Commonwealth populous and busy cities and towns, alive with thrift and industry, singing the song of the wheel, the hammer, and the loom, and sweet with homes; if there be institutions of learning; if there be provision broadcast for the education and elevation of all her children, inde-

pendent of race or color or condition, it is because the advancement of science has made all this possible and easy. From Franklin and Rumford to Morse and Bell, Massachusetts has welcomed and fostered every new addition to scientific enterprise and achievement. And yet she pays you the highest compliment by asking for yet more. Her farms, her factories, her homes, all clamor for still swifter means of development and product and comfort. If she points with pride to her great names in the realm of scientific research and progress, she also points to them still more impressively as examples of what yet greater things this generation may do for the advancement of science and the bettering of human life. With gratitude and respect, with also expectation and demand, I therefore welcome you, gentlemen, to the Commonwealth of Massachusetts; a Commonwealth that has a future all the more because it has a history and a past; the Commonwealth of Adams and of Andrew, the Commonwealth of Franklin, of Agassiz and Morton, of Harvard and its sister colleges, the Commonwealth of the technical institute and the free school, and a Commonwealth that sees its highest destiny in the highest advancement of the knowledge and happiness of all its people.

President MORGAN responded for the Association in the following words: —

Mr. Chairman: The Association have listened with pleasure to your address of welcome to the city of Boston. In no other city of our land are better appreciated the unity of the sciences and the brotherhood of scientific men. These are central ideas of this Association; and when we meet among a people whose hospitality is vitalized by intelligent sympathy, a powerful impulse is given to the work which it is designed to promote. I venture to predict, sir, that this meeting will become memorable in our history.

It may seem singular that this session of the Association should be the first one held in the good city of Boston during the long series of twenty-nine annual meetings. It has, however, met at Cambridge, which in the public eye is part of Boston. We cannot and we ought not to separate Cambridge, with its noble university, and its distinguished body of teachers, from Boston, in which the roots of Cambridge are planted. They are "one and inseparable" in association as in fame.

Thus we are enabled to say that this Association is indebted to Boston for a peerless cluster of presidents; the illustrious and lamented Agassiz, to whom American science is so deeply indebted; the learned and gentle Wyman, whose loss we still mourn; these have ceased from among us, and their departure has rounded and completed their fame. Rogers, Peirce, Gould, Gray, Lovering, yet remain with us, and therefore we cannot, on this occasion, speak of them as their distinction deserves. "Seri in cœlum redeatis."

The Association, Mr. Chairman, were glad of the invitation to hold

this session in the metropolis of New England; in a city which has long held, and still holds, the front rank in our country in science and in art, and in the apprehension of the principles of political and social well-being; where commercial enterprise has always been an instinct, and commercial honor a natural sentiment. The members of this Association, one and all, desire to express their gratification at the cordial terms of welcome with which they have been received. It gives me great pleasure, as a part of my official duty, to return their thanks to the people of Boston and its vicinity, and to the various institutions representing its intellectual life, for their kind efforts to render this meeting pleasant as well as successful. Without making any discriminations, we may be allowed to notice the special means for our accommodation provided by the Institute of Technology. We are glad to be under the roof, so to speak, of the chairman of the local committee.

Mr. Mayor: The American Association for the Advancement of Science is popular in its character, as it should be. Investigators in all departments of science are cordially welcomed to its membership. By this free intercourse of persons engaged in scientific pursuits, results of the highest importance are constantly attained. The young are stimulated to greater efforts by the encouragement, and even by the criticisms, of the more advanced, while the latter gain in their mental scope by suggestions springing from younger minds. The unseen benefits of these yearly meetings in arousing the minds of young men and prompting them to enter upon fields of labor, no doubt far exceed the benefits which are apparent. The quickening power thus imparted is one of the best results of our Association.

It is precisely here, Mr. Mayor, that the reception accorded to this session by the city of Boston has its deepest significance. When the meetings of this Association become indifferent to the communities among which they are held, its usefulness will be near its end. There is a direct connection between the work upon which its members are engaged and the material prosperity of the country, in which all alike have an interest. Scientific investigations ascertain and establish principles which inventive genius then utilizes for the common benefit. We cannot have a great nation without great development of the industrial arts; and this, in its turn, depends upon the results of scientific discovery as necessary antecedents. Material development, therefore, is intimately related to progress in science. The cordial welcome, Mr. Mayor, which you have extended to this Association, in behalf of the people of Boston, manifests an appreciation of the relation of science to human progress which is complimentary as well as gratifying to its members. We fervently trust that the results of this meeting will not disappoint the public expectation.

Your Excellency, Governor of the Commonwealth of Massachusetts: Without intending to depart from the proprieties of the occasion, it may

be proper to say that those of us who come from beyond the Hudson can but feel that in entering New England we enter the birthplace of American institutions. To some of us it is the land of our fathers, and we cannot approach the precincts of their departed presence without the sentiment of filial veneration. Here they laid broad and deep the foundations of American freedom, without which American science would have been an infant in leading-strings to-day. Here was developed the Township, with its local self-government, the basis and the central element of our political system. Upon the township was formed the County, composed of several towns similarly organized ; the State, composed of several counties; and, finally, the United States, composed of several States, each organization a body politic, with definite governing powers in a subordinate series. But the greatest of all, in intrinsic importance, was the Township, because it was and is the unit of organization and embodies the great principle of local self-government. It is the restored Attic Deme of Cleisthenes, the greatest and the grandest of the Grecian ruins. Tyranny and barbaric war had defaced it ; ignorance and superstition had buried it deep. It was virtually lost to mankind, until, after twenty centuries, it was reinvented in New England. It is at once the greatest and most important of American institutions, because it determines the character of the state and national governments. It is also historically significant, because it shows that American Democracy may justly claim to be the daughter of that Athenian Democracy which generated and produced the most signal outburst of genius and intellect in the entire history of the human race. Nor is this presage of the future without its own significance. What was achieved for philosophy and art under the free institutions of Athens may yet be achieved for science in the evolution of the same forces in America.

Mr. Chairman and gentlemen, with a grateful appreciation of the kindness of the people of Boston, the Association is now prepared to enter upon the regular work of the session.

The PERMANENT SECRETARY gave notice that the following members of the Association had died since the last meeting : —

GEORGE W. ABBE, of New York.
E. B. ANDREWS, of Lancaster, Ohio.
HOMER C. BLAKE, of New York.
F. A. CAIRNS, of New York.
CALEB COOK, of Salem, Mass.
BENJAMIN F. MUDGE, of Manhattan, Kansas.
THOMAS NICHOLSON, of New Orleans, La.
LOUIS FRANÇOIS DE POURTALÈS, of Cambridge, Mass.

The financial report, also presented by the PERMANENT SECRETARY, showed a balance in favor of the Association.

The STANDING COMMITTEE was then completed by the election of the following six Fellows : —

N. T. LUPTON, of Nashville.
F. W. CLARKE, of Cincinnati.
E. T. COX, of Oakland.
WILLIAM HARKNESS, of Washington.
O. T. MASON, of Washington.
S. A. LATTIMORE, of Rochester.

On motion of Prof. LUPTON, it was voted that a committee be appointed by the President to prepare a message of greeting to the British Association, to be sent by cable. Professors W. B. Rogers, Asa Gray, and N. T. Lupton were appointed, and sent the following despatch : "The American Association for the Advancement of Science, in session in Boston, sends cordial greetings to the British Association at Swansea, on the occasion of its fiftieth meeting."

It was also voted that the President be requested to appoint a committee of three to propose suitable resolutions of regret at the death of the late General Albert J. Myer of the United States Signal Service.

The GENERAL SECRETARY presented a list of four hundred and twenty-five names of persons recommended to membership by the Standing Committee, and the same were duly elected.

After the reading of several notices by the GENERAL SECRETARY, the Association adjourned to meet in Sections.

At 7.30 P. M. the Association again met in General Session, in Huntington Hall, to hear the address of the retiring President, Prof. GEORGE F. BARKER, of Philadelphia.

Thursday, August 26. — *Cambridge Day.* The Association was called to order at 11.30 A. M. in Sanders Theatre, by President MORGAN.

Prof. A. M. MAYER read an eulogy of Joseph Henry.

Twenty persons proposed by the STANDING COMMITTEE were duly elected members.

Dr. J. L. LE CONTE moved that the Constitution and By-Laws of the Association be so amended as to establish a section of Biology. This was voted and referred to the STANDING COMMITTEE.

The Association then adjourned to meet as Section B, when Vice President AGASSIZ delivered his address.

Martin Brimmer, Esq., in behalf of the President and Fellows of Harvard College invited the members of the Association to dine in Memorial Hall.

Friday, August 27. — A General Session was held in Huntington Hall, at 10 A. M., President MORGAN in the chair.

One hundred persons recommended to membership by the STANDING COMMITTEE were unanimously elected.

After the reading of several notices, the General Session adjourned to meet in Sections.

At 7.30 P. M. a General Session was held in Huntington Hall, President MORGAN in the chair.

Prof. A. GRAHAM BELL gave his paper on the Photophone, which was illustrated by the use of the lantern.

Saturday, August 28. — President MORGAN called the General Session to order, at 10 A. M., in Huntington Hall.

Twenty-one persons were elected to membership on recommendation of the STANDING COMMITTEE.

Various announcements were made for the day by the GENERAL and PERMANENT SECRETARIES.

Prof. STONE moved that a Committee on Standard Time be appointed. Seconded by Prof. REES, and referred to the STANDING COMMITTEE. Adjourned to meet in Sections.

Monday, August 30. — General Session was called to order in Huntington Hall, at 10.30 A. M., President MORGAN in the chair.

On the recommendation of the NOMINATING COMMITTEE, the invitations from Cincinnati to hold the next meeting in that city were accepted, and the 17th of August was agreed upon as the time for the next meeting.

The GENERAL SECRETARY then read the following cablegram: —

"To the President of the American Association for the Advancement of Science, Boston, Massachusetts: — British Association, in completing its fiftieth year, returns thanks for congratulations to its brother Association on the other side of Atlantic.

President British Association, Swansea."

The GENERAL SECRETARY then read letters from the Natural History Society of Montreal inviting the Association to hold the meeting of 1882 in that place. Also letters from the Minnesota Academy of Natural Sciences, Governor of Minnesota, Mayor of Minneapolis, the Geological and Natural History Survey of Minnesota, and the Board of Trade of Minneapolis, inviting the Association to hold its next meeting in their State.

Dr. DAWSON made a few remarks with reference to the Montreal invitation.

On the recommendation of the NOMINATING COMMITTEE, the following resolutions were passed: —

Resolved, That the thanks of the American Association for Advancement of Science be tendered to the Scientific Society of Montreal for its cordial invitation to meet in that city in 1882.

Also *Resolved*, That the thanks of the American Association for Advancement of Science be tendered to the Governor of Minnesota, the Mayor and City Council of Minneapolis, the Geological and Natural History Survey of Minnesota, and to the Board of Trade of Minneapolis for their cordial invitation to meet in their State in 1881.

And further *Resolved*, That this Association trusts that the invitations may be repeated at another time.

Reports of Committees being in order, the PRESIDENT called upon the several Chairmen.

The Permanent Committee on Weights, Measures, and Coinage did not report.

Mr. HOLLEY reported progress for the Committee to Memorialize the Legislature of New York for a new survey of Niagara Falls, and asked that the Committee be continued.

Report accepted, and Committee continued.

Mr. HOUGH gave an abstract of the Report of the Committee on Cultivation of Timber and the Preservation of Forests.

Report accepted, and Committee discharged.

Committee on International Congress of Geologists was discharged.

Committee on the Yellowstone National Park was discharged.

Committee on Relation of Science to the Industrial Arts reported progress.

Report accepted, and Committee continued.

Committee on Meteorological Researches was discharged.

Committee to obtain Permanent Funds was continued.

Committee on Membership was continued.

Committee on Science Teaching in the Public Schools presented their report by their Chairman, Dr. E. L. YOUMANS, which was accepted, and the Committee was discharged. It was *voted* that the members of this Committee be appointed a Committee on The Best Methods of Science Teaching in the Public Schools.

Prof. HARKNESS reported for the Committee on Amendments to the Constitution. The report was adopted and ordered to be printed, and the Committee was discharged.

Mr. ELLIOTT made a brief report for the old Committee on the Registration of Deaths, Births, and Marriages.

The General Session then adjourned to meet in Sections.

Tuesday, August 31st. — General Session in Huntington Hall, President MORGAN in the chair.

Nine persons proposed for membership by the STANDING COMMITTEE were duly elected.

Judge HENDERSON moved that the officers for the next meeting, recommended by the NOMINATING COMMITTEE, be elected by the Secretary casting one ballot for the whole list, which was voted, and the officers nominated were declared elected, as follows: —

G. J. BRUSH, President.
A. M. MAYER, Vice President, Section A.
C. V. RILEY, General Secretary.
JOHN TROWBRIDGE, Secretary of Section A.
WILLIAM SAUNDERS, Secretary of Section B.
W. S. VAUX, Treasurer.
HENRY WHEATLAND, THOMAS MEEHAN, Auditing Committee.

The election of Vice President, Section B, was postponed until next General Session.

The GENERAL SECRETARY then read the following Resolutions recommended by the STANDING COMMITTEE, which were adopted: —

Resolved, That the second evening of each Session be set apart for the social reunions of the several Sections, the object of which shall be the promotion of the personal acquaintance of the members with each other.

Also *Resolved*, That such reunions shall be under the direction of the Sectional officers, who, in conjunction with the Local Committee, shall make for such reunions all necessary arrangements, which shall be announced in the circular of the Local Committee when practicable.

The following Resolutions, recommended by the STANDING COMMITTEE, were also adopted: —

Resolved, That the American Association for the Advancement of Science recognizes the value of contemporaneous observations, at numerous and well selected stations, and with standard instruments, as a first and indispensable condition of converting meteorology from a chaotic mass of useless facts into a science.

Resolved, That this Association acknowledges its obligations to the first Secretary of the Smithsonian Institution for originating, supporting, and cherishing such a system of meteorological observations throughout the vast domain of the United States, until it had outgrown the resources of the Institution, had justified its continuance by its proved usefulness, and had awakened the fostering interest of the Government.

Resolved, That in the opinion of this Association the welfare of commerce and agriculture and the comfort of every member of the community have been promoted by the weather reports and weather charts which have been issued by the Chief Signal Service at Washington, while they have at the same time furnished food for scientific thought.

Resolved, That the Association feel and hereby express the great loss which this service has suffered in the recent death of its chief officer, General A. J. Meyer, whose energetic administration of novel duties, seconded by his able corps of scientific assistants, has commanded universal respect at home and abroad.

On recommendation of the STANDING COMMITTEE it was voted, That a Special Committee be appointed to select a series of stars suitable for standards of stellar magnitudes, and to report to the Association at the next meeting.

On recommendation of the STANDING COMMITTEE, a Committee on the Registration of Deaths, Births, and Marriages was appointed, consisting of E. B. ELLIOTT, F. B. HOUGH, J. B. KILLEBREW, JOSEPH S. COPES, E. T. COX.

The following Resolution was reported from the STANDING COMMITTEE: —

Whereas to-day, the 31st of August, is the ninety-fifth birthday of M. E. CHEVREUL, senior member of the French Academy, therefore be it *Resolved*, That the American Association for the Advancement of Science, now in session in Boston, send him its hearty congratulations, with the hope that his life and his labors may be prolonged at least to the full century. And be it further *Resolved*, That the President of the Association be hereby instructed to officially transmit the above resolution.

Prof. MAYER and Mr. GARFIELD made a few remarks on the eminent worth of M. Chevreul, and the Resolutions were adopted.

Prof. MINOT made a communication in regard to the plethysmograph, and invited members to examine the apparatus.

On motion, Prof. N. T. LUPTON, of Vanderbilt University, was added to the Committee "on the Best Methods of Science Teaching in the Public Schools."

On motion of Prof. I. E. HASBROUCK, the Association proceeded to ballot for Fellows.

Judge HENDERSON and Prof. HASBROUCK were appointed tellers.

Prof. YOUNG moved that the tellers make their report in print at the Morning Session.

Carried.

The Association then adjourned to meet in Sections.

At 7.30 P. M. a General Session was held in Huntington Hall for the purpose of hearing Prof. HYATT's paper on the Transformations of Planorbis, — a practical illustration of the evolution of species, — with stereopticon views. This was followed by a series of exhibitions illustrative of the use of the lantern-microscope. After which was an exhibition of microscopes, accessory apparatus, and objects, under the auspices of the Section of Microscopy of the Boston Society of Natural History, in the lower rooms, where a collation was also served.

Wednesday, September 1. The last day's session was opened at 10.40 A. M., President MORGAN in the chair.

The list of Fellows nominated by the STANDING COMMITTEE was declared elected, as follows: —

Prof. W. A. ANTHONY, Ithaca, N. Y.
HENRY P. ARMSBY, New Haven, Conn.
Prof. W. S. BARNARD, Ithaca, N. Y.
Prof. W. J. BEAL, Lansing, Mich.

Dr. EMIL BESSELS, Smithsonian Institution, Washington, D. C.
Prof. C. E. BESSEY, Ames, Iowa.
Prof. A. S. BICKMORE, New York, N. Y.
Prof. H. P. BOWDITCH, Boston, Mass.
Prof. A. A. BRENEMAN, Ithaca, N. Y.
ALVAN G. CLARK, Cambridgeport, Mass.
VERPLANCK COLVIN, Albany, N. Y.
Prof. A. J. COOK, Lansing, Mich.
Prof. LE ROY C. COOLEY, Vassar College, Poughkeepsie, N. Y.
Dr. THOMAS CRAIG, U. S. Coast Survey, Washington, D. C.
Prof. C. R. CROSS, Institute of Technology, Boston, Mass.
Prof. A. E. DOLBEAR, College Hill, Mass.
Prof. F. P. DUNNINGTON, University of Virginia, Va.
J. RAYNER EDMANDS, Cambridge, Mass.
HENRY EDWARDS, New York, N. Y.
ARTHUR H. ELLIOTT, School of Mines, New York, N. Y.
Dr. R. J. FARQUHARSON, Davenport, Iowa.
Prof. EDGAR FRISBY, Washington, D. C.
Dr. S. A. GOLDSCHMIDT, New York, N. Y.
F. A. GOOCH, Newport, R. I.
CLARENCE KING, New York, N. Y.
HENRY C. LEWIS, Germantown, Pa.
E. P. LULL, Commander U. S. N., Sitka, Alaska.
Lieut. D. A. LYLE, U. S. A., National Armory, Springfield, Mass.
Prof. H. N. MARTIN, Baltimore, Md.
Prof. C. C. MERRIMAN, Rochester, N. Y.
Dr. C. S. MINOT, Boston, Mass.
J. DUNCAN PUTNAM, Davenport, Iowa.
J. J. PUTNAM, M.D., Boston, Mass.
PIERRE DE P. RICKETTS, New York, N. Y.
Prof. H. A. ROWLAND, Baltimore, Md.
J. J. SKINNER, New Haven, Conn.
Mrs. ERMINNIE A. SMITH, Jersey City, N. J.
Prof. F. H. SMITH, University of Virginia, Va.
ALFRED SPRINGER, Ph.D., Cincinnati, Ohio.
Dr. G. M. STERNBERG, Washington, D. C.
J. P. TREMBLY, M.D., Oakland, Cal.
WARREN UPHAM, Nashua, N. H.
LEONARD WALDO, S.D., New Haven, Conn.
Prof. C. K. WEAD, University of Michigan, Ann Arbor, Mich.
Prof. C. O. WHITMAN, Tokio, Japan.
Prof. A. A. WRIGHT, Oberlin, Ohio.

Nine new members were recommended by the STANDING COMMITTEE and were elected.

Dr. GEORGE ENGELMANN of St. Louis was recommended by the NOMINATING COMMITTEE for Vice President of Section B. On ballot he was declared elected.

The STANDING COMMITTEE recommended the following resolution, which was adopted: —

Whereas Dr. CHARLES T. JACKSON, one of the founders and an early President of the Association of American Geologists and Naturalists, has, after many years of illness and seclusion, just passed away, it is fitting that this Association express its high appreciation of his long and valuable services, both as an original investigator in American Geology and Mineralogy, and as a teacher of Chemistry, which will cause his name to be long held in honor and in grateful remembrance.

At the suggestion of the STANDING COMMITTEE the following was *voted*: —

That any member of the Association who pays the additional sum of Ten Dollars may have the volumes of the Proceedings of the American Association for the Advancement of Science to which he may be entitled, which are issued subsequent to the payment, bound in cloth; and on the payment of Twenty Dollars, bound in one-half Turkey morocco.

Prof. HARKNESS made a few remarks in regard to the desirability of recommending the next Nominating Committee to favorably consider the invitation to Montreal in 1882.

The SECRETARY then read the following resolution, passed by the STANDING COMMITTEE: —

Resolved, That the STANDING COMMITTEE most earnestly recommend to the NOMINATING COMMITTEE to give favorable consideration to the invitation to meet in Montreal in 1882.

This resolution was unanimously adopted.

Principal DAWSON of Montreal thanked the Association for its action.

The PERMANENT SECRETARY called the attention of the Sectional Committees to a meeting for the transaction of important business.

Adjourned to meet in sections.

The closing session was held at Huntington Hall at 8 P.M., President MORGAN in the chair.

Thirteen persons were elected members on recommendation of the STANDING COMMITTEE. The Committee also nominated three members for Fellows, and they were elected. [Their names are included in the preceding list of Fellows elected.]

The GENERAL SECRETARY presented a resolution from the STANDING COMMITTEE appointing a special committee to wait upon his Excellency the President of the United States, in relation to the Signal Service, which was adopted.

President MORGAN, after a few remarks relating to the cordial reception which the Association had received from the people of Boston and vicinity, called for such resolutions as had been prepared to be offered at this closing session.

Prof. HARKNESS, after appropriate introductory remarks, offered the following : —

Resolved, That the cordial thanks of the American Association for the Advancement of Science be tendered

To his Excellency the Governor of the Commonwealth of Massachusetts, and to his Honor the Mayor of Boston, for their cordial and eloquent addresses of welcome to the Association on the occasion of its opening session.

To Professor William B. Rogers, for his eloquent words of welcome, and for his energy and perseverance in rendering this meeting of the Association the most successful and important in its history.

To the Corporation of the Massachusetts Institute of Technology, for its liberality in opening its buildings and all their facilities for the use of the officers and members of the Association throughout the meeting ; also to the Faculty, for their personal attentions to the wants of the members and visitors.

To the Local Committee of the citizens of Boston, and especially to its officers and to the members of the Executive Committee, for their arduous labors in obtaining funds, and in arranging so admirably for railroad facilities, for lodgings, for the mid-day lunch, and for the hospitalities which have been so generously provided for the members attending the meeting.

To His Honor the Mayor, and the other Municipal authorities, for their liberal provision in behalf of the city for the Harbor Excursion on the 28th ult., which was so much enjoyed by the members of the Association.

Seconded by Judge HENDERSON, and enthusiastically carried.

Professor NASON then proposed the following : —

Resolved, That a vote of thanks be tendered

To the Boston Society of Natural History, for the numerous courtesies rendered by its officers, and for the use of its rooms.

To the Trustees of the Boston Museum of the Fine Arts, for the invitation to visit their rare and beautiful collections.

To the President and Fellows of Harvard College, for the use of Sanders Theatre, for the bountiful collation served in Memorial Hall, for the invitation to the Botanic Garden and to the Observatory, museums, laboratories, and other departments of the University.

For the delightful receptions given by

Prof. and Mrs. William B. Rogers, at the Hotel Vendome,
Dr. and Mrs. A. Graham Bell, at Cambridge,
Mr. and Mrs. S. Endicott Peabody, at Salem.

The resolution was seconded by Rev. Mr. SHACKELFORD, and after remarks by several members was unanimously adopted.

Professor LATTIMORE offered the following: —

Resolved, That the thanks of the Association be given

To Dr. Henry Wheatland, President of the Essex Institute, to Judge William C. Endicott, President of the Peabody Academy of Science, and to the Salem Committee, for the interesting and profitable visit to Salem and its scientific institutions on Monday last.

To the Eastern Railroad Company, for the use of trains to convey the members of the Association to and from Salem, Rockport, and Marblehead, and for their great liberality in furnishing trains free of cost for the three days' excursion to the White Mountains.

To Messrs. Samuel H. Scudder, Edward Burgess, John M. Ordway, Charles R. Cross, R. H. Richards, William H. Niles, J. Rayner Edmands, Hamilton A. Hill, Charles L. Jackson, A. P. Rockwell, J. R. Chadwick, C. F. Folsom, Nathan Appleton, E. S. Ritchie, Charles H. Williams, Charles E. Fay, Mrs. R. H. Richards, and Mrs. W. H. Niles, for their very kind and numerous attentions, and as representatives of the many who have spared no pains to contribute to the comfort and convenience of the members of the Association.

To the representatives of the Press, who have attended its meetings and reported its proceedings so fully, particularly to the Boston Daily Advertiser for the arrangements made for obtaining extended reports of the proceedings of the various Sections.

To the following Corporations and persons, for highly appreciated attentions and valuable services: —

The Western Union Telegraph Company, Geo. F. Milliken, Manager.
American Rapid Telegraphic Dispatch Company.
American Bell Telephone Company, W. H. Forbes, President.
Dr. A. Graham Bell, for special telephone conveniences.
The American Academy of Arts and Sciences.
The Massachusetts Historical Society.
The Massachusetts Horticultural Society.
The Historic-Genealogical Society.
The Society of Decorative Art.
The Young Men's Christian Association.
The Young Men's Christian Union.
The Warren Museum.
The Boston Public Library.
The Athenæum Library.
The Boston Medical Library.
The Trustees of the Old South Church.
The Perkins Institution and Massachusetts School for the Blind.

Commodore George M. Ransom, Commandant Navy Yard, Charlestown.
Colonel T. S. Laidley, U.S.A., Commandant Watertown Arsenal.
The South Boston Iron Company.
The President and Faculty of Tufts College.
Mrs. Thomas P. James, of Cambridge.
The New England Woman's Club.
Lieutenant Commander White, of the U. S. training ship Minnesota.
Mr. James B. Francis and other Manufacturers at Lowell, and the Boston and Lowell Railroad Company for courtesies extended on Monday.
The Heliotype Printing Company.
The School Committee of Boston.
The Principal of the Chauncy Hall School.
The Adams Express Company.
The American Express Company.
The United States and Canada Express Company.
The New York and Boston Despatch Company.

To the following Railroads for reducing their rates to members attending the meeting: — Concord, Northern New Hampshire, Central Vermont, Boston & Providence, Old Colony, Boston & Albany, New York, New Haven & Hartford, Eastern, Boston & Maine, Maine Central, European and North American, St. John & Maine, Michigan Central, Chicago, St. Louis & New Orleans, Cleveland, Columbus, Cincinnati & Indianapolis, Union Pacific, Central Pacific, East Tennessee, Virginia & Georgia, Philadelphia, Wilmington & Georgia, Grand Trunk, Boston, Concord & Montreal, Fitchburg & Hoosac Tunnel Line, Vandalia Line, Bee Line, and Wabash Line.

This resolution was most heartily seconded and endorsed by Rev. H. C. HOVEY, of New Haven, Mr. JOHN L. PROCTER, of the Geological Survey of Kentucky, and Dr. J. LAWRENCE SMITH, of Louisville, and was adopted by a rising vote.

The PERMANENT SECRETARY stated that as a slight memorial of the Boston meeting, a copy of the Memoirs of the Association had been, by vote of the STANDING COMMITTEE, presented to the New England Woman's Club.

After remarks by several members, President MORGAN, with a few concluding words, declared the twenty-ninth meeting of the Association adjourned.

J. K. REES,
General Secretary.

REPORT OF THE PERMANENT SECRETARY.

In 1847 the "Association of Geologists and Naturalists" held its meeting in Boston, and it was then decided that the usefulness of the Association could be increased by an enlargement of its objects, and the adoption of the name of the American Association for the Advancement of Science. After a third of a century, the Association met for the first time in its natal city, under the general guidance of Professor William B. Rogers, as Chairman of the Local Committee, who was also the presiding officer at its birth; and it was determined that the time had come when the Association, for the second time, should open its doors still wider, and, by the addition of other departments of research, give a still broader scope to its objects.

The changes in the constitution of the Association, which will be required in order to introduce the new sections, will be reported in print for final action at Cincinnati. It need only be stated here that the proposed changes provide for eight sections, and a permanent subsection (of Microscopy), in place of the two sections and the several subsections which have been found necessary at late meetings.

The large number of members present, the great increase in the number of papers entered, some of which were of unusual importance, and the great addition to the membership of the Association, mark the Boston meeting as the most successful ever held, while the perfect arrangements and elaborate provisions made by the Local Committee, who were so cordially seconded in the work by hundreds of the liberal citizens of Boston, as well as by its Government, were decided evidence of the respect and esteem in which the Association is held.

Considering all that was done for the comfort and entertainment of the members, the generous hospitality of the city, the cordial welcome of the Governor on the part of the State, and the liberality of the Eastern Railroad Company, as well as the hospitable greeting of the Corporation and officers of Harvard University, and that of many of the citizens of Salem, on the days devoted to visits to those places of learning and culture, the members of the Association, certainly, must regard this meeting as a brilliant success, that will be long remembered with satisfaction and gratitude by those who were present, and have additional reason to feel that its founders, a third of a century ago, acted with wisdom and far-seeing sagacity when they laid the foundation for "migratory meetings, in order to promote intercourse between those who are cultivating science in different parts of the country," and thus provided for their "increased facilities and a wider usefulness."

On Wednesday morning, August 25, 1880, the president of the last

meeting, Professor George F. Barker, of Philadelphia, called the Association to order in Huntington Hall, and introduced President Morgan. The Rev. Dr. George E. Ellis offered prayer, and remarks were then made by Professor William B. Rogers in behalf of the Local Committee. His Honor Mayor Prince and His Excellency Governor Long, extended a most cordial welcome to the members of the Association, to which President Morgan responded. The formalities of organization were then completed.

Sections A and B, and the Subsections of Chemistry, Microscopy, and Anthropology, were called to order by their officers in the several rooms in the building of the Massachusetts Institute of Technology, and perfected their organization by electing their several committees.

At one o'clock, a bountiful lunch was served by the Local Committee in the Gymnasium of the Institute, and an hour and a half of sociability followed.

At half-past two o'clock, Vice President Hall, of Section A, delivered his address in Huntington Hall, and at four o'clock Chairmen Ordway and Powell, of the Subsections of Chemistry and Anthropology, gave their addresses before their respective sections. In the evening Professor Barker delivered the presidential address to a large audience at a General Session of the Association, in Huntington Hall. After the address, a reception was given to the Association and many invited guests, by the Trustees of the Boston Museum of Fine Arts at the Museum.

During the day the Local Committee distributed to each member of the Association a pocket map of Boston and vicinity, with a pamphlet containing a brief account of the Scientific Institutions in Boston, Cambridge, and Salem, both of which had been specially prepared for the occasion by the Local Committee, under the immediate direction of Mr. Samuel H. Scudder. On the walls of the Local Committee room several large maps and plans were hung, showing the different horse-car routes of the vicinity, the water-supply system, old and new drainage systems, and other matters of general interest.

A special post-office, telephone, telegraph, and express offices, and newsstand, were established in the Institute building, and were of very great service to the members; thanks particularly to the liberality of the Western Union Telegraph and American Bell Telephone companies.

Many invitations and offerings of courtesies were announced on the daily programme from the various institutions of the city and vicinity, and every effort was made to convey to the members of the Association a cordial greeting.

The Entomological Club of the Association was more largely attended than ever, and on a later day it was resolved to dissolve the Club and reorganize as a permanent subsection of the Association.

The Association of Agricultural Chemists also called a meeting, to be held in the rooms of the Boston Society of Natural History, and the American

Chemical Society gave notice of a dinner of members of the society, to be held at Young's Hotel, on Friday evening.

On Thursday the Association held its General Session at 11 A.M., in the Sanders Theatre, Cambridge, at which Professor A. M. Mayer, in behalf of the committee appointed at a previous meeting, read an eulogy of the late Professor Joseph Henry. Immediately after the Association met as Section B, and Vice President Agassiz delivered his address. At half-past one o'clock, by invitation of the President and Fellows of Harvard College, the Association took dinner in Memorial Hall, Martin Brimmer, Esq., of the corporation, presiding.

After the dinner visits were made to the Museum of Comparative Zoology, the Peabody Museum of Archæology and Ethnology, the Physical Laboratory, the Mineralogical Cabinet, the Chemical Laboratories, the Library, the Gymnasium, and other College buildings and places of interest. Special receptions were also held in the afternoon at the Botanic Garden, the Observatory, and at the room of the Cambridge Entomological Club, and at Mrs. T. P. James's. At six o'clock a tea was given to the Association at the Botanic Garden, followed by a visit to the Observatory, and by a reception at the house of Mr. and Mrs. Alexander Graham Bell, the members returning to Boston about midnight.

Friday was given to solid work in the Sections, after a short General Session in the morning. The intermission between the morning and afternoon sessions was most pleasantly passed at the Association lunch, furnished by the Local Committee. In the evening Professor Bell's paper, on the photophone, was delivered in General Session at Huntington Hall, and was followed by a reception at the Vendome, given to the Association by Mr. and Mrs. William B. Rogers.

After the General Session on Saturday morning, the sections and subsections met for the reading of papers.

Section A was temporarily subdivided by the separation of the papers on Physics from those on Mathematics and Astronomy. Section B also made a temporary division into Geology and Biology, and the Subsection of Entomology was formed.

By invitation of His Honor the Mayor of Boston, the Association made an excursion down the harbor on the steamer Empire State. A collation was provided on board the steamer by the city authorities, and after a delightful trip the party returned at seven o'clock. In the evening, the Nominating and Standing Committees met at the Vendome. From eleven until two o'clock the Woman's Laboratory, the Biological Laboratory, and the workshops attached to the Institute of Technology were open to members of the Association for special examination. A Guide to the Museum of the Boston Society of Natural History, specially prepared for the occasion by Professor Hyatt, the Curator of the Museum, was distributed to members of the Association during the morning, as was also a guide-

book of Salem, which was sent with the compliments of the Essex Institute. On Monday, the Mining Laboratory of the Institute was put in operation, under the direction of Professor Richards, in order that members of the Association might see the practical way in which instruction in the school is given, and Mrs. Richards also explained the method of instruction given in the Woman's Laboratory. Members were also particularly invited to examine the Chauncy-Hall School House during the day. The Heliotype Printing Company also invited all interested to visit their establishment, and see the process of photo-mechanical printing. Professor Farlow opened his Cryptogamic collection, at Cambridge, to such as wished to examine it.

At the General Session in Huntington Hall in the morning, the invitations to hold the next meeting in Cincinnati were accepted; a number of Fellows were elected, and reports were received from several special committees. After short meetings of the Sections the members met at one o'clock at the lunch given by the Local Committee. The afternoon and evening were given up to several excursions and receptions. About four hundred members with ladies accepted the invitation to visit Salem, and were transferred to and from the city by a special train, generously furnished by the Eastern Railroad Company. At the Salem station the party were met by the Salem Committee and conducted to the Peabody Academy, the Essex Institute, and other places of interest. At five o'clock the members were conveyed in carriages to Kernwood, where they were hospitably entertained by Mr. and Mrs. S. Endicott Peabody at their beautiful residence; the party returned to Boston at nine o'clock, after a very enjoyable excursion, which reminded many members of the meeting of the Association held in Salem in 1869.

The Eastern Railroad also furnished free tickets to such of the members as wished to pass the afternoon at the seaside on Cape Ann, and to those who desired to make a geological excursion to Marblehead, and many took advantage of these liberal offers.

The Hon. James B. Francis very kindly arranged for a visit to Lowell and its factories, the Boston and Lowell Railroad placing a special car, free of charge, at the disposal of the Association. On the arrival of the party at Lowell, carriages and refreshments were provided by the manufacturing companies.

On Tuesday officers were elected for the Cincinnati meeting, after which the Sections met. The recess from one to half-past two o'clock was enjoyed in the lunch room, and the Sections were in session all the afternoon. In the evening Professor Alpheus Hyatt delivered his paper on the Transformations of Planorbis, as a popular lecture, with stereopticon views, in Huntington Hall. This was followed by a series of pictures thrown on the screen by the use of the lantern-microscope. In the room below an important and instructive exhibition of microscopes, accessory apparatus,

and objects, was given under the auspices of the Section of Microscopy of the Boston Society of Natural History, and a collation was served in the adjoining hall.

During the day a party visited the Watertown Arsenal, by invitation of the Commandant, Colonel Laidley, to see the machine used by Government for testing the strength of materials. Another party visited the Physiological Laboratory of the Harvard Medical School, by invitation of Professor Bowditch, under the guidance of Dr. Minot, at which place experiments with the plethysmograph were made. Still other parties accepted the special invitations to the Warren Museum of Natural History, which contains the well-known Warren Mastodon; the Historical Museum at the Old South Church; the Bunker Hill Monument; and by invitation of the Boston School Committee, the English High and Latin School, and the Girls' High School. A number of members also made a visit to Tufts College, by invitation of its President and Faculty.

The last day of the meeting was opened by a General Session in Huntington Hall, on the morning of Wednesday, September 1, and after a brief session the Sections and Subsections met to conclude the reading of papers still before them, adjourning for a recess and to meet the Local Committee at its concluding lunch, which had formed such an agreeable and essential part of the arrangements made by the Committee. In the evening the Association again met in General Session, and after the transaction of the final business of the meeting, resolutions, thanking the Local Committee, and the many individuals, corporations, and societies who had so generously and hospitably extended a welcome to the members of the Association, were passed with much enthusiasm, after being seconded by heartfelt remarks by various members. President Morgan, in a few brief words, then declared the Association adjourned, to meet in Cincinnati on the 17th of August, 1881.

The adjournment of the meeting was followed by a social gathering in the hall.

On Thursday the Eastern Railroad generously provided a special train for three hundred members of the Association, for a trip from Boston to the Fabyan House, White Mountains, with the privilege of returning free on any train, on or before September 4, and also permitting members to exchange their passes for regular tickets, good until October 1, by the payment of a small sum. Greatly reduced rates were also made for round-trip tickets, good until September 15, for the benefit of those who could not go on the special train. The Boston, Concord, and Montreal Railroad also furnished special rates for the Mountains and return, and the several hotels in the Mountains made special rates for the party. At 9.20 A.M., the members of the Association started in the special train as the guests of the Eastern and Portland and Ogdensburg Railroad Companies, under the escort of the Apalachian Mountain Club. Lunch was served on the

train by the Local Committee of Boston. In the evening, a meeting of the Appalachian Club was held at Fabyan's, at which the members of the Association were present. After the adjournment of the Club, an informal meeting of the Association was held, with Professor George F. Barker in the chair, and the following resolutions were unanimously adopted: —

Resolved, That the members of the American Association for the Advancement of Science who have enjoyed the excursions so generously tendered by the Eastern Railroad Company and the Portland and Ogdensburg Railroad Company, express their hearty appreciation of the kindness of these corporations and those who have co-operated with them, and their pleasure in the perfection of the arrangements made to render their journey delightful in the highest degree.

Resolved, That the thanks of the excursionists of the American Association for the Advancement of Science are hereby tendered to Messrs. J. Rayner Edmands and William H. Niles, and their associates of the Excursion Committee, for their efforts in behalf of the members of the Association who participated in the White Mountain trip.

On Friday the excursionists ascended Mount Washington. The party then separated, many remaining about the mountains for some time, while others returned to Boston on Saturday.

An excursion was also provided to Moosehead Lake, at greatly reduced rates, and complimentary passes were issued by the Boston and Albany Railroad Company to those members who wished to visit the Archery Tournament at Ridge Hill Farms, the gardens of Mr. Hunnewell, Wellesley College, and South Natick, the place of the Apostle Eliot's Indian work.

A summary of the statistics of the Boston meeting shows that there were entered on the register 997 names of persons who regularly attended the meeting, including members and invited guests. Six hundred and three new members were elected, of which number 518 have paid the admission fee, and are now entered on the roll of the Association. Nine declined, 1 has died, and 75 have not yet been heard from in reply to notices sent. Forty-six members were made fellows, and of these 38 have accepted at this date, and 8 have not yet replied to the notices informing them of their election. Eight members died in the interval between the Saratoga and Boston meetings.

Two hundred and seventy-nine papers were entered for reading at the meeting. Of these, 2 were afterwards withdrawn by their authors; 1 was declined as not coming within the province of the Association; 2 were read in General Session, and 135 were read either in full, or by abstract or title in Section A, and 139 in Section B, including their subdivisions. One hundred and thirteen of the papers were accepted by the committee for publication, and 85 were received from their authors, and appear in this volume, in addition to the several addresses,

reports of committees, and the eulogy on Professor Henry. A number of the accepted papers were offered by their authors too late for publication, and comparatively few were handed in within the specified time of one month. In this connection I beg to urge the consideration of the following plan for publication of papers received at future meetings as one which will not only make the early publication of the annual volume practicable, but will also, perhaps, better serve the interest of the Association. Namely: That in addition to the publication of the several annual addresses and the reports of committees, a short abstract be given of every paper permitted by the Sectional Committees to be read, unless special reasons should occur for its omission, provided the author of a paper furnishes the abstract to the Secretary of his section during the meeting, or to the Permanent Secretary within a month after adjournment. Should this be done, the annual volume would give a fair presentation of the subjects brought forward at each meeting, and probably would be regarded as more valuable than now by the majority of the members.

The cash account for the year, covering the period embraced between August 23, 1879, and August 23, 1880, presented at the Boston meeting, is more satisfactory than for many years preceding. The Life Membership Fund has reached the sum of $882, without including the accrued interest of nearly $100. There was also a small balance of over $80 in the treasurer's hands, and the receipts and expenses for the year leave a balance to the next general account of $448.24. This good result is in part due to the gifts of Messrs. Vaux, Flint, and Weld, which, as will be seen by the statement on a following page, amount to $131.00. For the other items reference is made to the cash account in this volume.

The Standing Committee at the Boston meeting called attention to the desirability of reprinting some of the volumes of Proceedings, of which copies can no longer be supplied, provided the necessary funds could be obtained by subscription. The only answer yet made to this very important request is the receipt of one subscription from a member in Virginia, who has given his mite for the purpose. As it is now impossible to fill orders for complete sets of the Proceedings of the Association, except as odd copies of Volumes II, XXVI and XXVII are obtained, it is very desirable that a general response be made to this notice. With the exception of the present, none of the volumes of Proceedings have been stereotyped. Volume I was reprinted several years ago.

The total number of members and fellows now enrolled is 1,555, but of these 102 are in arrears for the three assessments of St. Louis, Saratoga, and Boston; 79 for Saratoga and Boston, and 125 for Boston alone, which if paid would amount to $1,767.

The number of members and fellows on the list a year ago was 1,036, and two years ago, 962. At this date, 1,249 members and fellows have paid the assessment for the Boston meeting.

Since the last report was made, 1250 copies of the Saratoga volume have been printed, and 866 have been distributed to members, 67 by vote of the Standing Committee as exchanges, &c., and 49 copies have been sold. Of back volumes, 9 have been distributed to members, 28 by vote of the Standing Committee, and 203 have been sold since the last statement was made. One copy of the Memoirs has been presented to the Woman's Club in Boston, and 3 have been sold.

F. W. PUTNAM,
Permanent Secretary.

SALEM, Mass., July 13th, 1881.

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

1879-80.

To Assessments previous to the 28th Meeting . .	$540 00	
" for Saratoga 28th " . .	1,653 00	
" for Boston 29th " . .	1,035 00	
		$3,228 00
Entrance Fees, — St. Louis	$5 00	
" " Saratoga	755 00	
" " Boston	630 00	
		1,390 00
Publications and Binding		140 75
Fellowship Fees		58 00
Life Membership Commutation		450 00
General Receipts : —		
William S. Vaux, Donation	$100 00	
D. B. Flint, "	16 00	
W. G. Weld, "	25 00	
Postage and Discounts	50	
Illustrations for Saratoga Vol.	22 00	
		163 50
		$5,430 25

SALEM, MASS., August 28, 1880.

I have examined the above account, and certify that

IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE. Cr.

			1879-80.
By Balance from last Account			$187 91
Expenses Saratoga Meeting:—			
General Expenses at Saratoga	$148 57		
Record Book for St. Committee	3 00		
500 copies Constitution and List of Members,	38 25		
			189 82
Expenses Saratoga Vol., 1,250 *copies:—*			
Composition, Paper, and Press-work	$1,673 88		
Illustrations	24 50		
Binding 1,215 in Paper, 10 in ½ Turkey, 25 in Cloth, and 25 Cloth Covers	389 00		
Wrapping 850 for distribution	10 00		
Extra Copies of Addresses, and Reports	45 26		
			2,142 64
General and Office Expenses:—			
Office Rent to July 1, 1880	$100 00		
Janitor, 1 year	25 00		
P. O. Box Rent	4 00		
Subscription U. S. Postal Guide	1 50		
Scudder's Catalogue	5 00		
Postage and Stationery	209 87		
Tickets, Circulars, and Blanks	161 75		
Expressage	18 55		
Telegrams	4 45		
Binding Volumes of Proceedings	47 92		
Extra Assistants	34 46		
Fuel	16 25		
Volumes bought	6 00		
Minor Office Expenses	10 89		
			645 64
Salary Assistant Secretary	$366 00		
" Permanent "	1,000 00		
			1,366 00
Life Membership Fund			450 00
Balance to new account			448 24
			$5,430 25

the same is correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor.*

SCIENTIFIC INSTITUTIONS

OF

BOSTON AND VICINITY.*

BOSTON.

In Boston, the different societies are first mentioned; next, the educational institutions, excepting such as belong to Harvard University, which will be found under Cambridge, with the other departments of the University; a notice of the Museum of Fine Arts closes the list.

AMERICAN ACADEMY OF ARTS AND SCIENCES.

The American Academy of Arts and Sciences was founded May 4, 1780, and is, with the exception of the Philosophical Society of Philadelphia, which was founded a few months earlier, the oldest scientific society in America. The act of incorporation states that the "Design and institution of said Academy is to promote and encourage a knowledge of the antiquities of America, and of the natural history of the country, and to determine the uses to which the various natural productions of the country may be applied; to promote and encourage medical discoveries, mathematical disquisitions, philosophical inquiries and experiments; astronomical, meteorological, and geographical observations; and improvements in agriculture, arts, manufactures, and commerce; and in fine, to cultivate every art and science which may tend to advance the interest, honor, dignity, and happiness of a free, independent, and virtuous people."

The former Presidents of the Academy were Governor Bowdoin, John Adams, John Quincy Adams, Charles Francis Adams, Nathaniel Bowditch, John Pickering, Dr. Jackson, Dr. Bigelow, and Dr. Gray. The number of Fellows is limited to two hundred.

The rooms of the Academy are in the Athenæum Building on Beacon Street, Boston. Here will be found a valuable Scientific Library. The meetings of the Academy for the presentation and the discussion of scientific and literary papers are held once a month during the winter, and a volume of Proceedings is now published every year. By means of a fund left by Count Rumford, the Academy aids investigations on Light and Heat, and confers the Rumford Medal upon those who have made valuable contributions to pure science or applied science in the domain of light and heat.

* Prepared by the Local Committee of the American Association for the Advancement of Science, for distribution to Members of the Association, at the Boston Meeting.

The Academy celebrated its centennial in June last, and in honor of the event has raised, principally from among its own members, a publication fund of considerable proportions. It has already published fourteen volumes of Memoirs in 4to, and fifteen volumes of Proceedings in 8vo.

Prof. Joseph Lovering is President of the Academy, and Prof. J. P. Cooke and Prof. John Trowbridge, Secretaries.

BOSTON SOCIETY OF NATURAL HISTORY.

This Society, founded in April, 1830, has just celebrated its fiftieth anniversary. After having been established successively in Pearl, Tremont, Mason, and Bulfinch Streets, it now occupies one third of a square on Berkeley Street, between Boylston and Newbury Streets, a gift from the State, the land bordering the square having been sold at a premium sufficient to cover the cost. The building itself was erected in 1863 by the gifts of its friends, and especially of Dr. W. J. Walker, of Newport, through whom, also, the larger part of its funded property was derived. In its earlier years the Society was continually in debt, having no funds of its own apart from the members' fees; but in 1840 it came into possession of $10,000 from the estate of Mr. A. S. Courtis, in season to save it from the fate of the Linnæan Society of New England, which preceded it. Since that gift it has been more and more prosperous, and is now one of the most flourishing, as well as best endowed, institutions of the kind in America.

Scientific meetings, either of the general society or of some one of its sections, are held weekly, or oftener, during the season. Its library, composed largely of the publications of the four hundred learned societies with which it is in correspondence, contains about 20,000 volumes and pamphlets.

It does not attempt to accumulate great stores of material in its museum, but selects only such as may be of the best educational use, and attempts to display these to the best advantage, hoping, by means of guide-books, to render these available to all in the highest degree. These collections are arranged in a serial order from the basement to the upper gallery, and the visitor is carried from the inorganic kingdom up through the extinct animals and plants to the existing, in such a way that he can scarcely fail to see their relations to one another. In the New England collections only is it intended that the fullest exposition will be given. Among valuable special collections in the Museum may be mentioned the Wyman Collection of Comparative Anatomy, the La Fresnaye Collection of Birds, the Pratt Collection of Shells, the Harris Collection of Insects, the Bailey Collection of Microscopical Objects, and the Greene and Lowell Herbaria.

In the laboratory of the Society instruction is given in natural history to the students of the Institute of Technology and the Boston University, as well as to special classes of teachers and students.

Regular courses of free instruction in the same topics have also been given for several winters to a large body of teachers of the public schools, in which work it has been supported by a strong public sentiment, and generously aided by friends who have supplied the necessary funds. This teaching has been wholly by oral lessons upon objects in the hands of all

the pupils, and which they have been allowed to retain. During the past winter an average attendance of 500 teachers proved the success of the enterprise, and more than 100,000 specimens were given away.

The Society is the guardian of a Prize Fund, intrusted to it by Dr. Walker, for annual and occasional prizes. The Annual Prizes to the amount of $50 or $100 are for the most worthy essays in response to questions assigned; the Grand Prize of from $500 to $1,000 is given every five years for original work. It has been twice awarded: the first time to Mr. A. Agassiz, of Cambridge; the second, to Dr. J. Leidy, of Philadelphia.

The Society has published twenty-six volumes of Proceedings in 8vo.; seven volumes of the Boston Journal of Natural History in 8vo.; two volumes, and part of a third, of Memoirs, in 4to., besides a volume of Special Memoirs just issuing in commemoration of its fiftieth anniversary; and three volumes of Occasional Papers in 8vo. The printing is done in the building.

Mr. S. H. Scudder is President of the Society; Dr. S. L. Abbot and Mr. Edward Burgess, Secretaries; and Prof. A. Hyatt, Custodian of the Museum.

SOCIETY OF ARTS OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

The objects of this Society are to awaken and maintain an active interest in the practical sciences, and to aid generally in their advancement and development in connection with arts, agriculture, manufactures, and commerce.

The Society invites all who have any valuable knowledge of this kind, which they are willing to contribute, to attend its meetings, and become members. This Society was organized in 1861, and now numbers over 200 members. It holds regular meetings at its room in the Institute Building, on the second and fourth Thursdays of each month from November to May inclusive. At these meetings are presented communications on various subjects of applied science, with the exhibition of machines and apparatus illustrating important inventions in the mechanic and useful arts.

Prof. William B. Rogers is the President of the Society, and Prof. G. Lanza its Secretary.

ARCHÆOLOGICAL INSTITUTE OF AMERICA.

The Archæological Institute of America was founded in 1879 for the purpose of promoting archæological investigation and research in this continent and the Old World. It is composed of Life Members who have contributed to its funds at one time not less than $100, and of Annual Members who contribute annually the sum of ten dollars.

It is desired by the founders and existing members of the Society that its lists should include associates from all parts of the country. The objects of the Institute have no narrow local interest. It hopes, by its work, to increase the knowledge of the early history of mankind, to

quicken the interest in classical and Biblical studies, to promote an acquaintance with the pre-historic antiquities of our own country, and to enlarge the resources of our universities and museums by such collections of works of art and remains of antiquity as it may be enabled to make.

The Institute, therefore, asks for and claims the support, not only of specialists, scholars, and men of science, but of all persons, men and women, throughout the country, who take an interest in the objects it has in view. Its first report, forming, with the accompanying papers, an octavo volume, was published a few months ago. During the current year it will conduct investigations in New Mexico and in Asia Minor.

Prof. Charles Eliot Norton is President of the Institute, and Mr. E. H. Greenleaf, Secretary.

APPALACHIAN MOUNTAIN CLUB.

The Appalachian Mountain Club was organized in January, 1876, for the encouragement of geographical research and mountain exploration in the eastern portion of the United States. Its five departments treat of Natural History, Topography, Art, Exploration, and Improvement. So far, the principal field has been in the White Mountain region. Much useful topographical work has been done, partly in connection with the United States Coast Survey, and the Geological Survey of New Hampshire. Several novel surveying instruments for use among the mountains have been invented, including a topographical camera, micrometer level, portable plane tables, &c. The department of Improvements has secured the construction of numerous paths, including those on Mounts Adams, Carrigain, Willey, Tecumseh, and Moat; also, through Carter's Notch, Adams's Ravine, and Greeley's Notch. Some of the grandest scenery in New England, before rarely visited, is thus rendered easily accessible. Monthly meetings are held in the winter at the Massachusetts Institute of Technology, at which papers are read and discussed, and afterwards published in "Appalachia," the organ of the Club. Similar meetings are held during the summer among the mountains, thus securing the interest and aid of the summer visitors in carrying out and extending the work of the Club. By a process of rapid and continuous growth, the membership of the Club now includes over two hundred and fifty ladies and gentlemen.

Prof. Charles R. Cross is President of the Club, and Messrs. Rest F. Curtis and J. B. Henck Jr., Secretaries.

MASSACHUSETTS HORTICULTURAL SOCIETY.

This Society was organized in 1829, "for the purpose of encouraging and improving the science and practice of Horticulture, and promoting the amelioration of the various species of trees, fruits, plants, and vegetables, and the introduction of new species and varieties." It is now, with the exception of the Pennsylvania Horticultural Society, the oldest institution of the kind in the country. The most prominent means by which it has sought to promote the objects for which it was founded has been

weekly and annual exhibitions of horticultural products in their seasons, which were commenced immediately on its formation, and have been continued every succeeding year. It has paid in prizes for objects exhibited at these shows more than one hundred thousand dollars.

Another prominent means of advancing the objects of the Society, and probably the most interesting to the members of the American Association for the Advancement of Science, is the Library, which was commenced immediately after the formation of the Society, and is now by far the most extensive horticultural library in the country. It contains more than 3,500 volumes, among which are many costly botanical works, and nearly a thousand pamphlets.

During the winter months, meetings for the discussion of horticultural subjects are held by the Society. These discussions are published in the Transactions of the Society, which now fill seven octavo volumes. A history of the Society for the fifty years of its existence is now passing through the press.

In 1831 the Society established the cemetery at Mount Auburn, the first of its kind in the country, and may therefore claim to be the parent of all the rural cemeteries in the United States. It still continues to receive a proportion of the proceeds of sales of lots, which, with its income from other sources, has given it more ample means for the promotion of horticulture than is possessed by any other horticultural society in the country. In 1845 the Society completed a hall for its exhibitions, which is believed to be the first building ever erected anywhere for similar purposes. The present hall, on Tremont Street, was completed in 1865.

Hon. Francis B. Hayes is the President of the Society, and Mr. Robert Manning, Secretary.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

This institution was incorporated in 1861, "for the purpose of establishing and maintaining a Society of Arts, a Museum of Arts, and a School of Industrial Science."

The Society of Arts, of which a special notice is given above, numbers between two and three hundred members, and meets in the Institute Building twice a month, from October to May, inclusive. The Museum embraces working models of machinery, apparatus, casts, prints, drawings, architectural plans, etc., as well as minerals and fossils, chiefly at present in the keeping of the Society of Natural History.

The School of Industrial Science, which is the central and most important feature of the Institute, provides a series of scientific and literary studies, and practical exercises embracing pure and applied Mathematics, the Physical and Natural Sciences with their applications, Drawing, the English Language, History and Political Economy, French, and German; these studies and exercises being so arranged as to offer a liberal and practical education in preparation for active pursuits, as well as a thorough training for special scientific professions.

The Regular Courses of the School are: 1. Civil and Topographical Engineering; 2. Mechanical Engineering; 3. Mining Engineering, or

Geology and Mining; 4. Building and Architecture; 5. Chemistry; 6. Metallurgy; 7. Natural History; 8. Physics; 9. Science and Literature; 10. An Elective Course.

Each of these courses extends through four years, and for proficiency in any one of them the degree of Bachelor of Science is conferred. Advanced courses of study may be pursued for the degree of Doctor of Science. Provision is also made for those who wish to pursue only special portions of the regular courses.

Instruction is given by lectures and recitations, and by practical exercises in the laboratories, the drawing-rooms, and in the field; these exercises being esteemed indispensable as well for their educational training as their professional use. Separate laboratories are provided for Chemistry, Physics, Mechanical Engineering, Mining Engineering, Metallurgy, Mineralogy and Lithology, and some branches of Natural History. The drawing-rooms are well equipped, and the classes in Engineering, Industrial Chemistry, Architecture, etc. have the use of objects, models, and drawings appropriate to their work. There are also special laboratories for the instruction of women.

A School of Mechanic Arts has been established for manual instruction in the use of tools, etc., which, while affording needful training to the students in Engineering, provides courses of shop-work, combined with appropriate studies for those who aim to engage in mechanical pursuits, rather than to become scientific engineers.

Adjoining the principal building of the Institution are the work-shops, the Women's Laboratory, and the Gymnasium, the latter also used for military drill.

The Institute further provides afternoon or evening free courses of instruction, scientific and literary, open to both sexes, supported at present by the Trustee of the Lowell Institute, who also maintains in the Institute a School of Industrial Design.

The published record of the alumni shows that a very large proportion of the graduates of the Institute of Technology are holding important positions in connection with railroads and other constructions, mines, chemical and metallurgical works, and other manufactories, or as teachers of science.

The Corporation of the Institute consists of thirty-six members, exclusive of the Governor of the Commonwealth, the Chief Justice, and the Secretary of the Board of Education, who are members *ex officiis*. The Officers of Instruction are sixteen Professors, composing the Faculty of the School, nine instructors, and about an equal number of assistants.

The President of the Institute is Wm. B. Rogers, LL.D., and the Chairman of the Faculty is Prof. John M. Ordway.

BOSTON UNIVERSITY.

The School of Liberal Arts was organized at the establishment of the University, in 1873. All its scientific courses are given by Professors of the Massachusetts Institute of Technology, and, with the exception of

Biology and Zoölogy, within the walls of that institution. In each case the instruction is expressly arranged for the class, and illustrated by collections and apparatus of the Institute. These courses include the following: —

Chemistry. Sixty hours, lectures and laboratory work; given by Prof. William Ripley Nichols, to the Junior Class.

Physics. One hundred hours, lectures and laboratory work; given by Prof. Charles R. Cross, to the Sophomore Class.

Geology. Forty hours, lectures and recitations; given by Prof. William H. Niles, to the Junior Class.

Botany. This is an elective study. The course includes twenty hours, and is given by Prof. John M. Ordway, to students of the Junior Class.

Biology and Zoölogy. This course, which is also elective, comprises eighty hours of lectures and laboratory work. It is given in the Laboratory of the Boston Society of Natural History, by Prof. Alpheus Hyatt, to students of the Junior and Senior Classes.

Astronomy. This course is elective, includes twenty-five hours, and is given by Prof. Charles R. Cross, to students of the Senior Class.

BOSTON OPTICAL WORKS.

The Boston Optical Works, of which Mr. Charles Stodder is the agent, has acquired a wide reputation for excellent workmanship in the construction of microscopes and telescopes, but it is chiefly known by the optical achievements of Mr. R. B. Tolles. The following are some of the results accomplished by Mr. Tolles : —

(*a.*) In the matter of definition of objectives his $\frac{1}{8}$ was the first to yield photographs of *Amphipleu a pellucida.*

(*b.*) Invention and practical development of telescopes of short focus. Quadruple cemented object-glasses (1864).

(*c.*) First adaptation of amplifiers to the microscope in 1857. Compound amplifier requiring no special adjustments.

(*d.*) The first demonstration of the possibility of larger angles for immersion than for dry lenses.

(*e.*) The first development and utilization of the new apertures of microscope objectives. In these new apertures, the angle is advanced beyond the critical angle.

(*f.*) The first publication of the plan of 4 system objectives with duplex front.

(*g.*) The central traverse lens, first used in 1870.

(*h.*) Invention of the device of the radial arm for the rotation of the sub-stage concentrically about the object under view, in 1870.

(*i.*) The illuminator front for the illumination of opaque objects, published in 1866.

(*j.*) The binocular eye-piece, patented in 1866, and the solid eye-piece, patented in 1855; with adjustment for different thicknesses of slides.

(*k.*) Apparatus for illumination, having a lateral concentric rotation.

MUSEUM OF FINE ARTS.

The Museum of Fine Arts is situated at the corner of St. James Avenue and Dartmouth Street, upon land given by the city to an incorporated Board of Trustees, on condition that the collections should be free to the public on one day of each week. This is the case on Saturdays, and also, by special permission of the Trustees, on Sundays, from 1 to 5 P. M. The total number of visitors during the past year was 157,191.

The present building, designed by Messrs. Sturgis and Brigham, and erected from funds derived from private subscription, is but one side of the intended edifice, which, when completed, will be in the shape of a parallelogram, having a great court roofed with glass in the centre, high enough to contain casts of the largest size, such, for instance, as a cast of the column of Trajan, the façade of the Parthenon, etc. By using terra-cotta enrichments upon the building, the Trustees hoped to encourage the use in this country of a highly endurable, comparatively cheap, and thoroughly artistic material for decorative purposes, and thus make the very walls of the edifice serve an educational purpose. The terra-cotta work, made in England by Messrs. Blashfield & Co. expressly for the Museum building, consists of mouldings, crockets, finials, and other architectural details; of two large bas-reliefs representing the Génius of Art welcoming all nations, and the union of Art and Industry; and of a series of portrait heads in roundels of the most celebrated artists, ancient and modern. Among them are those of Pericles, Lorenzo de' Medici, and Prince Albert, as respectively the most illustrious promoters of Art in antiquity, in the period of the Renaissance, and in our own time.

First Floor.

On entering the building the visitor finds himself in a spacious hall, containing modern marbles, the most notable of which are Crawford's Orpheus, his group of Hebe and Ganymede, Matthiæ's bust of Beethoven on a richly sculptured bracket, Monteverde's First Inspiration of Christopher Columbus, etc. On the left-hand side, at the back of the hall, are fragments of ancient marbles found at Rome. These are set in the wall after the Roman fashion. The door-way on the opposite side of the hall opens into the room containing the Way collection of Egyptian antiquities, with which the visitor should begin, passing from it through the succeeding rooms, which contain a chronologically arranged series of casts, illustrative of the history of sculpture from about 700 B. C. to the end of the sixteenth century of our era.

Egyptian Room. — The two statues of the goddess Pascht, the fragments of a colossal figure and the throne on which it sat, the lid of a sarcophagus in basalt, and the capitals in sandstone, were procured in Egypt, in 1835, by the late Mr. John Lowell. To the left of the door on entering is a flat case containing "squeezes," or impressions on wet paper from Temple sculptures in Egypt, and at the end of the room, above the door, the cast of a bas-relief on the northern wall of the Temple of Karnak repre-

senting Seti I. (B. C. 1458) attacking a fortress in Palestine. The flat cases under the windows contain a great number of charms, amulets, scarabæi, etc.

The mummy cases in the centre of the room, and those set up against the wall to the right of the door on entering, are so arranged as to show the several cases in which each mummy was enclosed, with their lids. The show-cases set against the wall opposite the windows contain specimens of every sort of manufacture. Here are to be found examples of straw and bead work, linen robes, papyri, a few pieces of furniture, wooden painted slabs (stêles), Osirid figurines, alabaster head rests and jars, etc. In the flat cases below are specimens of glass work, Egyptian and Assyrian beads, a unique set of Arabic coins in glass of the tenth century, a green jasper scarab of great beauty, date about 1591 B.C., and a number of terra-cotta lamps, Greek, Roman, and Christian.

Collection of Casts. — The catalogue contains full particulars concerning all the casts in this and the succeeding rooms. Here nothing more is attempted than to point out a few of the most interesting.

The First Greek Room contains casts of Greek works of the seventh and sixth centuries B. C., of one pre-historic monument, the Gate of Lions at Mycenæ, and a few casts of Assyrian reliefs belonging to the eighth and seventh centuries B. C.

The Second Greek Room is called the Olympian room, on account of the casts from the western pediment of the Temple of Jupiter at Olympia, lately excavated by the German government. These works belong to the fifth century B.C., and are supposed to be by Pæonios, of Mende in Thrace. The same sculptor made the flying Victory which stood before the temple on a three-sided column. The middle of the room is occupied by a cast of the celebrated group of Hermes and the Infant Dionysos, by Praxiteles, which stood in the Heræum at Olympia. Around it are casts of several Praxitelean marbles at Rome.

The Third Greek Room contains casts from the marbles of the Parthenon pediments, the work of Phidias in the fifth century B. C. Here also are some of the Parthenon metopes, and a cast of the Potidæan inscription, the epitaph of the warriors who fell before Potidæa in 432 B. C.

The Fourth Greek Room is called the room of the Mausoleum from the casts of marbles belonging to that celebrated tomb which Artemisia raised in honor of her husband, Mausolus (d. 353 B. C.), at Halicarnassus. Here also are a number of stêles or grave-slabs, the finest of which are the Amazon from the Villa Albani, and the Dexileos stêle erected in memory of Dexileos, one of the five cavaliers who fell at Corinth early in the fourth century B. C.

The Fifth Greek Room, on the other side of the hall, contains casts of the Apollo Belvidere, the Mattei Amazon, the Ludovisi Mars, the Menander, the Apollo Musagetes with Clio and Thalia, and the Laocoön.

Sixth or Roman Room. — Here are casts of the bronze wolf at the Capitol, an Etruscan work of the third century B. C.; the Naples Agrippina, mother of Nero; and on the wall the great bas-relief on the inside of the Arch of Titus, representing the triumphal entry of that Emperor with the spoils of Jerusalem.

Architectural Room. — The greatest ornament of this room is a full-size cast of the very beautiful Pandroseum or Portico of the Caryatides on the Acropolis at Athens. The wall at the farther end is decorated with casts of twelve spandrels from the so-called Angel Choir of Lincoln Cathedral, and that on the side is covered with casts of Arabic and Persian ornamental work.

The Renaissance Room is filled with casts from Italian marbles of the fourteenth, fifteenth, and sixteenth centuries. The casts on the screen are from the marble Organ balustrade, made for the Cathedral at Florence by Luca della Robbia, between 1435 and 1455.

Beyond the Screen, in the other half of this room, is a very interesting collection of Greco-Etruscan vases; a number of Tanagra figurines; and the Cyprian antiquities, objects of terra-cotta and glass, statuettes, etc.

Second Floor.

The upper hall has a number of pictures hanging upon its walls. Among them are Allston's unfinished Belshazzar, and West's King Lear. At the farther end, near the window, is a cast of the second gate made by Lorenzo Ghiberti for the Baptistery at Florence, in the fifteenth century. The door to the left hand leads into

The Gallery of Tapestries. — Four of these tapestries, probably of Flemish manufacture, belonged to Louis Philippe. The carved and gilded oak panels between the tapestries are from the Hotel of the Constable Anne de Montmorency, at Paris. The cases contain a number of Italian, Japanese, and Chinese embroideries, many of which are of the utmost beauty.

The Loan Room, entered by the door at the end of the gallery, is rich in Chinese porcelain, Japanese lacquer, enamels, majolica, etc. *The Lawrence Room* is fitted up with carved wood-work of the time of Henry VIII. It opens into a room containing specimens of carving in wood, ancient and modern.

The Picture Gallery. — In this gallery the pictures are constantly changed, so that it is impossible to say what its contents may be from month to month.

The Allston Room contains a number of pictures by Allston, Copley, Trumbull, and Stuart. *The Water-Color Room*, next to it, constantly changes its contents. *The Gray Engraving Rooms*, opening out of this latter, contain the large and valuable collection of engravings bequeathed to Harvard College by the late Mr. Francis Gray. Among the late additions, the Monte Sancto di Dio is the most precious. Its three prints, engraved by Baccio Baldini, at Florence, in the fifteenth century, are among the earliest specimens of copper-plate engravings of the Italian school.

The Schools of Drawing and Painting, and of China-Painting and Wood-Carving, occupy rooms in the basement of the building, and a large studio in the third story.

The Curator's room and that appropriated to the Library are in the basement. Gen. Charles G. Loring is the Curator of the Museum.

The Libraries of Boston and Cambridge will be referred to at the close of the account of Cambridge institutions.

CAMBRIDGE.

In Cambridge, the interest centres in the University and its various departments. Some of these are situated in different parts of Boston, but, for the sake of unity, will be mentioned here. The College itself was founded in 1636 by the General Court of the Colony, which gave four hundred pounds "towards a school or college." Various departments for post-graduate study have gathered around it during the present century, until it can now be said to embrace foundations more or less ample for all the sciences and arts.

PEABODY MUSEUM OF AMERICAN ARCHÆOLOGY AND ETHNOLOGY.

The Museum was founded by George Peabody, in 1866. During the life time of its first Curator, Dr. Jeffries Wyman, its collections were lodged in Boylston Hall. The present building on Divinity Avenue, one fifth of the intended structure, erected in 1877, will form, when connected with the Zoölogical Museum, one wing of the building planned by the late Professor Agassiz.

By the instrument of trust, Mr. Peabody placed $150,000 in the hands of trustees, and provided that the income of $90,000 should be devoted to the care and increase of the Museum, and to the maintenance of a professorship, while the remaining sum of $60,000 should be invested and accumulated as a building fund until it amounted to at least $100,000, when a building could be erected on land furnished by the College free of cost or rental, the building when completed to become the property of the College. The building was constructed at a cost of $54,000, leaving the original building fund of $60,000 intact for new accumulation. The income that can be devoted to the care and increase of the Museum, including salaries, is only $4,500.

The nucleus of the collection, brought together by Dr. Wyman in November, 1868, consisted of about fifty objects, one half of which were received from the College, and the moiety from his own collection. Less than six years afterwards, at the time of his death, the entries in the catalogue were nearly 8,000. Since then the collections have increased at a still more rapid rate, and have more than doubled within the last four years. The entries now number about 24,000.

The new cases in the building have been made with especial care, and members of the Association interested in museum economy would do well to examine them. Only a portion of the rooms have yet received their cases, and the arrangement, and especially the proper labelling of the collections, are yet far from complete. Not more than half of the collections are displayed, even including those in the temporary cases.

The object of the arrangement is to invite and foster scientific research into the life, history, and connections of extinct tribes and nations, and of the more or less savage or barbarous peoples of to-day. To a large extent the method of treatment which this requires associates the perfect and the rude, to the loss of artistic effect. Thus, however, the story of the past is more truthfully told, and the arrangement is very suggestive. In this way only can an idea be gained of the relative perfection of different arts in a given tribe of past or present times; and the Museum, as a whole, becomes a history of comparative civilization.

In the special ethnological arrangement, objects of doubtful authenticity are rigorously excluded. The northern room on the *first floor* is devoted to North America, containing objects from the mounds and stone graves of the United States, and from the Southern and Western caves. In the gallery of this room are exhibited pottery and other articles from Central America, Mexico, and the West Indies. Here is also temporarily placed the small Egyptian collection. The gallery of the southern room, the floor of which is occupied as an office and work-room, will contain some typical Etruscan pottery, and collections from the Swiss lakes and other parts of Europe. In the hall on the *second floor* are the collections from the ancient and modern Pueblos. The southern room on this floor contains the Peruvian and Brazilian collections, with a few from other parts of South America. On the gallery of this room are the objects from the Pacific Islands, Eastern Asia, and Africa. The northern room, with its gallery, will have the collections from the Pacific coast of North America, and from the Indian tribes now existing, and the stone implements of the Atlantic and Central States. Here one will find the important series from New Jersey, which have attracted much attention. The collection of human crania and skeletons is on the *third floor*. F. W. Putnam is the Curator of the Museum.

MUSEUM OF COMPARATIVE ZOÖLOGY.

The Museum had for its nucleus the private collections of Professor L. Agassiz, by whom it was planned, and under whose direction it was begun. The present building was first occupied in 1859, the portion at that time completed having been eighty feet in length. There have been two additions made to it, in 1872 and 1878. When the part now (1880) in process of erection is finished, the whole structure will have a length of three hundred feet, and a breadth of sixty, with six available stories. The addition now being made forms the northwest corner, and a part of the front of the main structure; which, as originally planned, will have a length, on Oxford Street, of four hundred and ten feet. The cost of the building, not including the cases or other interior fittings, will have amounted, when the part now being added is finished, to about $225,000. The Museum has an invested fund of $484,000, yielding an income of about $26,000 a year. The care of this fund is intrusted to the Corporation of Harvard College; the management of the institution is in the hands of its own Faculty, five in number, among whom the President of

the University and the Curator only are *ex-officiis* members. There are fourteen Professors and Assistants attached to the Museum, at the present time; to them is intrusted the work of arranging and describing the material as received. There is an artist constantly employed, and also five young women, who assist in the care and arrangement of the collections. The Library contains about 13,500 volumes, and is rapidly increasing. There is a department of Geology connected with the Museum, with a separate invested fund of about $70,000, and a valuable special geological library.

The collections of the Museum are very extensive; but only a small portion of them are displayed to the public, by far the larger part being arranged with a view to as compact storage as is compatible with their being accessible for scientific use and consultation. The representation in the departments of ichthyology and entomology is especially complete and valuable; and the corals and echinoderms are believed to be more fully illustrated here than anywhere else in the world. The paleontological collections are also very extensive, exceeding in importance those of any museum in this country. The deep-sea Fauna is also especially rich. The publications of the Museum are issued in two series, one in quarto, and the other in octavo; of each of these, six complete volumes and parts of others have been issued. Upon the completion of the addition to the building now in process of erection, the corner piece will be chiefly occupied by lecture-rooms, the library, and laboratories: in the latter, there will be ample accommodations for undergraduates and special students in both the zoölogical and geological departments. In the lower story of the wing now completed, the paleontological collections will be arranged. About one third of this wing will be used for rooms which are to be open to the general public; the remaining two thirds will be devoted to the storage of the collections in such form as to be easily accessible for study to specialists in the various departments, and to rooms for the assistants.

Mr. Alexander Agassiz is the Curator of the Museum.

BOTANIC GARDEN AND HERBARIUM.

The Botanic Garden occupies seven acres of land at the corner of Linnean and Garden Streets, Cambridge. The fund for its establishment was raised by subscription in 1805, and the grounds were laid out in 1807. The income available for its support was for many years very small, and sometimes precarious; in 1864 it was considerably augmented by subscriptions, and a movement to increase this somewhat further, so that the Garden may be placed upon a sufficient and independent foundation, is now in progress.

William Dandridge Peck was in charge of the Garden from its establishment until his death, in 1822. Thomas Nuttall was Curator from 1822 to 1834. It was left mainly in charge of the original gardener, William Carter, until 1842, when Dr. Asa Gray became its director for the ensuing thirty years. It was subsequently directed by C. S. Sargent, in

connection with the Arnold Arboretum at the Bussey Institution; at the beginning of the current year its charge reverted to the Professor giving the instruction in Phænogamic Botany, George L. Goodale. Although Dr. Gray, in 1872, relinquished the care of the Garden, he still co-operates in every plan for the advancement of its interests.

The Garden below the terrace is laid out in plots which contain representatives of the natural orders of flowering plants and ferns. Wherever it is practicable the sequence of genera is that given in Bentham and Hooker's *Genera Plantarum*. Many of the plants not seen in their proper places in the orders will be found in the rockeries, frames, and houses where alone they can thrive.

The green-houses are in two ranges. Five face the main garden and are devoted to exotics of economic and botanical interest. The range behind these consists of two buildings for the propagation of plants and for the winter housing of succulents. Frames in the immediate vicinity of this range are filled with succulents, etc., and with the alpine plants of the White Mountains. The shrubbery near the lawn is mainly composed of Australian myrtles and acacias, beyond which is a group illustrating the various modes in which plants climb. The whole number of species cultivated in the Garden is not far from 6,000. William Falconer is head gardener. The Curator of the Herbarium is to have charge of the nomenclature of the plants in the garden.

In the year 1864, the building for the Herbarium was erected by Mr. Nathaniel Thayer, for the reception of the collections and library of Prof. Gray, then presented to the University. In 1871 Mr. Hunnewell built the annexed botanical laboratory and lecture-room, and a connecting portion of conservatory, since which the principal botanical instruction has been given at the Botanic Garden. Last winter a needed addition to the Herbarium was made, to contain the botanical library. The Herbarium and Library, under the general superintendence of Prof. Gray, are in the special charge of Mr. Sereno Watson, as Curator. These two botanists have here prosecuted a series of researches and studies upon North American Botany, the principal results of which are recorded in a large number of memoirs and detached articles, and in independent works. These could not have been produced, nor can any important contribution to our systematic phænogamous botany now be satisfactorily made, without the aid which this establishment affords. It is almost daily consulted by other botanists, or by persons seeking special information, either personally or by correspondence, and by the sending of specimens for comparison.

The Herbarium is thought to contain over 250,000 specimens (including the Bryological Herbarium bequeathed by the late Mr. Sullivant). Most of the known North American species are represented by a suite of from two to a dozen specimens, indicating and authenticating its varieties, geographical range, &c. About 5,500 specimens were incorporated into the Herbarium during the year 1879, and the additions for the present year are at nearly the same rate.

The Library contains about 3,500 bound volumes, and what would amount to a thousand volumes of pamphlets.

Material for a Botanical Museum is now rapidly accumulating, but there

is, as yet, no adequate space for its display. During the last few months very valuable accessions have been received from the Museum of the Royal Gardens, Kew. The contemplated Museum will illustrate as fully as possible the useful products of the vegetable kingdom.

The laboratory for instruction and research in the lower Cryptogamia is in the upper part of Boylston Hall, and is under the charge of W. G. Farlow, the Professor of Cryptogamic Botany. Dr. Farlow's extensive Herbarium of Algæ and Fungi is temporarily deposited in the same place.

The instruction of students at the Botanic Garden devolves upon Prof. Goodale, supplemented for a part of the year by Prof. Farlow in the Cryptogamia. An independent or extra-collegiate course, of lecture-room and laboratory instruction combined, specially intended for teachers, is maintained throughout the earlier half of the summer vacation.

BUSSEY INSTITUTION.

The Bussey Institution is a department of Harvard University, in Jamaica Plain, not far from the Forest Hills Station of the Boston and Providence Railroad. It is a School of Agriculture, Horticulture, and Veterinary Science, situated upon a farm of two or three hundred acres, a portion of which has been set apart for an Arboretum, in which shall be grown all kinds of trees, shrubs, and herbaceous plants which can support the climate of the locality. The principal school building, which is constructed of Roxbury pudding-stone, has considerable architectural merit, and is, perhaps, the handsomest building belonging to the University.

The Bussey Institution gives thorough and systematic instruction in Agriculture, Horticulture, Botany, Entomology, Agricultural Chemistry, and the Anatomy and Diseases of Domestic Animals. Seven teachers are connected with the school, and there is probably no college in the country where the courses of instruction upon agricultural science are so full or so well taught as here. In addition to their work at Jamaica Plain, some of the abler students find time to attend courses of instruction indirectly relating to agriculture, which are given in other departments of the University. As was to have been expected from the character of the instruction and the strict limitation of it to the branches provided for by the foundation, the number of students in attendance at the Bussey Institution has hitherto never been large, but the character or quality of the students has improved continually, and it is beginning to be understood by the community that this school offers peculiar and quite exceptional advantages to young men of studious habits and good abilities who wish to take the highest possible rank as agriculturists, horticulturists, or veterinary practitioners. There are now so many schools of agriculture in the country, and some of them are so strongly supported by State aid, and so loudly and persistently advertised by debates in the legislatures, that some time must necessarily elapse before the public can clearly determine which of the schools is really the most meritorious. It may be said of the Bussey Institution, that it has been established long enough to become

well organized and firmly rooted, that it is continually growing both in respect to the number and character of its students, and that development of this character gives promise of assured success in the future.

From 1874 to 1878 the Bussey Institution issued a "Bulletin," which contained the results of experiments and investigations made at the Institution, and it is greatly to be regretted that a lack of funds available for scientific research has made it necessary to suspend this publication for the time being.

ARNOLD ARBORETUM.

On the 29th of March, 1872, by an agreement made between the President and Fellows of Harvard College, and the trustees under the will of the late Mr. James Arnold, of New Bedford, the Arnold Arboretum was established.

Mr. Arnold's bequest for this purpose amounted to $100,000, the President and Fellows agreeing to devote to the new establishment one hundred and twenty-five acres of their Bussey estate in West Roxbury (now Boston).

This agreement provides that the Arnold Arboretum "shall contain, as far as practicable, all the trees, shrubs, and herbaceous plants, either indigenous or exotic, which can be raised in the open air in West Roxbury"; that these shall be properly and distinctly labelled, and that an Arnold Professor of Arboriculture shall be appointed to manage the Arboretum, and teach the knowledge of trees. The agreement forbade the spending of more than one third of the net income of the Arnold fund before it should have accumulated to $150,000.

Mr. C. S. Sargent was, in September, 1872, appointed Director of the new establishment, and the work of collecting and raising plants for the Arboretum was at once commenced, and a general system of correspondence and exchanges with other establishments of a similar character was inaugurated.

In March, 1879, the Arnold fund having accumulated to $150,000, the Corporation established the Arnold Professorship of Arboriculture, and Mr. Sargent was appointed to the chair.

A plan of the Arboretum made with reference to the possibility of including it in the general park system of the City of Boston, has been prepared by Mr. Fred. Law Olmsted, of New York. It is probable that such an arrangement, which would insure to the public all the advantages of freely visiting the Arboretum, without interfering with its scientific duties, will eventually be made.

The uncertainty in regard to this arrangement with the city has, however, delayed the permanent planting of the collections. These are still in nursery, and comprise more than a thousand species and varieties of hardy trees and shrubs. Thousands of plants and packages of seeds have been distributed from the Arboretum, and its influence, in increasing the study and planting of trees as an economic measure, is already widely felt.

It having been found necessary to establish a special Herbarium and Museum in connection with the Arboretum, such collections were commenced about a year ago. The Herbarium is now temporarily installed in Brookline, in charge of Mr. John Robinson.

The aims and purposes of the Arboretum, as at present organized, may be briefly characterized as follows: —

First. As a museum of living plants, in which every tree and shrub capable of withstanding the climate of Massachusetts is to find its appropriate place; this collection being supplemented by an herbarium, and various special collections, illustrative of trees, their products, and uses.

Second. As a scientific station for investigation into the characters, growth, economic and ornamental properties of trees; into the relations of forests to climate and the flow of rivers, and into the best methods of forest reproduction and management.

Third. As a school of forestry and arboriculture, in which special students may, when the demand for such instruction is felt, acquire the knowledge and training necessary to fit them for the care and increase of our forests.

Fourth. As a local educational establishment, capable, through "object teaching," of very considerable influence in increasing among the people of Boston and its neighborhood that knowledge which it is the duty of the Arboretum to disseminate.

ASTRONOMICAL OBSERVATORY.

The Astronomical Observatory of Harvard College was established in its present location in 1844, although observations (mainly magnetic and meteorological) had been carried on near the College buildings for the four previous years. The successive Directors of the Observatory have been Professors W. C. Bond (deceased 1859), G. P. Bond (1865), Joseph Winlock (1875), and E. C. Pickering, the present Director. The history and work of the institution are recorded in twelve quarto volumes of Annals. The equatorial telescope was mounted in 1847. At that time, its aperture of fifteen inches exceeded that of any refractor in existence except one of equal size. Among the objects studied by means of this instrument may be specially mentioned the planet Saturn, observed 1847–57, during which time the dark ring and the satellite Hyperion were discovered; the Great Comet of 1858, for his discussion of which Professor G. P. Bond received the gold medal of the Royal Astronomical Society of London; the Nebula of Orion, observed 1857–65; a series of interesting objects, representations of which were published, 1872–75, under the title, "Astronomical Engravings"; the satellites of Mars, 1877 and 1879; the brighter double stars, and most of the known satellites, observed photometrically, 1877–79. Observations are now in progress on the variations of the light of Jupiter's satellites at their eclipses, on the light and spectra of planetary nebulæ, and on the light of variable stars.

A meridian circle of the largest size was mounted in 1870, and has since been in constant use by Professor W. A. Rogers. Its principal work has

been the determination of the places of over eight thousand stars between 50° and 55° north of the equator. These observations were recently completed, after about eight years of continuous work.

A new instrument, called a meridian photometer, has been put into operation during the past year. It is intended for the measurement of the light of all stars visible to the naked eye in this latitude. About one hundred thousand observations will be needed for this purpose, and the work is going on at the rate of about four thousand a month.

Meteorological observations have been continued since the establishment of the Observatory. Numerous photographs of the sun were taken in the years 1870–76. Time signals have been distributed since 1872 over a large part of New England, and a time-ball is dropped daily in Boston by a clock at the Observatory.

The Observatory has been supported wholly by private munificence, except that it received aid from the College funds during its earlier years. The largest single gift made to it was a legacy of $100,000 from Mr. E. B. Phillips, received in 1849. A subscription of $5,000 a year for five years, raised in 1878, has recently nearly doubled for the time the income applicable to scientific purposes.

LAWRENCE SCIENTIFIC SCHOOL.

This School, founded by Mr. Abbott Lawrence, in 1847, offers complete courses of instruction in various departments of science, as follows: —

I. *Course of four years in Civil and Topographical Engineering.*

Students completing this course receive the degree of Civil Engineer.

II. *Course of four years in Chemistry.*

This course is intended for students preparing to become practical chemists, or teachers of science; and students completing it in a satisfactory manner receive the degree of Bachelor of Science.

III. *Courses of four years in Natural History.*

These courses are designed to furnish a special training in the departments of geology and biology.

The studies of the first two years are common to both departments. Students are required to designate, at the beginning of the third year, the department in which they intend to take their degree. During their term of study, they will be regarded as the special students of the Professors in that department.

Students who make good use of the opportunities afforded by the summer schools, or who, during the long vacation, pursue studies under the direction of the Professors in their departments, may shorten this course of study to three years.

Students honorably completing either of the above courses receive the degree of Bachelor of Science.

IV. *Course of four years in Mathematics, Physics, and Astronomy.*

The degree of Bachelor of Science is also open to students of this course.

In all the courses of the Scientific School, the successful study of any subject in College is taken as an equivalent for the same subject in the Scientific School.

Besides the regular courses above mentioned, the School also offers facilities to teachers, and to persons preparing to be teachers, who desire to qualify themselves in the modern methods of teaching science by observation and experiment. This course is flexible and comprehensive; the instruction is mainly given in the laboratories and museums of the University, and is of the most practical character, every student being taught to make experiments and study specimens.

Special Students.

Persons who are not candidates for a degree may enter the School as special students, at any time, without examination, and avail themselves of its advantages in whatever manner, and to whatever extent, they see fit. Such students, who have been members of the School for at least one year, shall be entitled to a certificate, specifying the subjects in which they have proved themselves proficient, either by examination or by laboratory work.

All students of the Scientific School may, if found competent, pursue any of the courses of instruction given in the other departments of the University, except exercises carried on in the special laboratories, without additional charge.

PHYSICAL DEPARTMENT.

The Physical Cabinet of Harvard University is in Harvard Hall. The instruments in this collection are mainly for lecture purposes. The large lecture-room of the Department of Physics is in the same building. The Physical Laboratory of the University occupies the ground floor of Lawrence Hall, and a small lecture-room having a southern exposure for light is on the second floor of the same building.

The Laboratory is used entirely for purposes of instruction, although a certain amount of original work is carried on each year. The Physical Cabinet of the Rumford Professorship of Light and Heat is also in Lawrence Hall, and contains a very complete collection of optical apparatus.

CHEMICAL DEPARTMENT.

The Chemical Laboratories in Boylston Hall comprise the laboratory for descriptive chemistry and qualitative analysis, that for quantitative analysis, and the organic laboratory; the first has ninety-eight desks, each of which can be used by two men at different hours; the number of

students in this laboratory varies from one hundred to one hundred and seventy. The laboratory for quantitative analysis has thirty desks, with fifteen to thirty students. This was the only laboratory in the original building, the French roof containing the large laboratory having been added less than ten years ago. The organic laboratory has twelve desks, with six to twelve students. There are also two lecture-rooms, an apparatus-room, and four private laboratories for the professors and assistants. The collection of chemical substances in the entry contains many rare elements and interesting chemical products. In addition to elementary instruction in all branches of chemistry, especial attention is paid to advanced work in organic and inorganic chemistry; in the latter, the researches of Professor Cooke and his students on antimony may be mentioned, while over twenty important papers on organic chemistry have been published since the establishment of the organic laboratory in 1875.

The mineralogical cabinet, one of the richest in the country, occupies two large halls; especially interesting are the collection of diamonds presented to the late Louis Agassiz by the Emperor of Brazil, the Tyrolese minerals from the collection of Count Liebner, and the finely crystallized specimens of American minerals. The elementary instruction in mineralogy is given by means of two full student-collections, one named, the other numbered, which are kept in the mineralogical work-room, where are also the lithological collection, and the desks for blowpipe analysis.

HARVARD MEDICAL SCHOOL.

The Harvard Medical School is separated from most of the other departments of the University in consequence of the necessity of securing facilities for clinical instruction, which can be obtained only in connection with the hospitals of a large city. The building occupied by the School is in North Grove Street, Boston, directly in the rear of the Massachusetts General Hospital. Here is to be seen the Warren Anatomical Museum, a collection illustrating normal, pathological, and comparative anatomy, the nucleus of which was formed by Dr. John C. Warren, and by him presented to Harvard College, together with a fund for its proper preservation. Since its foundation the Museum has grown steadily in value and importance by the gifts of various members of the profession, and particularly by the labors of its late Curator, Dr. J. B. S. Jackson. Among the most important recent additions may be mentioned the model museum of dermatology and syphilography presented by Dr. Edw. Wigglesworth. This is a collection of models of diseases of the skin, each model being made from a cast taken directly from the patient and subsequently colored at the bedside. Some of the most valuable and unique specimens belonging to the Warren Museum, including the skull and bar illustrating the celebrated "crow-bar case," are at present stored in the museum of the Medical Improvement Society, in Temple Place, for the sake of greater security against fire than the Warren Museum affords.

In the basement of the building in North Grove Street are to be found

the chemical laboratories and the dissecting room. The upper story is devoted to instruction and research in microscopy and physiology. The physiological laboratory contains an extensive collection of instruments used in physiological investigations.

HARVARD NATURAL HISTORY SOCIETY.

The Harvard Natural History Society, one of the older associations of the University, has members among the College instructors and graduates as well as among the students. It holds meetings twice a month for the reading of original papers and for discussion; during the past year its attention has been turned rather more to geology than to botany or zoölogy; chemistry and physics, included in the object, though not in the name of the Society, have received much less attention.

For the past three years, annual courses of free public lectures on scientific subjects have been given by Professors of the University and others, under the auspices of the Society. During the same time, prizes for essays on Natural History have been offered, with the design of encouraging habits of observation in scholars of High Schools and Academies.

Prof. N. S. Shaler is the President, and Mr. Frederic Gardiner, Jr., Secretary of the Society.

Besides these departments of the University, the following Cambridge institutions will have a certain interest to scientific men.

NUTTALL ORNITHOLOGICAL CLUB.

The Nuttall Ornithological Club was organized seven years ago by the younger ornithologists of this vicinity. In 1876 the publication of a quarterly bulletin was commenced, and was so well received, that it has since appeared with great regularity, the fifth volume being on the point of completion. It is now the recognized medium of ornithologists all over the country, few of whom are not enrolled members of the Club. The Club has no established head-quarters, but meets fortnightly at the house of one of its members.

Mr. William Brewster is President, and Mr. Henry A. Purdie, Secretary of the Club.

CAMBRIDGE ENTOMOLOGICAL CLUB.

This Club was formed early in 1874, and incorporated in 1877. It holds monthly meetings on Follen Street, at the office of its Secretary, which alternate with the meetings in Boston of the Entomological section of the Natural History Society. Very soon after its foundation it established a monthly journal, under the title of "Psyche," now in its third volume, in which especial attention is paid to the biological side of the science, and to the bibliography of American entomology.

Mr. Edward Burgess is the President of the Club, and Mr. B. P. Mann, the Secretary.

TELESCOPE MANUFACTORY OF MESSRS. ALVAN CLARK AND SONS.

The workshop of Messrs. Alvan Clark and Sons, is situated on the western bank of the Charles River, just below the Brookline Bridge. Public attention was first turned to their work in 1862 by the discovery of the companion to Sirius with the 18-inch object-glass (now mounted at the Chicago Observatory) which they had recently completed. The discovery of the Satellites of Mars, in 1877, with the 26-inch lens of the Washington Observatory, again excited general interest in their telescopes. A 30-inch lens for the Pulkova Observatory is now in process of construction by the Clarks; also, telescopes of 21 and 16 inches for Prof. Young and Mr. Swift. There is no attempt at show in the workshop, and a visitor would scarcely suspect the high quality of the work from the apparently rough methods employed. Their success arises mainly from the great ingenuity, mechanical skill, and manual dexterity of Mr. Clark and his two sons, who constitute the firm. Almost every Observatory in the country is indebted to them for some portion of its equipment. Besides telescopes, various other instruments requiring special skill in plan or construction, have been intrusted to them, including the apparatus for observing the transit of Venus in 1874, for measuring the velocity of light, and for the photometric observations of the Harvard Observatory.

THE LIBRARIES OF BOSTON AND CAMBRIDGE.

For the sake of convenience it seems best to speak in one place of the advantages to a scientific man which the libraries of this vicinity afford. Over half a million of books are accessible to a student. The Public Library of Boston, with somewhat over 200,000 volumes (including only the works in Bates Hall), and the College Library in Cambridge, with somewhat less than 200,000 volumes in the central department, are two of the three largest libraries in the country; both of these libraries are rich in scientific works. But besides these there are many special libraries, such as those of the departments at Cambridge and the societies in Boston, which are of still greater importance to the specialist; while the Boston Athenæum, a library of general literature, containing more than 100,000 volumes, is easy of access by introduction, and is rich in many fields of general science, and particularly in Geography.

The library of the Lawrence Scientific School (2,200 volumes) is devoted mainly to Engineering; that of the Bussey Institute (2,200 volumes), to Agriculture. The Phillips Library of the Astronomical Observatory consists of about 3,300 volumes; that of the Botanic Garden, of about 3,500 volumes, besides 1,000 volumes of pamphlets; the Peabody Museum of Archæology, of 500 volumes; and the Museum of Comparative Zoölogy, of about 13,500 volumes. The last mentioned is especially rich in Paleontology and Entomology, and is complemented by a separate and extensive geological library, not included in the above enumeration. In

Boston, the Natural History Society, and the American Academy, the former with about 20,000, the latter with about 16,000 volumes and pamphlets, are well supplied with the transactions of learned societies, the Academy leaning to those dealing with the physical, the Society to those with the natural sciences. The earlier volumes of the great academies of the world can generally be found at one of the three larger libraries of the neighborhood, and the larger part of the newer transactions are received in exchange by the Academy, the Natural History Society, the Zoölogical Museum, or the Observatory; while such as must be obtained by purchase will be generally sought successfully at the Boston Public or the Harvard Library. The purchasing powers of the smaller libraries are not great, and their accessions are to a great degree made up from their exchanges; but the larger libraries are liberal to science, and the conditions are constantly improving. In 1878, a classified list was published of the current serial publications taken in the nineteen principal libraries of the vicinity, which included over 2,000 publications, about half of them scientific.

Besides these the Institute of Technology has a library of about 3,000 volumes, the Horticultural Society has about the same number, not including its pamphlets, and the Agricultural Library at the State-House is tolerably large. The Harvard Medical School has a working library of 2,000 volumes, and our other medical libraries have been consolidated into one, now possessing about 10,000 volumes and 6,000 pamphlets. At the same time it should be mentioned that the medical department of the Public Library contains over 10,000 volumes.

Among special scientific libraries which have found their way to public institutions in this vicinity may be mentioned the Bowditch Mathematical, at the Boston Public Library; the Greene Botanical, Binney Conchological, Harris Entomological, and Bailey Microscopical, at the Natural History Society; the Jacobi Mathematical, at the Harvard College Library; the Gray Botanical, at the Botanic Garden; and at the Museum of Comparative Zoölogy, the DeKoninck Paleontological, the Whitney Geological, the Zoölogical and Paleontological Library of the late Professor Agassiz, and the Entomological Library of Dr. Hagen.

A bulletin of current accessions is issued by the University Library; and similar periodical publications come out at intervals, from the Boston Public Library and from the Boston Athenæum. All these libraries are thoroughly indexed under subjects in catalogues open to the inquirer.

SALEM.

PEABODY ACADEMY OF SCIENCE.

THIS institution was founded by George Peabody, who, on the 26th of February, 1867, placed in the hands of nine trustees the sum of $140,000, for the promotion of science and useful knowledge in the county of Essex.

He directed $40,000 to be applied to the purchase of the East India Marine Hall, in Salem, and other land adjoining thereto; the fitting up and furnishing the same for the proper arrangement of the Museum of the Salem East India Marine Society, and the scientific collections of the Essex Institute; conditional agreements having been previously made by the two institutions to place them therein on special permanent deposit.

The remaining sum of $100,000 is invested as a permanent fund, the income only of which can be used for the purposes of the trust.

The various alterations having been completed in August, 1869, Wednesday, the 18th of that month, the first day of the annual meeting of the American Association for the Advancement of Science, in Salem, was taken to dedicate the Museum to the uses for which it was designed by the founder.

The collection of the East India Marine Society, which is especially rich in the native implements of the Pacific Islands, and in specimens illustrating the costumes and habits of the inhabitants of various countries in the East Indies, and the Essex County collection of the Institute, are its marked features, and the many additions that have since been made contribute largely to its value and interest.

The Museum is open to the public every day (Sundays excepted), from 9 A. M. to noon, and from 1 to 5 P. M.

The Academy has printed six of its annual reports, and three memoirs. "The American Naturalist," Vols. II. to X., were also printed under its auspices.

A Summer School of Biology has been held for six weeks, commencing the first of July, for the past five years. This was commenced under the direction of Prof. A. S. Packard, Jr., continued by Mr. James H. Emerton, and is now in charge of Prof. E. S. Morse.

Hon. William C. Endicott is the President of the Academy; Mr. A. C. Goodell, Jr., the Secretary; and Prof. E. S. Morse, the Curator of the Museum.

ESSEX INSTITUTE.

This institution was formed by the union of the Essex Historical and the Essex County Natural History Societies, in February, 1848. The

former Society was incorporated in 1821, the latter in 1836. In February, 1870, an act was passed by the Legislature enlarging the powers of the Institute so as to include among its objects the promotion of the arts, science, and literature, in addition to those of the original societies. Its Library contains about 35,000 volumes bound, and about 120,000 pamphlets, exclusive of duplicates, and embracing all departments of literature.

Its Museum contains a large collection of antiquarian and historical relics, paintings, engravings, medals, coins, papers, currency, &c. The scientific portion is deposited in the Museum of the Peabody Academy of Science. At the time of its removal, the collection contained over 125,000 specimens in the departments of Ethnology, Zoölogy, Botany, Mineralogy, and Geology, scientifically arranged, and in good part labelled and catalogued.

Its Publications are the "Proceedings of the Essex Institute," 6 vols., 8vo.; "The Naturalist's Directory"; "The American Naturalist," Vol. I. (afterwards transferred to the Peabody Academy of Science); "Bulletin of the Essex Institute," 11 vols., 8vo.; "Historical Collections of the Essex Institute," 16 vols., 8vo. The Historical Collections and Bulletin are regularly issued.

Field meetings are held in different parts of the county for scientific and historical investigation and discussion. Evening meetings for the same purpose are regularly held during the winter at the rooms of the Institute. The Institute also gives courses of scientific and other lectures, and series of concerts during the winter months, and Horticultural and Art Exhibitions during the spring, summer, and autumn months.

Its Rooms are in Plummer Hall, Salem, so called in honor of Ernestus Augustus Plummer, whose sister, Miss Caroline Plummer, cherishing his memory, and directing that her gift should be recorded in his name, bequeathed to the proprietors of the Salem Athenæum the sum of $30,000, to be expended in the erection of a suitable building for scientific and literary purposes.

Dr. Henry Wheatland is the President of the Institute, and Mr. George M. Whipple, Secretary.

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